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Aid curse with Chinese characteristics? Chinese development flows and economic reforms

Samuel Brazys[1] · Krishna Chaitanya Vadlamannati[1]

Abstract
The emergence of China as a major development partner requires a reassessment of traditional donor–recipient dynamics. In addition to adopting new rhetoric like "South–South cooperation" or "Win–Win," China has eschewed classifications and practices of the traditional donors of the Organisation for Economic Co-operation and Development's Development Assistance Committee. Yet the "new approach" and willful ignorance may not spare China from encountering traditional development challenges. In this paper, we consider whether Chinese development efforts have disincentivized difficult economic reforms by providing recipient governments with alternative resources for building support. Using an instrumental variable approach with panel data covering 106 countries during the 2000–2014 period, we find that when comparing Chinese development flows to several Western donors, the former's flows inhibit broader economic reform. The findings are robust to alternative specifications, data, instruments, and approaches.

Keywords Development aid · Economic reforms · Endogeneity · China

JEL Classification P1 · F35 · C33 · C36 · O5

1 Introduction

In 2004, prompted by major Western donors, the Federated States of Micronesia (FSM), one of the most aid-dependent countries in the world, undertook an economic reform initiative to replace subnational sales taxes with a national value-added tax (VAT). The tax reform project was to be a keystone initiative in modernizing the FSM's governmental revenues to offset a built-in decrement in annual budget grants from the United States. The

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effort came tantalizingly close to fruition in 2013, when implementation legislation was passed at the national level and by two of the four constituent states. However, both Pohnpei and Yap failed to pass the necessary laws, and as of January 2020, the VAT reform remained in limbo.

Why did the VAT reform effort fail in the FSM? The prima facie explanation is that the reforms, while touted by international institutions as a means of mobilizing much-needed revenue (International Monetary Fund 2017), were politically costly, as they were opposed by vested business interests.[1] However, it undoubtedly was easier to avoid those political costs, because the mid-2000s also saw a gradual shifting in the sources of external budgetary assistance. While still dominated by the United States, the People's Republic of China increased the amounts and frequency of its assistance to the FSM.[2] The external funds were welcomed as an alternative by a political elite wary of economic dependence on the United States, and undermined the leverage of the United States and its partners to press for the passage of tax reform legislation.[3] As unconditional budget grants, the funds softened the public budget constraint (Kornai 1986), allowing politicians to continue to rely on public spending to maintain popular support.[4]

In this paper we investigate whether our implicit suggestion in the vignette above, that Chinese aid undermined a Western-backed economic reform by creating an institutional "aid curse," is observable as a more general phenomenon. The importance of the inquiry at hand stems from China taking its place amongst the largest development partners over the past 15 years (Dreher et al. 2020). That fact, coupled with a burgeoning literature on the characteristics (Bräutigam 2011), modalities (Schiere 2010; Dreher et al. 2020), and impacts (Ben Yishay et al. 2017; Isaksson and Kotsadam 2018; Dreher et al. 2019) of Chinese development efforts, stresses the importance of fully considering all aspects of how China is engaging the developing world. China explicitly distances itself from traditional donor–recipient dynamics (Woods 2008; Bräutigam 2011) and has been reluctant to engage with international institutions promoting economic development cooperation, transparency,

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[1] http://pacificpolicy.org/2013/05/fsm-tax-reform/, accessed 03-01-2018. The same domestic constituency was also broadly opposed to trade liberalization efforts (Brazys 2014).

[2] After making a commitment of US $ 4 million in 2008 (http://china.aiddata.org/projects/40039, accessed 03-01-2018), China disbursed US $1.5 million in 2011 (http://www.fsmpio.fm/RELEASES/2011/february/02_14_11.html, accessed 03-01-2018) before committing a further US $10 million, or roughly 4% of FSM's GDP in 2015 (http://www.guampdn.com/story/news/2015/12/03/fsm-seeks-end-compact-agreement-us/76755600/, accessed 03-01-2018).

[3] Ibid. While working for the FSM's chief executive, on numerous occasions this manuscript's author overheard senior policymakers, including the president, invoke China as an alternative to US support. The unconditional nature of Chinese budgetary grants was touted in contrast to the US funding, which is approved by an annual meeting of a Joint Economic Management Committee (JEMCO), established by the Compact of Free Association treaty between the US and FSM (Brazys 2014), comprising three US and two FSM members, making budgetary decisions by simple majority vote.

[4] Members of the FSM Congress, state legislatures, governors, and the president are allocated "representation funds" (see http://www.fsmcongress.fm/pdf%20documents/19th%20Congress/BILLS/CB%2019-32.pdf). While working for the FSM Executive, the present manuscript's author overheard several second-hand accounts of politicians' "representation funds" being utilized to buy rice, other consumables, or both for constituents. Those impressions are substantiated by various public auditor accounts that have found irregularities with respect to the funds (see http://www.kpress.info/index.php?option=com_content&view=article&id=531:pohnpei-files-criminal-charges-against-former-governor-john-ehsa&catid=8&Itemid=103 or http://www.fm/news/kp/2008/june08_3.htm). Representation funds are allocated from general funding, which would include tax revenues and unconditional budget support, such as the Chinese grants, but not conditional budget support like the US funding.

and effectiveness. However, even if Chinese development programs differ qualitatively from the Organisation for Co-operation and Economic Development's (OECD) Development Assistance Committee's (DAC) efforts, representing "dragon fruits" rather than traditional donors' "apples," the external flows still have the potential to affect the political economies of the host countries (Dreher et al. 2018).

While previous literature has documented the potential for an institutional aid curse (Svensson 1999; Knack 2001; Moss et al. 2006; Brazys 2016), China appears to be a particularly likely candidate, given its stated policies of noninterference and its indifference to governance or conditionality reforms (Hernandez 2017). While stunting local institutional reform may not be the aim of Chinese development flows, it nonetheless creates a negative externality that ultimately can work at cross-purposes to broader economic development. Our vignette is also suggestive of a further complication associated with China's rise as a development actor, namely its interactions with traditional donors. Early evidence suggests that Chinese development efforts may be undermining the aims of traditional donors (Brazys et al. 2017; Hernandez 2017; Zeitz 2020), even if that consequence is unintentional or indirect (Swedlund 2017). A Chinese aid curse that frustrates institutional reform would directly challenge the aims of many traditional donors who seek to promote good governance through aid conditionality (Molenaers et al. 2015).

In the following sections, we first develop theoretical underpinnings for an aid curse "with Chinese characteristics" based on the discretionary nature of much Chinese development assistance. We then test our theoretical expectations by drawing on a recently assembled global database of Chinese aid projects to explain changes in rankings of economic freedom around the world. Instrumenting for both, we compare the differential impact of Chinese and Western aid flows on those outcomes. We report evidence that China's development flows do indeed hinder economic reforms, while finding that, if anything, Western flows promote those reforms. We conclude with thoughts on the implications of our findings not only for Chinese development efforts, but also for China's role as a new global power.

2 Aid curse with Chinese characteristics

We suggest that Chinese aid may undermine economic policy reforms owing to its fundamental characteristics of "noninterference" and respect for state sovereignty (Alden 2005; Bräutigam 2011; Reilly 2012; Dreher et al. 2019). China has repeatedly and explicitly disavowed any desire for governmental reform in its development packages (Hernandez 2017). It has, of course, been shown that economic reforms impose political costs on recipient countries' leaders in the short run, and the absence of institutional conditionality makes Chinese aid attractive to leaders who fear that institutional reforms might undermine their domestic bases of support (Mohan and Power 2008; Swedlund 2017). Noninterference means, at a minimum, that Chinese aid is unlikely to proactively *contribute* to economic institutional reform.

However, Chinese development assistance also may actively *hinder* economic reform. First, any evidence that the political "aid curse" might be overstated (Altincekic and Bearce 2014) is predicated on the theoretical basis that aid is not as fungible, unconditional, or stable as a source of revenue as was assumed earlier (Djankov et al. 2008). Recent studies find that Chinese aid *is* fungible, unconditional, and stable (Kishi and Raleigh 2015; Strange et al. 2017; Cruzatti et al. 2020). Cash grants, or other forms of public budgetary

support, may enable governments to function without having to raise revenue from domestic sources. Taxation and tax reform are both politically costly, especially for "invisible" taxes, such as the value-added tax (VAT),[5] or the more visible income tax (Appel 2006). Yet, it is precisely such tax reforms that are often needed in developing countries to both widen and deepen the tax base, putting government finances on a sustainable footing (Keen and Ligthart 1999).

Moreover, when aid is restricted in disposition, it can promote rent seeking in a wide variety of government functions, including public employment, fuel or food subsidies, or constituent-targeted infrastructure projects (Tullock 1967; Svensson 2000; Economides et al. 2008; Ahmed 2012). Such socially wasteful activities can become especially pronounced in states that already have histories of rent-seeking norms (Pedersen 1997; Choi and Storr 2019). To the extent that Chinese aid comes as cash, it characteristically has "no strings attached" and is likely to fulfill the discretionary criteria that can induce dependency (Perlez 2006; Woods 2008, p. 1210; Bader 2015; Gonzalez-Vicente 2015; Hackenesch 2015).

However, previous work has also noted that China's development assistance in the form of cash budgetary grants is relatively infrequent (Brautigam 2009). Instead, Chinese development efforts tend to come through projects and in-kind contributions, often related to commercial endeavors, and tied to Chinese suppliers/providers (Brazys 2019; Dreher et al. 2018). While the aid flows are more restricted than budgetary grants, they may still be technically fungible if they finance projects that would have been implemented otherwise. They may provide sufficient geographic and/or political *discretion* in that the funds can be targeted to the core supporters of political elites. If a leader can direct sufficient patronage to her "selectorate," then she may not need to promote broader economic or revenue growth by introducing economic reforms (Bueno de Mesquita 2005; Bueno de Mesquita and Smith 2010; Ahmed 2012). Indeed, both Bader (2015) and Dreher et al. (2019) find that Chinese economic cooperation is used in that way. Patronage may well be preferable to engaging in economic reforms that might undermine the selectorate's political support (Biglaiser and DeRouen 2011). As such, the aid may entrench existing rent-seeking institutions and patronage networks and foster institutional dependence that inhibits economic reforms (Bueno de Mesquita et al. 2002). Dreher et al. (2019) argue convincingly that the noninterference and national sovereignty characteristics of Chinese aid suggest that, in the absence of some other (Chinese) economic or security motivations, the Chinese government is unlikely to be concerned about how projects are distributed within a given country. They note that Chinese projects are frequently "demand-driven" and describe a process of Chinese aid allocation that is ripe for sectoral and/or geographic discretion by host-country leaders, who often are themselves personally responsible for negotiating the assistance (Dreher et al. 2019).

Chinese development flows likewise may undermine institutions directly, because they are often associated with contemporaneous commercial projects, are explicitly commercial themselves, or both. China is an imperfect model of institutional reform (Wederman 2004), and indeed, China ranks poorly on Transparency International's "Exporting Corruption" index. Several recent studies have reported evidence that China's development efforts are associated with more local corruption (Brazys et al. 2017; Isaksson and Kotsadam 2018).

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[5] Invisible to consumers in that it is embedded in retail prices (Musgrave 1972).

The arguments above stand in stark contrast to several Western aid donors, who have long histories of explicitly linking aid to economic policy reform (Haggard and Webb 1993). While a debate about whether conditionality efforts have borne fruit is ongoing (Dreher and Gehring 2012; Dollar and Svensson 2000; Heckelman and Knack 2008; McGillivray 2009; Abbott et al. 2010; Nelson and Wallace 2016), unlike the Chinese case, at least a prima facia intent to enact economic policy reforms can be observed. While good intentions don't necessarily mean that Western aid did not undermine economic policy reform, because of an "aid curse" or "aid dependence" (Knack 2001), it does form the basis for thinking that Western aid and Chinese aid may have differential effects.[6]

Accordingly, we hypothesize that increases in Chinese development assistance will reduce the rate of economic institutional reforms, but that no such effect will be found for aid from Western donors. Our hypothesis may operate through one or more of the mechanisms described above. Leaders have no incentive to introduce costly and politically unpopular economic reforms if they are given access to discretionary resources sufficient to maintain their support in the short term.

3 Data and methods

3.1 Model specifications

To examine our theoretical propositions, we consider panel data covering 117 countries (see "Appendix 1" for a list of the countries) over the 2000–2014 (15-year) period, which coincides with China's rise as a major development partner. Since some of the data are not available for all countries for all years, our dataset is unbalanced. We thus estimate:

$$\Delta \, \text{EFR}_{it} \; = \; \phi_i \; + \; \beta \text{EFI}_{it-1} \; + \; \beta \ln(Aidpc)_{it-1} \; + \beta Z_{it} \; + \; \lambda_t \; + \; \upsilon_i \; + \omega_{it}, \qquad (1)$$

where $\Delta \, \text{EFR}_{it}$ is our outcome variable, which measures changes in economic reforms; ϕ is the intercept; $\ln(Aidpc)_{it-1}$ is our key explanatory variable of interest; Z_{it} are control variables; λ_t is a vector of year dummies; υ_i contains country-specific dummies; and ω_{it} is the error term.

Following de Soysa and Vadlamannati (2012) and others, we enter the yearly change in the Fraser Institute's Economic Freedom Rankings (EFR) for country i in year t as our dependent variable.[7] We control for policy convergence by entering EFR lagged 1 year, because countries already at high levels of freedom change much more slowly than those that are less free. The mean value of year-to-year change in EFR in our sample is 0.03, with a standard deviation of 0.19, suggesting significant variation in policy reforms across the sample countries; the maximum value is 1.34 and its minimum is −1.09. Further description of the EFR can be found in Table 1.

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[6] A substantial body of literature exists on the possibility of an (Western) institutional aid curse. In particular, numerous scholars have investigated the extent to which aid might undermine domestic tax and revenue efforts (see, e.g., Moss et al. 2006; Besley and Persson 2014). Furthermore, as helpfully observed by a referee, heterogeneity amongst (and even within) DAC donors is likely. That said, as DAC donors all adhere, at least in principle, to DAC rules, we believe it to be reasonable to consider them as a whole.

[7] Further details on the dataset can be found in de Soysa and Vadlamannati (2017, pp. 275–276). Also see https://www.fraserinstitute.org/economic-freedom/dataset?geozone=world&page=dataset.

Our main independent variable is Chinese development aid from AidData's (2017) Global Chinese Official Finance Dataset, version 1.0, developed by Dreher et al. (2018).[8] The dataset covers Chinese aid activities in 138 countries over the 2000–2014 period (Dreher et al. 2020). According to AidData, the total amount of aid and other state financing during the sample period amounted to $354.4 billion. The dataset has been broadly relied on by scholars to examine the causes and consequences of Chinese aid in Africa (e.g., Dreher et al. 2018; Isaksson and Kotsadam 2018; Brazys et al. 2017; Hernandez 2017; Strange et al. 2017; Zeitz 2020).[9]

We enter total Chinese development flows per capita (in logs), measured in constant US dollar prices, capturing both Overseas Development Assistance (ODA) and Other Official Flows (OOF) for 122 countries annually over the 2000–2014 period. The mean value of Chinese flows per capita is about $44, with a standard deviation of $385 and a maximum value of $14,360, suggesting very large variation in the sample.

We look at collective aid from all DAC donors. To measure DAC aid, we rely on *gross* aid disbursements per capita (log) measured in millions of US dollars at current prices[10] from a total of 23 DAC nations.[11]

We lag all our aid variables by 1 year because the effects are unlikely to be contemporaneous. Lagging by a year allows the aid variables to show up in economic reforms. Figure 1 captures the trend of Chinese development flows (logs of means) and yearly changes in EFR during the 2000–2014 period. The figure shows fewer economic reforms coinciding with more Chinese aid activities. Note that the change in EFR since 2007 has been dramatic, possibly attributable to the global financial crisis, which we control for in our model below.

It is likely that our key variables of interest—Chinese and Western development aid—are endogenous to economic policy reforms. It could be that economic policy reforms (or lack thereof) might influence aid allocations in the first place. That issue is not trivial, because those who argue that Chinese aid curtails economic reforms also make causal claims that Chinese development assistance seeks out countries that score poorly on economic reform indices.[12] Likewise, Western aid is clearly endogenous when Western donors include either a priori or post hoc reform conditionality in their aid packages (Collier et al. 1997).

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[8] See http://aiddata.org/data/chinese-global-official-finance-dataset.

[9] It is important to note, however, that the methodology and the resulting dataset have been subject to scholarly critique: some projects have been found to be in error. See http://www.chinaafricarealstory .com/2013/04/rubbery-numbers-on-chinese-aid.html, https://www.aiddata.org/blog/a-rejoinder-to-rubbery-numbers-on-chinese-aid, https://www.economist.com/china/2017/10/12/despite-its-reputation-chinese-aid-is-quite-effective (Accessed 28-06-2018).

[10] Note that DAC aid is measured in current US dollar prices, but the inclusion of year fixed effects should capture inflation.

[11] The countries are Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Japan, Luxembourg, the Netherlands, New Zealand, Norway, Portugal, Spain, South Korea, Sweden, Switzerland, the United Kingdom, and the United States. We enter gross disbursements for DAC aid because it is a more accurate measure of actual aid activity (commitments sometimes are cancelled or altered). We rely on gross commitments for China's aid because, unfortunately, those are the only amounts reported by AidData. Updating the results with China aid disbursements when that information becomes available would be a useful exercise.

[12] The empirical evidence, however, suggests a strong negative correlation between Chinese aid allocation and per capita income in recipient countries (Dreher et al. 2018).

Table 1 Components of the Fraser economic freedom index (EFR) *Source*: Gwartney and Lawson (2008), https://www.fraserinstitute.org/studies/economic-freedom

Area 1: Size of government: expenditures, taxes, and enterprises

    A. General government consumption spending as a percentage of total consumption

    B. Transfers and subsidies as a percentage of GDP

    C. Government enterprises and investment

    D. Top marginal tax rate

        (i) Top marginal income tax rate

        (ii) Top marginal income and payroll tax rates

Area 2: Legal structure and security of property rights

    A. Judicial independence (GCR)

    B. Impartial courts (GCR)

    C. Protection of property rights (GCR)

    D. Military interference in rule of law and the political process (CRG)

    E. Integrity of the legal system (CRG)

    F. Legal enforcement of contracts (DB)

    G. Regulatory restrictions on the sale of real property (DB)

Area 3: Access to sound money

    A. Money growth

    B. Standard deviation of inflation

    C. Inflation: most recent year

    D. Freedom to own foreign currency bank accounts

Area 4: Freedom to trade internationally

    A. Taxes on international trade

        (i) Revenues from trade taxes (% of trade sector)

        (ii) Mean tariff rate

        (iii) Standard deviation of tariff rates

    B. Regulatory trade barriers

        (i) Non-tariff trade barriers (GCR)

        (ii) Compliance cost of importing and exporting (DB)

    C. Size of the trade sector relative to expected

    D. Black-market exchange rates

    E. International capital market controls

        (i) Foreign ownership/investment restrictions (GCR)

        (ii) Capital controls

Area 5: Regulation of credit, labor, and business

    A. Credit market regulations

        (i) Ownership of banks

        (ii) Foreign bank competition

        (iii) Private sector credit

        (iv) Interest rate controls/negative real interest rates

    B. Labor market regulations

        (i) Minimum wage (DB)

        (ii) Hiring and firing regulations (GCR)

        (iii) Centralized collective bargaining (GCR)

        (iv) Mandated cost of hiring (DB)

        (v) Mandated cost of worker dismissal (DB)

Table 1 (continued)

        (vi) Conscription

    C. Business regulations

        (i) Price controls

        (ii) Administrative requirements (GCR)

        (iii) Bureaucracy costs (GCR)

        (iv) Starting a business (DB)

        (v) Extra payments/bribes (GCR)

        (vi) Licensing restrictions (DB)

        (vii) Cost of tax compliance (DB)
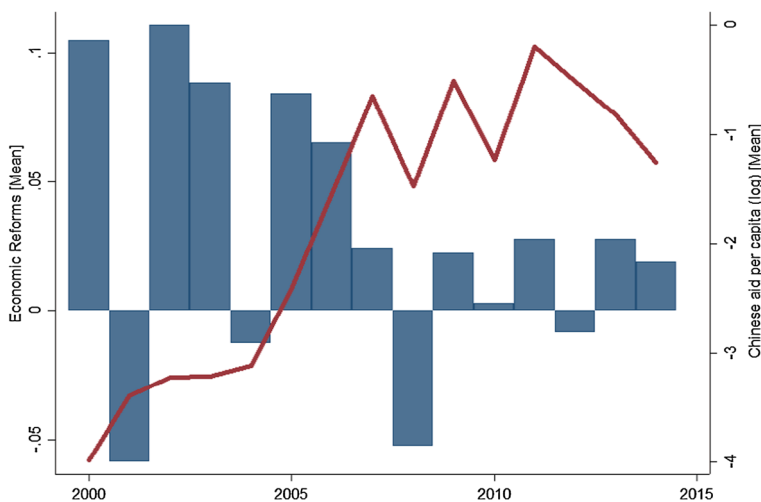
Fig. 1 Economic reforms and Chinese aid per capita (log) during the period 2000–2014

To address the endogeneity problem, we estimate a two-stage least-squares instrumental variables (2SLS-IV hereafter) regression. In order to instrument for Chinese aid, we follow Dreher et al. (2018, 2019) and compute the *probability* of a recipient country receiving Chinese aid, weighted by steel production (logged) in China, $iv = \left[\frac{1}{15} \sum_{y=1}^{15} p_{it} \times \ln(\text{steel})_t \right]$, which is lagged by 3 years.[13] While the steel production data are taken from the World Steel Association's (2017) statistical yearbook, the *probability* of receiving Chinese aid is the fraction of years during the 2000–2014 period during which such Chinese aid has been received. Thus, a country receiving aid from China for many years can be classified as belonging to the *high exposure group*, while those with a low probability of receiving aid are in the *low exposure group*. The interaction captures the extent to which Chinese aid allocated to a recipient country is driven by excess (above-average) steel production in China.

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[13] We lag the IV by 3 years in order to allow sufficient time (at least 2 years) for steel production's effects to materialize. Recall that our key explanatory variable is lagged by 1 year.

We believe that the interaction is exogenous because excess steel production is being used by China for infrastructure connectivity projects such as bridges, new trains and loco-motives, high-speed railroads, ports, highways, oil and gas pipelines, telecom, and electric-ity grids (Bräutigam 2011). As such, Dreher et al. (2018, p. 4) argue that

> the Chinese government considers steel to be a strategically important commodity and therefore maintains excess production capacity. This policy choice by the Chi-nese government results in a surplus of steel, some of which China uses for aid pro-jects around the world. In years when production volumes are high, China's supply of aid is also higher.

Furthermore, Huang (2016) notes that infrastructure development is a crucial element of China's recently unveiled flagship program, the Belt and Road Initiative (BRI). The BRI is a massive plan to build infrastructure projects that support major land- and sea-based economic corridors to link and develop the economies of Eurasia (Bluhm et al. 2018; Vad-lamannati et al. 2019). The projects, funded by Chinese aid money, will result in increased demand for products like steel (Vadlamannati et al. 2019). Thus, the identifying assump-tion is the same as in Dreher et al. (2018), namely that economic policy reforms will not be influenced differentially by changes in China's steel production in countries with high versus low probability of receiving Chinese aid. Our identifying assumption is also simi-lar to that of Dreher and Langlotz (2020), first adopted by Werker et al. (2009), in which a time-varying exogenous variable interacts with an endogenous variable that varies only across countries to create an instrument that then varies both across countries and over time. Thus, the excludability assumption is that the economic policy reforms for countries with differing levels of exposure to Chinese aid in the past will not be affected differently by changes in China's steel production, other than by its impact on Chinese development aid.

In order to construct our instrument for DAC aid, we follow Dreher and Langlotz (2020) and enter the *probability* of a recipient country receiving DAC aid, weighted by the *aver-age government fractionalization index* for 23 DAC countries, which varies across time—DAC$iv = \left[\frac{1}{15} \sum_{y=1}^{15} p_{it} \times (\text{gov\_frac})_t \right]$. As before, we lag the variable by 3 years. The *government fractionalization index,* sourced from the Database of Political Institutions developed by Beck et al. (2001), is coded on a scale of 0–1, in which a value closer to 1 denotes the probability that two cabinet members picked at random from the governing coalition will be affiliated with different parties.[14] On the other hand, a value of 0 denotes a single-party government. The intuition behind the instrument, as highlighted by Dreher and Langlotz (2020), is that budget deficits resulting from excessive government spending are more likely in countries with fragmented polities.

Two plausible ways can be identified through which greater political fragmenta-tion results in increases in government spending. First is the idea of *pork-barrel politics* (Keefer and Khemani 2009), which suggests that government targets public works: spend-ing narrowly in legislators' constituencies to build support for its broader policy agenda (Hallerberg et al. 2009). A second reason is legislators' influence on budgets. A govern-ment budget, according to Eslava and Nupia (2017), is a sum of policy demands of vari-ous party groupings in the legislature. Thus, the demands of different parties representing the respective interests of their constituents are brought into the budget-making process.

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[14] For the US government, the fractionalization index score is always zero. Hence, we replace it with legis-lature fractionalization.

Several studies in the literature have reported a positive relationship between legislative fragmentation and government expenditures (Volkerink and de Haan 2001; Roubini and Sachs 1989; Hallerberg and von Hagen 1999; Edin and Henry 1991). That empirical relationship is important for our purposes, because Alavuotunki and Sandström (2018), Brech and Potrafke (2014), and Round and Odedokun (2004) find that government spending, driven by ideological and political considerations, expands aid budgets.[15]

The validity of our instruments depends on instrument relevance and the exclusion criteria. The instrument relevance condition suggests that the instrument must be correlated with the explanatory variable in question. The joint *F*-statistic in the first stage of the IV regressions is examined to test the relevance of the instruments (Bound et al. 1995). A rule of thumb is that instruments are considered relevant when the joint *F*-statistics in the first-stage regression model exceed a threshold of 10 (Staiger and Stock 1997).

The second condition is that the selected instruments should not be systematically correlated with the error term in the second stage of the equation, i.e., $\left[\omega_{it} \mid IV_{it} \right] = 0$, meaning that the selected instruments should not have any direct effect on the outcome variable of interest (EFR), but their effects should instead be channeled only indirectly through the instrumented variable. The excludability of our instrument rests on the assumption that the economic policy reforms will not be differentially affected in countries with different propensities to receive aid, controlling for country and year-specific fixed effects. Moreover, the omitted variables would have to follow a time trend similar to year-specific steel production (log) and, moreover, affect our dependent variable *differently* in countries with *different* probabilities of receiving Chinese aid. The identifying assumption here is not affected by unobserved trends that correlate with both steel production in China and EFR. Although it is not a sufficient condition, the exclusion restriction will be violated if long-term trends differ across countries with different chances of Chinese aid (Christian and Barrett 2017). Following Dreher et al. (2018) and Stubbs et al. (2020), we plot steel production (log) in China over time, along with EFR by *high* and *low exposure groups,* to check whether the long-term EFR trends for the two groups are correlated with the long-term trend in Chinese steel production. Likewise, we plot average government fractionalization in DAC donor countries and the EFR by *high* and *low exposure groups*. The results, discussed in Sect. 4, suggest no apparent nonlinear trend between steel production (log), government fractionalization, and economic policy reforms in *high* and *low exposure groups*.

We first estimate our regression using ordinary least squares (OLS) with year and country-specific fixed effects in Table 2 column 1, before turning to a parsimonious 2SLS-IV model in column 3.[16] We cluster standard errors over countries. As a robustness check, we enter a number of control variables, Z_{it}, in columns 2 and 4 obtained from Pitlik (2007) and other prominent studies on the determinants of economic freedom (Drazen and Easterly 2001; Potrafke 2013; Bjørnskov and Potrafke 2012).[17] Most of those variables arguably are

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[15] The finding is corroborated by Faini (2006) and Beenstock (1980), who report that the size of the donor country's aid budget is a function of its fiscal condition.

[16] One obvious problem we encounter is that in a short panel of 15 years that includes fixed effects and a lagged EPR level might cause inconsistent estimations resulting in a downward bias of the coefficient, known as the "Nickell bias" (Nickell 1981). We therefore rely on a system-generalized method of moments (SGMM) estimator to counter that problem.

[17] Our controls include the GDP growth rate, the Polity IV regime type, Laeven and Valencia's (2008) economic crisis dummy measure, which captures systemic banking, currency, and debt crises, IMF program participation as in Boockmann and Dreher (2003), a dummy assigning the value of 1 for a left-wing government in power and 0 otherwise sourced from Beck et al. (2001), and a measure of natural resource rents as a share of GDP. Full justification for the controls can be found in the Online Appendix.

Table 2 Influence of Chinese aid on economic reforms: 2SLS-IV estimations

	(1)	(2)	(3)	(4)
Economic freedom index $t-1$	−0.323***	−0.330***	−0.391***	−0.392***
	(0.0316)	(0.0308)	(0.0308)	(0.0302)
Chinese aid per capita (log) $t-1$	−0.00391**	−0.00353**	−0.0308***	−0.0326***
	(0.00170)	(0.00172)	(0.0117)	(0.0113)
DAC aid per capita (log) $t-1$	0.00209	−0.00316	0.0428	0.0380
	(0.0119)	(0.0119)	(0.0758)	(0.0704)
Control variables	*No*	*Yes*	*No*	*Yes*
Estimator	OLS-FE	OLS-FE	2SLS-IV	2SLS-IV
Country fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
First-stage F-statistics of Chinese aid IV			10.01***	9.83***
First-stage F-statistics of DAC aid IV			9.50***	10.56***
Number of observations	1122	1102	1049	1031
Number of countries	92	90	106	102
First-stage model				
Steel production (log) × probability of receiving Chinese aid			5.865**	5.842**
			(2.575)	(2.595)
Government fractionalization × probability of receiving DAC aid			1.614***	1.445***
			(0.469)	(0.476)
Control variables			*No*	*Yes*
Country fixed effects			Yes	Yes
Year fixed effects			Yes	Yes
Number of observations			1049	1031
Number of countries			106	102

Standard errors in parentheses

Statistical significance: ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$

endogenous and introduce bias even if our aid measures are instrumented with excludable instrumental variables. We follow two approaches to address that concern. First, we estimate both OLS and 2SLS-IV models without including any control variables, except for a lagged EFR level. It is noteworthy that our exclusion restriction holds in the absence of control variables, so that their exclusion does not bias our estimates. Second, we also run models by including the control variables, but lagging them all by 1 year to mitigate the reverse causation problem.[18] The descriptive statistics are provided in "Appendix 2", and the data sources and definitions are presented in "Appendix 3".

4 Empirical results

Table 2 presents the main results. Our results broadly support our hypothesis, as shown in Table 2. In column 1, which is a parsimonious model, we find a negative effect of Chinese development aid per capita (log) on economic policy reforms that is significantly different from zero at the 1% level. The substantive effects suggest that a standard deviation increase in Chinese aid per capita (log) is associated with a 0.02-point reduction in the EFR, which is about 51% of the mean of economic policy reforms. Furthermore, moving from the mean to maximum value of Chinese aid per capita (log) in the sample is associated with a 0.04-point decline in economic policy reforms. That is an economically meaningful amount considering that the mean annual economic reforms measure in our sample is 0.03. Note that the results are from a parsimonious model in which we control only for lagged EFR and DAC aid per capita. On the other hand, we do not find any significant effect of DAC aid on economic policy reforms.[19] Our results remain robust to controlling for other independent variables (not shown here) in column 2.

Next, we turn to our preferred identification strategy of instrumenting for Chinese and DAC aid flows. The results from the IV estimations hinge on the assumption that the identification strategy applied is fully valid. In order to examine the validity of our identification strategy, we present the first-stage regression results from predicting Chinese and DAC aid allocations in columns 3 and 4 at the bottom of Table 2. As seen there, we find a positive effect of the instrumental variables on the allocation of Chinese aid, suggesting that more countries participate in Chinese aid programs in the past when steel production in China is high. Similarly, for DAC aid, our first-stage regression estimation suggests that variation in aid flows from DAC donors is driven by government fractionalization in donor countries. Figure 2 plots the magnitudes of the interaction effect from the first panel's model.

To calculate the marginal effect of Chinese and DAC aid probabilities, respectively, we consider the conditioning variables (namely, logged steel production and governmental fractionalization) and display aid's total marginal effects conditional on steel production and government fractionalization. The y-axis of the left-hand panel of Fig. 2 displays the marginal effect of aggregate Chinese aid allocations; the marginal effect is evaluated on the steel production variable on the x-axis. The right-hand panel of Fig. 2 displays the marginal effect of aggregate DAC aid allocations.

As seen, and in line with our theoretical expectations, the probability of receiving Chinese aid in the past increases aid flows from China when steel production in China

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[18] The results are reported in the Online Appendix.

[19] This "null" effect (rather than a positive effect) may well be the result of the heterogeneity of DAC donors as noted in footnote 6 above.

increases. The results are supported by previous studies (Dreher et al. 2016; Vadlamannati et al. 2019). Similarly, we find that the probability of receiving DAC aid increases when governments are more fragmented, a result in line with the findings of Alavuotunki and Sandström (2018). Note that the additional statistics on instrument relevance, namely the *joint F-statistics* from the first-stage regressions, reject the null that the instruments selected are not relevant. The *F*-statistics in both columns 3 and 4 surpass conventional levels of weak identification tests, such as Staiger and Stock's (1997) threshold of 10 as well as Stock and Yogo's (2005) conservative critical value of 7.56, tolerating a maximum 2SLS-IV size distortion of 10%.

With respect to the excludability of our instrumental variables, we examine the non-linear trends in the economic policy reforms (our dependent variable) in countries with high and low exposure to Chinese and DAC aid conditional on the exogenous variation in steel production and government fractionalization during our study period shown in Fig. 3. The upper left quadrant in Fig. 3 shows the temporal evolution of steel production (log) in China, while the upper right quadrant captures the economic reforms across recipient states with high and low exposure to Chinese aid in the past. As shown, no nonlinear trend is evident between steel production in China and economic reforms in high exposure states.

Similarly, the bottom left quadrant in Fig. 3 shows the temporal evolution of the average government fractionalization index of DAC countries; the bottom right quadrant captures the economic reforms across states with high and low exposure to aggregate past DAC aid. Once again, no distinct nonlinear trend is seen between government fractionalization in the DAC countries and economic reforms in high exposure states.

Overall, three key findings can be drawn from the IV results shown in Table 2. First, our results on Chinese aid flows remain robust even after correcting for endogeneity concerns. Moreover, the substantial effects of Chinese aid flows have increased many folds relative to the corresponding OLS estimations. For instance, a standard deviation increase in Chinese aid per capita (log) is associated with a 0.16-point reduction in the EFR, which is eight times a large as the OLS estimation and about 85% of one standard deviation in economic policy reforms. The findings suggest that the OLS models might be underestimating the effects of Chinese aid flows. Second, even after correcting for endogeneity concerns, DAC aid remains statistically insignificant. Finally, the additional statistics and tests reported in Table 2 speak to the strength of our instruments, suggesting powerful and excludable instruments that have accounted for reverse causality and other sources of endogeneity.

5 Extensions and robustness checks

5.1 Stand-alone and disaggregated Chinese aid

We interrogate our main findings in several ways. First, we examine the Chinese aid finding on its own more closely. While our tests above considered *all* Chinese development flows, our arguments also imply that the likelihood of aid undermining reform rests on the degree of *discretion* in allocating aid. Flows that political leaders can direct to individuals, sectors, or locations of their choice are more likely to provide an alternative basis of political support, thereby obviating the need for broader economic reforms that may be painful in the short term. Importantly, the project-level data allows us to proxy discretion in two separate ways. First, the data distinguish between "ODA-like" and "OOF-like" (Other Official Flows) flows (Dreher et al. 2018; Strange et al. 2017). ODA-like flows typically encompass
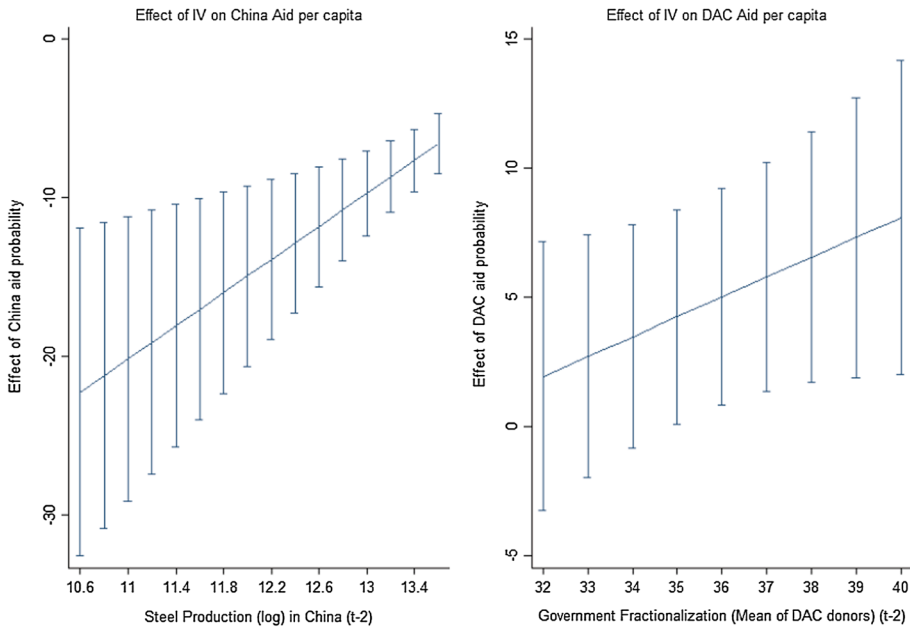
Fig. 2 Visualized effect of the instrumental variables

the budgetary and in-kind project grants discussed above that are concessional to the same degree as the DAC's ODA definition. Conversely, OOF-like flows capture projects of a more commercial rather than developmental nature, are not highly concessional, or are not directed to ODA-eligible countries.[20] OOF flows are thus likely to have less *discretion* than the former because they often accompany specific Chinese commercial interests and may be tied to some nondiscretionary location, such as the site of a natural resource (Isaksson and Kotsadam 2018). That said, Dreher et al. (2019) find that ODA-like flows are, if anything, less likely to be directed to a leader's region of birth than a measure including all aid flows, suggesting that all flows may be subject to such capture.

Unfortunately, we could not find convincing *different* instruments for each of the possible aid sub-flows. As such, we estimate separate models for Chinese ODA and OOF using the same steel instrument discussed above. Doing so of course limits our ability to identify partial effects but may give us some indication about whether any qualitative difference can be observed in the effects of ODA and OOF flows on economic reform. The results are presented in Table 3 below.

We estimate models of Chinese aid in column 1 (parsimonious) and column 2 (controls) before disaggregating the data in columns 1–3. Interestingly, we find a strong negative effect of ODA-like flows on economic reforms that is significantly different from zero at the 1% level. However, our results using the OOF-like variable are statistically nonsignificant. While again noting that the estimates are not partial effects, the differences in the results are suggestive of a qualitative difference between Chinese

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[20] See the AidData glossary for a more elaborate definition (https://www.aiddata.org/pages/tuff-glossary, accessed 26-05-2019).
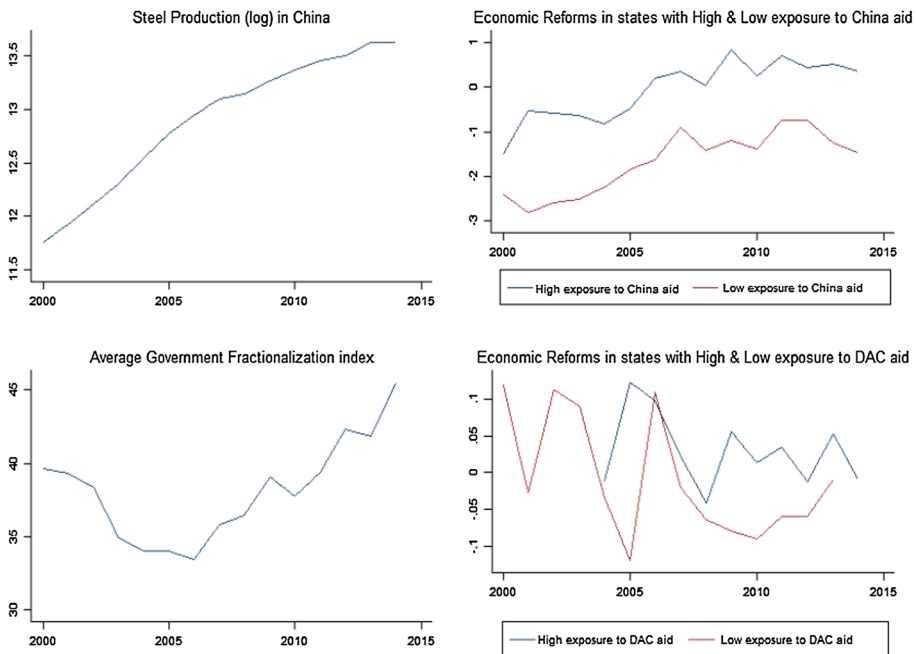
Fig. 3 Parallel trends in economic reforms

ODA and OOF aid flows, a finding that is consistent with Brazys et al. (2017). The results once again hinge on the assumption that the identification strategy applied is fully valid. We first present the first-stage regression results from predicting Chinese aid allocations at the bottom of Table 3. As seen in columns 1–3, with the exception of OOF-like flows, we find a positive effect of the instrumental variables on allocations of Chinese aid and ODA-like flows, suggesting that more countries were likely to participate in Chinese aid programs in the past when steel production in China was high. In the Online Appendix we provide further tests, including the visualized effect of the instruments (in Fig. 4 in Online Appendix) and an examination of parallel trends (in Fig. 5 in Online Appendix).

Second, we take advantage of the ability to proxy for project discretion by calculating the share of projects that are classified as "Social Infrastructure and Services" (i.e., projects with Creditor Reporting System (CRS) codes from 100 to 199). The mean ratio of discretionary projects in our sample for a country-year is 0.42, with a standard deviation of 0.35. Using that measure, we introduce an interaction term between Chinese aid per capita to examine whether Chinese development flows inhibit reforms when the ratio of discretionary projects is large. As before, we enter the independent variables from outcome Eq. (1) and control for both year and country fixed effects. We find that the conditional effect on economic reforms of Chinese aid and the discretionary aid share is negative. The results are presented in Table 4 in the Online Appendix, along with a graphical representation of the interaction effects using conditional plots in Fig. 6 in the Online Appendix.

Table 3 Influence of disaggregated Chinese aid on economic reforms

	(1)	(2)	(3)	(4)
Economic freedom index $t-1$	−0.358*** (0.0302)	−0.358*** (0.0300)	−0.305*** (0.0541)	−0.332*** (0.0510)
Chinese ODA per capita (log) $t-1$	−0.0379*** (0.0141)	−0.0353** (0.0141)		
Chinese OOF per capita (log) $t-1$			−0.000537 (0.0184)	−0.00638 (0.0178)
Control variables	*No*	*Yes*	*No*	*Yes*
Estimator	2SLS-IV	2SLS-IV	2SLS-IV	2SLS-IV
Country fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
First-stage *F*-statistics of Chinese ODA IV	10.17***	9.64***		
First-stage *F-statistics* of Chinese OOF IV			3.88**	4.06**
Number of observations	92	92	106	106
Number of countries	755	752	722	696
First-stage model				
Steel production (log) × probability of receiving Chinese ODA	6.412** (2.934)	6.389** (2.994)		
Steel production (log) × probability of receiving Chinese OOF			1.691** (5.590)	3.753 (5.702)
Control variables	*No*	*Yes*	*No*	*Yes*
Country fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Number of observations	92	92	106	106
Number of countries	755	752	722	696

Standard errors in parentheses

Statistical significance: ***$p < 0.01$, **p < 0.05, *p < 0.1

5.2 Robustness checks

We examine the robustness of our findings in several ways. First, we report our baseline estimation results in Table 2 with all control variables. Our results reported in Table 5 in the Online Appendix show that the findings are robust to controlling for other variables. Furthermore, we enter additional control variables to the models in Table 2, such as labor strikes, anti-government protests, number of cabinet changes (Campos et al. 2010), the Herfindahl–Hirschman index of government fractionalization (e.g., Bjørnskov 2016; Potrafke 2013; Campos et al. 2010; Alesina et al. 2006; Pitlik and Writh 2003), GDP level (log), and an economic sanctions dummy, which could influence both aid allocations and economic policy reforms. Inclusion of the additional variables does not change the findings (Table 6, Online Appendix). Next, we test whether the 2SLS-IV estimations are sensitive to different lag structures of our instruments. We lag Chinese and DAC aid per capita measures by 2 and 3 years and the instruments by 3 and 4 years, respectively. The results (in Table 7, Online Appendix) remain robust to using alternative lag structures. Following Vadlamannati (2020) and Vadlamannati et al. (2020), we exclude the observations with extreme values of the dependent variable. However, a histogram of the dependent variable (Fig. 7, Online Appendix) suggests that the variable is normally distributed, with a few extreme

observations. Our results without outliers, reported in Table 8 in the Online Appendix, are qualitatively unchanged, suggesting that the results are not driven by extreme values. Next, we use an alternative method of operationalization for Chinese aid and DAC aid. We use aid flows measured in millions of US dollars (log) at current prices. Note that we add 1 to observations with zero values before taking the natural log. Our results in Table 9 in the Online Appendix hold when we use these measures. Finally, to address the "Nickell bias" (Nickell 1981) problem, we estimate a system-GMM estimator as suggested by Arellano and Bond (1991), where we instrument for lag dependent variable. Our results (reported in Table 10, Online Appendix) on the Chinese aid measure remains negative and significantly different from zero at the 1% level. Moreover, the Hansen J statistic shows that the null hypothesis of instrument exogeneity cannot be rejected at conventional levels of significance. Furthermore, the Arellano–Bond test of second-order autocorrelation suggests that the estimator is consistent. All the tables reporting the results of our robustness checks are available on request.

6 Conclusions and discussion

The findings in this paper suggest that despite being a "new" development partner, Chinese development aid flows may be accompanied by negative externalities, something that has dogged 60 years' experience with development assistance from OECD donors. In particular, increased Chinese development efforts undermine the impetus for the economic reforms that can ultimately free counties from the yoke of dependence on external flows. No such finding is present when looking at flows from Development Assistance Committee (DAC) donor partners.

Interestingly, however, the empirical results may be driven only by Chinese assistance that is sufficiently discretionary. When China gives recipients a freer hand over their development flows, leaders can use the resources as a substitute for improved economic performance in building and maintaining their political support. The fact that China appears to be (officially) indifferent to such externalities makes it an institutional aid curse "with Chinese characteristics" and a significant challenge to overcome. Additional externalities may stem from Chinese aid dependence, allowing developing countries to skirt or shirk reforms demanded by traditional donor conditionality, inducing those donors to loosen the conditions under which their aid is given, or both (Zeitz 2020; Hernandez 2017).

It remains unclear whether China's principles of "noninterference" and "sovereignty" are more than just rhetorical devices. China may be willing to keep "hands off" with respect to aid flows if its broader investments are secure. If and when countries struggle to meet their obligations to China, post hoc conditionality may appear. Several incidents already suggest that Chinese "noninterference" may be only skin deep. Recently, Sri Lanka, finding it hard to service development loans from China, signed over a major port on a 99-year lease.[21] Similarly, a senior Australian official raised concerns about small island states in the Pacific, including the Federated States of Micronesia (FSM), racking up large debts to China.[22] Indeed, when Tonga pressed China to transform a $60 million (15% of GDP) loan

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[21] https://www.nytimes.com/2017/12/12/world/asia/sri-lanka-china-port.html, accessed 08-02-2018.

[22] http://www.scmp.com/news/china/diplomacy-defence/article/2127626/china-funding-white-elephant-infrastructure-projects, accessed 08-02-2018.

into a grant in 2013, concerns were raised that China would use that leverage to establish a naval base in the country.[23]

More broadly, our findings add further support to the literature suggesting that China is acting as a revisionist power as it takes its mantle as a leader in global international affairs (Brazys and Dukalskis 2017). While economic growth may flourish in the short term, China's apparent disregard for promoting norms of good economic governance (not to mention other civil, political, and human rights) brings China into conflict with Western donors, even if the latter are not responding directly to an increased Chinese presence in the development space (Humphrey and Michaelowa 2019). Indeed, resentment towards China has already bubbled to the surface in several developing countries where it operates (Buckley 2013; Wang and Elliot 2014). As much as China may want to pursue a new approach to development partnerships, it may find that, as with their OECD counterparts, long-term development success is no easy task.

Appendix 1: List of countries

Afghanistan	Czech Republic	Latvia	Sao Tome and Principe
Albania	Denmark	Lebanon	Saudi Arabia
Algeria	Djibouti	Lesotho	Senegal
Angola	Dominican Republic	Liberia	Serbia
Antigua and Barbuda	Ecuador	Libya	Seychelles
Argentina	Egypt	Lithuania	Sierra Leone
Armenia	El Salvador	Macedonia	Singapore
Australia	Equatorial Guinea	Madagascar	Slovakia
Austria	Eritrea	Malawi	Slovenia
Azerbaijan	Estonia	Malaysia	Solomon Islands
Bahrain	Ethiopia	Maldives	South Africa
Bangladesh	Fiji	Mali	South Korea
Barbados	Finland	Mauritania	Spain
Belarus	France	Mauritius	Sri Lanka
Belgium	Gabon	Mexico	Sudan
Belize	Gambia	Moldova	Suriname
Benin	Georgia	Mongolia	Swaziland
Bhutan	Germany	Morocco	Sweden
Bolivia	Ghana	Mozambique	Switzerland
Bosnia-Herzegovina	Greece	Myanmar	Syria

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[23] http://www.pireport.org/articles/2013/12/20/chinese-loan-puts-tonga-difficult-position-%E2%80%98aki lisi-pohiva, accessed 08-02-2018.

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Botswana	Grenada	Namibia	Taiwan
Brazil	Guatemala	Nepal	Tajikistan
Brunei	Guinea	Netherlands	Tanzania
Bulgaria	Guinea-Bissau	New Zealand	Thailand
Burkina Faso	Guyana	Nicaragua	Togo
Burundi	Haiti	Niger	Tonga
Cambodia	Honduras	Nigeria	Trinidad and Tobago
Cameroon	Hungary	North Korea	Tunisia
Canada	India	Norway	Turkey
Cape Verde	Indonesia	Oman	Turkmenistan
Central African Republic	Iran	Pakistan	Uganda
Chad	Iraq	Palestinian Adm. Areas	Ukraine
Chile	Ireland	Panama	United Arab Emirates
China	Israel	Papua New Guinea	United Kingdom
Colombia	Italy	Paraguay	United States of America
Comoros	Jamaica	Peru	Uruguay
Congo, Democratic Republic	Japan	Philippines	Uzbekistan
Congo, Republic	Jordan	Poland	Vanuatu
Costa Rica	Kazakhstan	Portugal	Venezuela
Cote d'Ivoire	Kenya	Qatar	Vietnam
Croatia	Kuwait	Romania	Yemen
Cuba	Kyrgyz Republic	Russia	Zambia
Cyprus	Laos	Rwanda	Zimbabwe

Appendix 2: Descriptive statistics

Variables	Mean	SD	Minimum	Maximum	Observations
Change in Economic freedom index	0.03	0.19	−1.09	1.34	1982
Economic freedom index $t-1$	6.71	0.92	2.93	8.86	1864
Chinese aid per capita	45.34	389.15	0.00	14,361	1793
Chinese aid per capita (log)	−1.73	4.50	−6.91	9.57	1793
Chinese ODA per capita (log)	−2.83	4.17	−6.91	8.50	1560
Chinese OOF per capita (log)	−4.31	4.29	−6.91	8.69	1113
Share of discretionary projects	42.28	35.12	0.00	100.00	1263
GDP growth rate	8.00	1.58	4.78	11.12	2580
Polity democracy index	4.22	5.83	−82.48	104.48	2580
Economic crises	3.61	6.43	−10.00	10.00	2375
Natural resource rents/GDP	11.50	16.48	−1.19	100.37	2580
IMF program	0.03	0.17	0.00	1.00	2576
Left governments	0.09	0.28	0.00	1.00	2579

Appendix 3: Data sources and definitions

Variables	Data definition and sources
EFR	EFR is made up of five sub-indices capturing: expenditure and tax reforms; property rights and legal reforms; trade reforms; reforms related to access to sound money; labor, business and credit reforms. These five sub-indices are made up of 35 components of objective indicators. The final index is ranked on a scale of 0 (not free) to 10 (totally free) and is sourced from the Fraser Institute (available at: https://www.fraserinstitute.org/economic-freedom/dataset?geozone=world&page=dataset)
Change in EFR	Year-to-year change in EFR sourced from the Fraser Institute
Chinese aid per capita	Aid flows including ODA and OOF-type flows measured in US$ constant prices (logged) and sourced from the AidData's Global Chinese Official Finance Dataset, version 1.0 (AidData 2017) developed by Dreher et al. (2018)
Chinese ODA per capita	ODA flows measured in US$ constant prices (logged), sourced from the Aid-Data's Global Chinese Official Finance Dataset, version 1.0 (AidData 2017) developed by Dreher et al. (2018)
Chinese grants per capita	Grants flows measured in US$ constant prices (logged) and sourced from the AidData's Global Chinese Official Finance Dataset, version 1.0 (AidData 2017) developed by Dreher et al. (2018)
Chinese aid projects	Count of all aid (ODA and OOF) projects in country i and year t (logged) based on the information sourced from AidData's Global Chinese Official Finance Dataset, version 1.0 (AidData 2017) developed by Dreher et al. (2018)
Per capita GDP (log)	GDP per head in 2000 US$ constant prices, sourced from the World Development Indicators (WDI) 2017, World Bank
Polity democracy	Polity IV, polity 2 index coded on a scale of -10 to $+10$, where the highest value implies full democracy lagged by a year, sourced from Gurr and Jaggers (1995)
Economic crises	Dummy takes the value 1 if a country is exposed to either currency crisis, banking crisis, debt crisis (or all together) lagged by a year, sourced from Laeven and Valencia (2008)
GDP growth rate	Rate of growth of GDP, sourced from the WDI, World Bank 2017
Natural resource rents/GDP	Total rents from natural resources as a share of GDP, sourced from the World Bank dataset on resource rents, 2017
IMF program	Dummy takes the value 1 if a country is in an IMF program for more than 5 months during the year, and 0 otherwise, obtained from Dreher (2006)

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