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The Effects of Chinese Aid on State Legitimacy in Africa: Cross-National and Sub-National Evidence from Surveys, Survey Experiments, and Behavioral Games

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Abstract

What are the effects of Chinese aid on the legitimacy of African states? China has dramatically increased the scope of its aid to Africa in recent years, igniting much debate among scholars and policymakers, some of whom characterize China as a "rogue donor" that is rupturing the social contract between citizens and recipient states. We test this proposition by combining within-country analysis of original surveys, survey experiments, and behavioral games in Liberia with cross-country analysis of AidData and Afrobarometer data on 38 African countries. We find little evidence indicating that exposure to Chinese aid has diminished Africans' perceptions of government or reduced tax compliance or morale, both important indicators of state legitimacy. Our results are consistent across settings, levels of analysis, and measurement and identification strategies, belying the conventional wisdom that China's increasing economic engagement imperils good governance or estranges citizens from recipient African states.

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1 Introduction

What are the effects of foreign aid on the legitimacy of recipient states? Does aid rupture the social contract between citizens and governments, as critics claim, or perhaps reinforce it, as advocates hope? Existing studies tend towards pessimism. A large and established cross-national literature finds that aid has pernicious effects on the behavior of government officials and the performance of government institutions (Brautigam and Knack 2004; Knack 2004; Moyo 2010; Svensson 2000). Sub-nationally as well, aid has been shown to exacerbate citizens' perceptions of state corruption and incompetence (Brass 2016), disincentivize accountability (Martin 2016), facilitate favoritism in the distribution of public goods (Jablonski 2014), and aggravate distrust in state institutions that prioritize (or appear to prioritize) donor preferences over citizen needs (Brautigam 1992).

Given the trillions of dollars in aid delivered to developing countries since World War II, understanding its impact on the relationship between citizens and recipient states has long been a matter of significant practical and theoretical concern. But the question has become especially urgent, and has provoked especially rancorous debate, with the dramatic expansion of Chinese aid to Africa in recent years.[1] The pace of growth has been staggering. At a Forum on China-Africa Cooperation (FOCAC) in 2000, the Chinese inaugurated a "strategic partnership" with 44 African governments, and pledges to the continent have doubled at each FOCAC summit since (Strange et al. 2013).

This expansion has proven extremely controversial. Critics argue that China is a "rogue" donor whose appetite for natural resources and reluctance to impose good governance conditions will "underwrite a world that is more corrupt, chaotic, and authoritarian" (Naím 2007)–a perspective echoed at the highest levels of the US government (Brautigam 2015; Smith 2012). Advocates, including some African heads of state, counter that Chinese projects are implemented quicker and at lower cost than their Western counterparts (Wade 2008). Critics respond that China prioritizes speed and low cost at the expense of quality and fair play.

Until recently, empirical evidence was scarce on both sides of this debate.[2] This is now beginning to change, with a host of new studies stimulated by the publication of AidData's pathbreaking *Chinese Official Finance to Africa Dataset* (Dreher et al. 2016; Strange et al. 2017).[3] Recent studies suggest that the effects of Chinese aid are more complex than media and policy (and sometimes scholarly) narratives suggest. On the one hand, some have found that Chinese aid stimulates economic growth (Dreher et al. 2017), and that China is no more likely than Western donors to support corrupt or autocratic regimes (Dreher et al. 2018). On the other, Chinese aid has been shown to increase both the perception (Brazys, Elkink and Kelly 2017) and reality (Isaksson and Kotsadam 2018*a*) of corruption in recipient countries,

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[1]For concision we use the word "aid" throughout this paper, though there is some ambiguity in how Chinese aid should be defined. Some Chinese-funded projects fit the conventional definition of Official Development Assistance (ODA)–they are granted with concessional terms and are intended to promote economic or social development–but others do not. In many cases it is impossible to tell the difference. We discuss the implications of this ambiguity for our analysis in Section 3.3.

[2]A notable exception is the pioneering work of Deborah Brautigam beginning in the 1990s. Her contributions are many, but see especially Brautigam (1998; 2009).

[3]See http://china.aiddata.org/.

while undermining collective bargaining by workers (Isaksson and Kotsadam 2018*b*) and facilitating "political capture" by elites (Dreher et al. 2016).

These studies have significantly advanced our understanding of the complex and sometimes contradictory effects of Chinese aid to Africa. But like the literature on foreign aid more generally, they are limited in several important ways. Most rely on one of two identification strategies, either selection on observables in the context of observational studies or random assignment to vignettes in the context of survey experiments. The former are prone to selection and omitted variables bias, while the latter may not generalize to the real world. Most also rely on surveys to operationalize the relationship between citizens and recipient states, but these measures are susceptible to social desirability bias and non-random refusal to respond. Finally, most studies are conducted either exclusively across countries (e.g. cross-national regressions) or exclusively within them (e.g. survey experiments). The former capture general trends but not country-specific patterns, while the latter capture country-specific patterns but not general trends.

We address these limitations by combining multiple sources of data and multiple measurement and identification strategies at multiple levels of analysis to assess the effects of Chinese aid on state legitimacy in Africa. Our research design integrates cross-national analysis of Afrobarometer and AidData data with sub-national analysis of original surveys, survey experiments, and behavioral games in Liberia, one of the world's most aid-dependent states. Each of these approaches complements and compensates for the limitations of the others. Our goal is to triangulate between them. While we explore multiple indicators of state legitimacy, we focus in particular on tax compliance and morale, arguably the most important and widely-used proxies for state legitimacy in the social sciences (Fain 1972; Gilley 2009; Levi, Sacks and Tyler 2009; Tyler 2006).

Contrary to our expectations (and our pre-specified hypotheses),[4] we find little evidence to suggest that Chinese aid has diminished the legitimacy of African states. In Liberia, we find that randomly-assigned vignettes about foreign provision of public goods have either null or *positive* effects on tax compliance and morale as measured through surveys and behavioral games. We similarly find that Liberians who are most affected by aid typically have more rather than less favorable perceptions of the Liberian government. Cross-nationally, we show that Chinese-funded projects tend to be sited in locations where trust in government is low. After correcting for this selection effect, however, the net impact on citizens' attitudes is generally null. These results are consistent across levels of analysis and measurement and identification strategies, and, as we discuss in Section 5, are unlikely to be artifacts of measurement error, selection bias, or lack of statistical power. Together they belie the notion that Chinese aid is provoking backlash among those most affected by it, or disrupting the social contract between citizens and recipient African states. We conclude by considering the implications of our results for the study and practice of aid in the future.

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[4]Our pre-analysis plan (PAP) was registered with the Evidence in Governance and Politics (EGAP) network prior to data collection. The link has been blinded for review, but is available upon request. The PAP refers specifically to our behavioral games in Liberia, but our surveys and survey experiments are similar in approach, and our hypotheses are the same regardless.

2 Theory

2.1 Foreign Aid and State Legitimacy

Scholars have long debated the effects of foreign aid on recipient states. Most have focused on the macro level, using cross-country comparisons to estimate the impact of aid on the behavior of government officials and the quality of government institutions. These studies have yielded mixed results, though in general, pessimism has prevailed. While some have found evidence of benign or even beneficial effects (Jones and Tarp 2016; Tavares 2003), most studies suggest that aid exacerbates corruption (Svensson 2000), diminishes bureaucratic quality (Brautigam and Knack 2004), undermines rule of law (Knack 2001), perpetuates authoritarianism (Ahmed 2012), and has at best no effect on democracy (Knack 2004). Aid may also disincentivize "tax effort" (Remmer 2004), fostering a "culture of dependency" in recipient states (Moyo 2010).

Our study extends a small but growing literature on the effects of aid on state legitimacy at the micro level (Baldwin and Winters 2018; Cruz and Schneider 2017; Dietrich and Winters 2015; Dietrich, Mahmud and Winters 2017; Guiteras and Mobarak 2014; Martin 2016; Sacks 2012). We conceptualize legitimacy as a function of citizens' relationships with state authority, rather than the "objective" characteristics of states themselves (e.g. the extent to which they promote human rights). Legitimacy is the belief that the state has a "right to rule" (Gilley 2009). That belief, in turn, manifests in a "disposition to obey" state authority (Weber 1994, 313). Legitimacy thus has two components: a "value-based" component–defined as citizens' "sense of obligation or willingness to obey authorities"–and a behavioral one, defined as "actual compliance with governmental regulations and laws" (Levi, Sacks and Tyler 2009, 356). We test the effects of aid on both of these components, focusing in particular on tax compliance and morale, which are widely viewed as crucial indicators of behavioral and attitudinal legitimacy, respectively.[5] Indeed, as Fain (1972, 20) argues, in many respects the "general problem of legitimacy" is best understood as a problem "exhibited in the authority of the tax collector."

The macro- and micro-level effects of aid are connected through the "virtuous circle" of legitimacy and public goods provision that lies at the heart of social contract theories of the state.[6] Citizens are more likely to comply with government restrictions (laws) and extractions (taxes) when they believe government will provide public goods in return, and will do so in a way that is transparent and responsive to citizens' policy priorities (Levi, Sacks and Tyler 2009). The more citizens perceive the state as legitimate, the more willing they are to pay taxes; the more taxes citizens pay, the more public goods the state can provide; and the more public goods the state provides, the more legitimate citizens perceive it to be. Moreover, the more taxes citizens pay, the more motivated they become to hold government accountable for the taxes it collects (Timmons 2005; Weigel 2017), thus reducing waste, improving responsiveness, and further increasing legitimacy.

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[5]Tax compliance is the extent to which citizens pay taxes. Tax morale is the extent to which they pay taxes *voluntarily*, because they believe they have a duty to do so (Luttmer and Singhal 2014).

[6]This virtuous circle is also related to the "fiscal contract," whereby the state commits to providing public goods using taxes that citizens commit to pay (Timmons 2005).

Most existing studies suggest that aid ruptures this virtuous circle. Citizens who learn about foreign funding of public goods may deduce that government is too corrupt or inept to provide them on its own (Brass 2016); that it prioritizes the preferences of foreign donors over citizens (Brautigam 1992); or that, thanks to the aid it receives, it is sufficiently flush that it does not need tax dollars in the first place. Knowledge of aid may also weaken citizens' incentives to hold government accountable (Martin 2016). Citizens who learn about biases or inefficiencies in the distribution of aid may blame government for failing to regulate donors, or may suspect government officials of engaging in favoritism themselves (Jablonski 2014). And citizens who learn about or experience abusive hiring or management practices by foreign contractors may blame government for allowing those practices to persist.

But as a number of recent micro-level studies suggest, these delegitimizing effects are not a foregone conclusion. Attribution errors may cause citizens to mis-identify who is supplying public goods in the first place, allowing governments to "claim credit" for benefits they did not provide (Cruz and Schneider 2017; Guiteras and Mobarak 2014). Even citizens who know about the external origins of public goods may nonetheless credit government for attracting foreign largess (Dietrich, Mahmud and Winters 2017). Moreover, foreign contractors often pay more generous wages than local firms (Gupta 2012); while this may distort local economies, it may simultaneously improve citizens' perceptions of government, which they may credit with bringing good, high-paying jobs to the country. Even citizens who disapprove of aid dependence may respond by paying more rather than fewer taxes, with the hope of reducing dependence in the future (Sacks 2012).

2.2   Chinese Aid to Africa–Virtuous Circle or Vicious Vycle?

Concerns about the delegitimizing impact of aid have become especially acute with China's emergence as a major donor and investor in Africa. Many believe Chinese aid is different from its Western counterparts, and that it is especially likely to have pernicious effects, for at least four reasons. First, critics have accused China of assisting African countries only to exploit their natural resources (Naím 2007).[7] If citizens share this view, they may conclude that any services China provides are intended not to improve their welfare, but rather to facilitate natural resource extraction. Citizens may then blame government for allowing this "new imperialism" to emerge (Okeowo 2013).

Second, while most Western donors maintain a large portfolio of (relatively) small-scale projects across a variety of sectors, China tends to favor fewer, larger-scale projects focused on infrastructure in particular: roads, electricity, schools, hospitals, etc. These are precisely the sorts of projects usually financed through tax revenues; citizens who learn about China's role in funding them may conclude that government is too incompetent to do so on its own. The scale and visibility of these projects may also limit government's ability to claim credit for them.

Third, while many donors attach "good governance" conditions to the aid they provide, China typically does not (Hernandez 2017), viewing conditionalities as a threat to state sovereignty (Brautigam 2009).

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[7]Thus far, systematic analyses do not support this perception. Patterns of Chinese aid seem to resemble those of other bilateral donors (Dreher et al. 2018).

There is also some evidence that China is willing to support corrupt or authoritarian regimes that Western aid agencies typically prefer to avoid (Cheung et al. 2012; c.f. Dreher and Fuchs 2015). In these settings in particular, the absence of conditionalities may exacerbate corruption and predation (Brazys, Elkink and Kelly 2017; Isaksson and Kotsadam 2018*a*), and may facilitate bias in the distribution of foreign funds (Dreher et al. 2016).

Finally, while many donors hire local contractors to implement their projects, China often relies on Chinese contractors instead (Brautigam 2009). This may aggravate concerns about "local ownership," and may complicate credit claiming, which typically depends on some degree of opaqueness in the source of funding for public goods (Guiteras and Mobarak 2014). Some Chinese contractors have also been accused of predatory hiring and management practices, potentially fomenting grievances not just against Chinese contractors themselves, but also against host governments for failing to regulate them in the first place.

2.3   Reasons for Optimism?

In our pre-analysis plan we hypothesized that Chinese aid would diminish the legitimacy of recipient states for the reasons described above. But each of these points is contested both empirically and theoretically, and some of the purported problems with Chinese aid may not be problematic at all from citizens' perspectives–for example, if they value large-scale infrastructure projects over smaller-scale alternatives, or if they object to good governance conditions as paternalistic.[8]

More fundamentally, contradictions in the literature on aid may reflect contradictions in the *reality* of aid, which may have complex effects on citizens who recognize both its costs and benefits. Offsetting effects may occur both within and across individuals. Citizens may credit government with attracting foreign funds, but still object to foreign influence over domestic spending; may blame government for misallocating aid dollars, but still appreciate the above-market wages that foreign contractors pay; may welcome the public goods that donors provide, but still worry about dependence; etc. Some citizens may disproportionately experience the harmful implications of aid, others the benefits. If these positive and negative effects counterbalance each other, the result will be, at worst, a net null.

3   Research Design

To test these propositions, we combine within-country analysis of original surveys, survey experiments, and behavioral games in Liberia with cross-country analysis of data from AidData and the Afrobarometer survey covering 38 African states.

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[8]In fact, evidence from Afrobarometer surveys suggests that citizens generally have favorable views of Chinese aid (Lekorwe et al. 2016).

3.1   Within-country Research Design

Our within-country research design focuses on Liberia, a small West African nation still recovering from 14 years of brutal civil war, and, more recently, the devastating Ebola crisis of 2014. Tax evasion has long been pervasive in Liberia, and the government has always been "desperately short of money" (Ellis 2006, 207). Underlying this problem is the nearly ubiquitous perception that government institutions are corrupt and inept—a perception that exacerbated, and was exacerbated by, the Ebola epidemic (Blair, Morse and Tsai 2017).

While the government is now trying to instill a culture of tax compliance among citizens, formidable obstacles remain. Liberia is one of the world's least developed and most aid-dependent countries. It boasts just several hundred miles of paved roads, an electric grid that barely powers the capital city, and a police force of 4,000 officers for a population of over four million. It placed 177th out of 188 countries on the 2016 UN Human Development Index,[9] and, in 2015, was ranked by the OECD as the second most aid-dependent country in the world, behind only the island nation of Tuvalu (OECD 2015).

Liberia is in many ways a "most likely" case for detecting the adverse effects of Chinese aid on state legitimacy. Anecdotally, China's rapidly expanding presence has stoked grievances and catalyzed civil unrest not just against Chinese contractors, but against the Liberian government itself. Liberian users of Chinese-funded projects complain of their low quality (Sieh 2014); Liberian employees of Chinese contractors complain of "modern slavery," appealing to the government to "urgently intervene before the situation turns worse" (The New Dawn 2013). As a damning 2014 report by the Sustainable Development Institute observed, the Liberian government is widely believed to be "more concerned with facilitating a smooth operating environment" for Chinese firms than with ensuring the welfare of Liberian citizens (Mukpo 2014, 8).

3.1.1   Behavioral Games

Our within-country research design begins with a modified version of the tax compliance game, a staple of behavioral economics (Alm, Jackson and McKee 1992). In the conventional setup, each participant receives an initial endowment—their "income"—and decides how much of that endowment to report. Reported income is taxed at a constant rate; unreported income is not taxed, but is subject to the possibility of an audit. Audited participants pay a fine on unreported income. Participants decide how much to report by maximizing their expected utility over four parameters: the size of their initial endowment, the tax rate, the probability of being audited, and the size of the fine. (We describe the logic of the game in further detail in the appendix.)

We implemented a modified version of the game in August 2015 with a convenience sample of 740 residents of Gbarnga, Bong County, the fourth largest city in Liberia. While participants were not randomly selected, Liberian facilitators were instructed to recruit from all neighborhoods in the city, and to sample both men and women, youths and elders, and members of both majority and minority ethnic groups.

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[9]See http://hdr.undp.org/en/content/human-development-index-hdi.

Prior to recruitment, facilitators administered pre-screening questions to test for literacy and numeracy. While this reduced the representativeness of the sample—as we show in the appendix, our participants tended to be younger and more educated than the average Gbarnga resident, and were more likely to be male—it was necessary to ensure comprehension of the game.

The game was implemented over 11 days at the offices of Parley Liberia, a local NGO. At the time, Parley was a new and relatively unknown organization; importantly, it had no affiliation with the Liberian government, and received no support from Chinese donors. Upon arrival at the study site, participants were given a unique ID number and randomly assigned to one of four rooms, each of which corresponded to a different treatment group, with eight participants and two facilitators per room. Participants were repeatedly reassured of their anonymity, and, to guarantee privacy, were separated from each other and the facilitators by tall wooden dividers.

Facilitators administered a short survey before the game began. They then explained the rules, providing examples and conducting comprehension checks throughout. In each round, participants were given an opaque manila envelope containing between 0 and 200 fake Liberian dollars (LD), redeemable for real LD at the end of the game. (200 LD is roughly equivalent to $2.75 USD, the daily wage of the average Gbarnga resident.) They were also given a template for anonymously reporting their income, which they were instructed to complete and return to the Liberian facilitators in each round. Reported income was taxed at a flat rate of 25%. The game was played over ten rounds, with two additional rounds of practice at the beginning to reinforce comprehension. Participants were not told in advance how many rounds they would play.

Importantly, participants were informed at the outset that all taxes paid would be delivered to the Liberian government. This should have encouraged them to view their decisions as consequential, and to interpret the game as directly connected to the real-world dynamics of taxation. (As we discuss in the appendix, focus groups conducted after the game strongly suggest that participants made this connection.) After deciding how much income to report, participants took turns drawing colored beans from a bag. Those who drew a black bean were audited; those who drew a white bean were not. The drawing was held in private behind the tall wooden dividers, and participants were instructed not to share the results with others in the room. The probability of being audited was announced beforehand, and was held constant at 10%. For ease of comprehension, audited unreported income was fined at a flat rate of 100 LD.[10]

After the two practice rounds, participants were read one of three vignettes about public goods provision in Liberia, focusing either on the Liberian government or on Chinese or American donors. We chose to use Americans donors as a benchmark because of Liberia's deep historical ties to the US—Liberia was founded by freeborn African-Americans and emancipated slaves—and because of Liberians' generally positive perceptions of American donors. Indeed, as we show in the appendix, participants expressed much more favorable views of the US than of China along every dimension we measured. We expected this comparison to starkly illuminate any potential negative effects of Chinese aid.

All vignettes emphasized that (1) government uses Liberian tax dollars to provide public goods, while

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[10]Ideally the size of the fine would have been proportional to the degree of under-reporting, but in pre-testing this calculation proved too challenging for some of our participants.

foreign donors do not; (2) government (or Chinese or American donors) plays a vital role in public goods provision; but (3) government (or Chinese or American donors) may nonetheless mismanage funds or mistreat Liberian workers in the process. Following the discussion in Section 2 above, the vignettes addressed both the positive and negative aspects of government- and donor-provided public goods. This "bundled" treatment more accurately captures the conflicting signals that citizens receive in the real world, and allowed us to avoid generating ill will towards donors or the Liberian government as a result of treatment—an important ethical consideration in and of itself. We discuss this issue in further detail in Section 5. A control group played the game without hearing a vignette. Balance tests and the text of the vignettes are included in the appendix.

After implementation was complete, facilitators conducted focus groups in which participants were asked six open-ended questions about their decision-making process during the game. These discussions were recorded, transcribed, and coded, allowing us to assess the internal validity of the experiment and document the ways that participants connected their experiences in the lab to real-world dynamics of tax compliance and evasion. Questions, coding rules, and excerpts from the focus groups are provided in the appendix as well.

3.1.2   Surveys and Survey Experiments

We supplemented the tax compliance game with surveys and survey experiments implemented in Gbarnga and in 38 rural towns and villages throughout Bong, Lofa, and Nimba counties. We focused on these counties because they are priorities for both economic development and state consolidation in the post-conflict period, and because together they are host the largest number of Chinese-funded projects outside the capital city, including the controversial Bong Mines iron ore project (Worzi 2017). They therefore represent an important test case for the effects of Chinese aid on state legitimacy in Liberia.

Both surveys were implemented between September and December, 2015. For the urban survey, we randomly selected 193 residents of 9 randomly-selected Gbarnga neighborhoods using the random walk technique, described in the appendix. For the rural survey, we sampled 18 randomly-selected residents of each community, also using the random walk technique. Importantly, the 38 communities in the rural survey are not representative of Liberia, nor of the counties from which they were sampled. Rather, they were the control group for an unrelated field experiment involving the Liberian National Police (LNP). Eligibility for that experiment was restricted to communities located near a usable road and with at least 500 residents. Our sample conforms to these criteria.

While this may limit the generalizability of our results, comparison to a recent nationally representative survey (Vinck, Pham and Kreutzer 2011) suggests that our 38 communities do not differ dramatically from the average Liberian town or village, either in these three counties or in the country as a whole.[11] Residents of these relatively accessible communities are also more likely to be affected by foreign-funded

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[11]For example, the average age of respondents in our sample is 39.8 years, compared to 37.4 nationwide. 88% of our respondents are Christian, compared to 86% nationwide. 31% of our sample has no education, compared to 35% nationwide. 65% of our respondents work in agriculture, compared to 43% nationwide, but compared to 72% in Bong, 73% in Lofa, and 60% in Nimba, the three counties covered in our survey (Vinck, Pham and Kreutzer 2011).

projects in the first place, making their responses especially informative for our purposes.

Both surveys included the same questions to measure respondents' prior exposure to Chinese and US aid, including whether they (1) could name or (2) had used any Chinese- or US-funded projects, and also whether (3) they or (4) any of their friends or family members had worked for a Chinese or US contractor. Both surveys also included the same survey experiment, in which respondents were randomly assigned to hear truncated versions of the same vignettes used in the tax compliance game. Following the vignettes, respondents were asked four questions measuring whether they believe Liberians have an obligation to pay taxes (1) even if foreign donors provide most services, (2) even if government "eats" some of the taxes it collects, (3) even if Liberian taxpayers are very poor, and (4) even if government makes bad policies. These conditional clauses were added to reduce the risk of social desirability bias: by offering respondents a possible justification for tax evasion, we hoped to alleviate any pressure to provide affirmative answers. A control group was asked the same four questions without the vignette. Balance tests, descriptive statistics, and the text of the vignettes are again provided in the appendix.

3.2 Cross-country Research Design

For our cross-country analysis we combine Afrobarometer data with information on the geographical distribution of Chinese aid gleaned from AidData's *Chinese Official Finance to Africa Dataset, 2000-2012, Version 1.1.1* (Strange et al. 2017; Dreher et al. 2016). AidData tracks aid from "non-traditional" donors, including China. Because many of these donors do not participate in international transparency regimes, the details of the aid they provide are generally unknown. AidData fills this gap with information derived from newspaper articles, radio and TV transcripts, the websites of recipient governments and their respective embassies, and academic articles and NGO reports. We merge this data with rounds 2-6 of the Afrobarometer survey, which AidData geocoded to the community (village) level. Our analysis includes data from all 38 Afrobarometer countries.[12]

We code whether each Afrobarometer respondent lives within 30, 40, or 50km of a Chinese project. Using an approach similar to Isaksson and Kotsadam (2018*a*), we then compare respondents living near *planned* projects–projects that have been announced, either formally (commitments) or informally (pledges)–to those living near projects that were either *active* (i.e. in the construction or implementation phase) or *completed* by the time of the survey. By comparing planned projects to active or completed ones, we correct for possible selection effects, whereby the siting of Chinese projects is itself correlated with state legitimacy. By comparing active to completed projects, we distinguish the impact of implementation– including potentially abusive hiring and management practices–from the impact of the services China provides.

Our identifying assumption is that, conditional on covariates, respondents living near planned projects are valid counterfactuals for those living near active or completed ones. One potential threat to this as-

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[12]Algeria, Benin, Botswana, Burkina Faso, Burundi, Cameroon, Cape Verde, Cote d'Ivoire, Egypt, Ethiopia, Gabon, Ghana, Guinea, Kenya, Lesotho, Liberia, Madagascar, Malawi, Mali, Mauritius, Morocco, Mozambique, Namibia, Niger, Nigeria, Sao Tome and Principe, Senegal, Sierra Leone, South Africa, South Sudan, Sudan, Swaziland, Tanzania, Togo, Tunisia, Uganda, Zambia, and Zimbabwe.

sumption is that recipient governments prioritize projects located in politically important areas. If projects sited in these areas are more likely to be completed than those sited elsewhere, then our approach is potentially biased. We view this risk as minor, as there is no reason to expect recipient governments to prioritize in this way during the implementation phase, but not during the planning phase. Nonetheless, we control for whether or not each project is located in the region of the president's birth, which has been shown to be an important source of favoritism in the distribution of Chinese aid (Dreher et al. 2016).

Another potential threat is that citizens protest objectionable projects during the planning or implementation phase, causing either China or the recipient government to cancel them altogether. Protests, in this sense, are not only an outcome of project planning and implementation, but also a potentially confounding determinant of project completion. Again, we view this risk as relatively minor, as there seem to be few (if any) examples of Chinese projects being cancelled due to protests in Africa.[13] Nonetheless, as an imperfect proxy, we use the Armed Conflict Location and Event Dataset (ACLED) to control for the number of protests that occurred within 30, 40, and 50km of each respondent before the first project was planned within that same radius.[14]

Finally, an important limitation of AidData is that it lacks precise information on the location and timing of many Chinese projects. Without accurate geographic and temporal data, we cannot tell whether a given respondent lives near a given project, nor what stage the project is in. This limitation afflicts *all* studies that use AidData to operationalize exposure to Chinese aid (Dreher and Fuchs 2015; Dreher et al. 2018; 2016; 2017; Hernandez 2017; Isaksson and Kotsadam 2018*a*;*b*; Jones and Tarp 2016; Strange et al. 2013), and ours is no exception. We include only projects for which there is precise geographic information within 25km of a known location (AidData precision code 1 or 2). We also exclude country-wide projects and projects distributed through the central government and targeting the capital city alone. From this subset we further exclude projects that are coded as either active or completed but that do not have corresponding start or end dates, using whatever temporal data *is* available to limit the number of observations we drop.[15] As of 2013, the last year for which data exists, there were 338 planned projects with precise geographic and temporal information, and 1,183 completed or active projects. This represents 43% of all projects in the AidData dataset.

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[13]There have been cases of cancellation due to protests in Asia–notably in Myanmar, Pakistan, and Nepal. The closest African analogue was the cancellation of a 2007 inauguration ceremony at Zambia's Chambishi copper mine (Alden 2007), but that project was still completed, protests notwithstanding.

[14]Ideally we could control for protests that occurred *after* the first project was planned but *before* it was completed. This, however, would induce post-treatment bias. Ours is a second-best approach. We interpret past protests as a proxy for *potential* protests in the future.

[15]If the project has a start but not an end date, we drop respondents who live near it and were surveyed after the start date. (Those surveyed before the start date are coded as living near a planned project.) If the project has an end but not a start date, we drop respondents who live near it and were surveyed before the end date. (Those surveyed after the end date are coded as living near a completed project.) If the project has neither a start nor an end date, we drop respondents who live near it and were surveyed after the commitment year. (Those surveyed before the commitment year are coded as not living near a project at all.)

3.3   Advantages and Limitations of Our Approach

Ours is not the first attempt to test the effects of foreign aid on the legitimacy of recipient states. While previous studies have yielded valuable insights, they have been limited in at least four crucial ways. First, most rely on just one measure of aid, typically gleaned from AidData (e.g. Jones and Tarp 2016) or the World Bank (e.g. Remmer 2004; Svensson 2000). This raises the risk of measurement error and missing data correlated with the dependent variable.[16] Sources like AidData also capture the presence of foreign-funded projects, but not the ways that individuals engage with those projects (e.g. as workers or beneficiaries).

Second, most studies rely on surveys alone to measure tax compliance and other proxies for state legitimacy (e.g. Dietrich and Winters 2015; Dietrich, Mahmud and Winters 2017; Sacks 2012). While informative, these proxies are susceptible to social desirability bias. Third, most previous studies rely on one of two identification strategies: selection on observables or random assignment to vignettes in the context of survey experiments (e.g. Dietrich and Winters 2015; Dietrich, Mahmud and Winters 2017). Selection on observables strategies are vulnerable to omitted variables, while randomly assigned vignettes may not generalize to the real world. Finally, most previous studies have been conducted at one of two levels of analysis: the individual level within a single country, or, more commonly, the national level across multiple countries. The latter captures general trends but not the specific dynamics of particular cases, while the former captures specific dynamics but not general trends.

Our study addresses these limitations by using multiple sources of data and multiple measurement and identification strategies at multiple levels of analysis. Each approach complements and extends the others. While surveys capture real-world, individual-level exposure to Chinese aid, they rely on self-reports, and thus are potentially susceptible to non-random recall.[17] AidData captures real-world exposure to Chinese aid without relying on self-reports, but only indirectly (since not all individuals who live near a Chinese project use or even know about it). Moreover, identification strategies that rely on AidData or survey self-reports are vulnerable to omitted variables. Survey experiments solve this latter problem, but are more stylized, and still rely on self-reports to measure tax compliance and other outcomes. The tax compliance game solves the selection problem while capturing observed rather than self-reported tax compliance, but is even more stylized. Finally, while our within-country data are rich and highly detailed, they may not generalize to other countries. Our cross-country data are coarser, but may have more external validity.

A key contribution of our study is our ability to triangulate across approaches to identify consistent patterns, standardizing elements of our research design where possible in order to facilitate comparison. (For example, we use similar vignettes in the survey experiment and behavioral game, and we incorporate Afrobarometer questions into our own survey). The more consistent our results, the less likely they are to reflect the idiosyncrasies of a particular method or sample. But our analysis is not without limi-

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[16]For example, if China is more likely to hide aid to corrupt or autocratic regimes, and if those regimes are more likely to be perceived as illegitimate, then cross-national comparisons are likely to underestimate the negative effects of Chinese aid on state legitimacy.

[17]For example, respondents who view China favorably may be more likely to remember using Chinese-funded projects.

tations. For example, because tax return data are scarce and unreliable in most African countries, we cannot assess the relationship between Chinese aid and *actual* tax compliance. While we measure tax compliance in multiple ways, all of our proxies rely on either survey self-reports or behaviors observed in the lab. Of course, even if we had access to tax returns, these would still yield problematic proxies for tax compliance, since we would not know taxpayers' true (rather than reported) income. This challenge has long been recognized in the taxation literature, and was one of the reasons the tax compliance game was created in the first place (Alm, Jackson and McKee 1992). Nonetheless, it is possible that Chinese (and other) aid affects real-world tax compliance in ways our research design cannot detect.

Our analysis is also limited by the difficulty of distinguishing Chinese aid (Overseas Development Assistance, or ODA) from other forms of assistance (Other Official Flows, or OOF). The former includes grants, debt relief, and highly concessionary loans, and is intended to promote economic or social development; the latter includes other loans and credits not intended to improve economic or social welfare. These different forms of "development finance" may reflect distinct policy priorities, and may have disparate effects on recipient states. But as Dreher et al. (2018, 11) note, information on concessionality and intent is unavailable for most Chinese projects, and these distinctions can only be drawn suggestively, even in the AidData dataset.[18]

While our inability to distinguish ODA from OOF is a limitation, it is not as restrictive as it may seem. Since we are interested in China's impact on perceptions of recipient states, the salient question for our purposes is how different categories of development finance are interpreted by citizens. In pre-testing in Liberia, we found that citizens generally do not distinguish between different types of projects, referring to them interchangeably as "aid," "investment," and "assistance." Citizens also tend to voice similar concerns about all types of projects (e.g. their implications for workers' rights). While disaggregating ODA from OOF is potentially worthwhile, our inability to do so should not affect the internal validity of our research design, and we interpret our results as capturing the impact of Chinese "aid," broadly defined.

4   Results

We present results for each component of our research design in turn. To summarize, we find little evidence that Chinese aid erodes tax compliance or morale or diminishes perceptions of government, either in Liberia or across Africa. In Liberia, randomly assigned vignettes about government or US provision of public goods *increase* tax morale (at least in our urban sample), while vignettes about Chinese-funded public goods have no effect relative to control (Section 4.1.2). Vignettes have no effect in our tax compliance game (Section 4.1.1) or rural survey experiment (Section 4.1.2), regardless of treatment group. Observationally, exposure to Chinese aid is associated with *improved* perceptions of government in our rural, and exposure to US aid is correlated with improved tax morale (Section 4.1.3).

Cross-nationally, Chinese projects tend to be sited in locations with lower trust in government and more negative assessments of democracy (Section 4.2). After differencing away this selection effect, however,

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[18]Dreher et al. (2018, 11) adopt a "second-best" approach by distinguishing "ODA-like" projects from "OOF-like" ones, recognizing that this distinction only partially aligns with more standard definitions of ODA and OOF.

the impact of Chinese projects on citizens' attitudes and behaviors is generally a net null. Disaggregating these results across the 20 Afrobarometer countries with sufficient data to allow for a country-by-country comparison, Chinese aid appears to have positive effects in about as many countries as it has negative effects, mixed or ambiguous effects in several countries, and null effects in most.

4.1 Within-country Results

4.1.1 Behavioral Game Results

Figure 1 reports average treatment effects for our modified tax compliance game in Liberia.[19] We present our results graphically for ease of interpretation. Our proxy for tax compliance is simply the ratio of unreported income to total income over the 10 rounds of the game; the lower this ratio, the higher the degree of tax compliance. While the income participants received in any given round was randomized, the total they received (1,000 LD) was not, and the distribution of possible incomes was held constant across participants to eliminate the possibility of differential income effects.[20] To mitigate incidental imbalance and increase precision, we include individual-level controls for gender, age, employment, religion, and education. The squares in Figure 1 denote predicted values of the dependent variable in each treatment group, and the lines denote 95% confidence intervals.

We find no evidence to suggest that information about foreign aid erodes tax compliance in the game. While rates of tax evasion are generally high—at or above 50% across the board[21]—they are not statistically different across treatment groups. Predicted values are nearly identical regardless of treatment group, ranging from a low of 49% in the US group to a high of 54% in the government group—a statistically insignificant difference of five percentage points. Our 95% confidence intervals are narrow as well, suggesting that these nulls are not merely artifacts of statistical imprecision.

4.1.2 Survey Experiment Results

Figures 2 and 3 report average treatment effects for our rural and urban survey experiments, respectively. The dependent variable is an additive index of dummies indicating whether respondents agree or strongly agree with each of the four statements about tax morale described in Section 3.1.2. (Our results are unchanged if we instead add the Likert scales without dichotomizing.) All specifications include

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[19]In our pre-analysis plan we pre-specified five hypotheses about heterogeneous treatment effects as well. These results are ancillary to our analysis here, but we report them in the appendix for completeness.

[20]In other words, in each round, each participant sampled without replacement from the same distribution of incomes. Over 10 rounds, each participant received each of 10 possible incomes exactly once–0, 70, 75, 80, 85, 95, 105, 130, 160, or 200 LD.

[21]Interestingly, observed rates of tax evasion in the game are much higher than self-reported rates in the survey, which, as we show in the appendix, range from 5% to 15%, depending on the sample. One potential explanation for this disparity is that survey respondents over-reported due to social desirability bias. (Avoiding this problem is part of the reason the tax compliance was created in the first place.) Another is that lab participants interpreted the game as disconnected from real-world taxation, and so evaded at a higher rate than they would outside the lab. We view this explanation as unlikely: in focus groups conducted after the game, we found that most participants directly connected the experiment to real-world taxation, and took their decisions seriously.

individual-level controls for gender, age, employment, religion, education, and household wealth. For the rural sample, we also include district fixed effects and community-level controls for population and average household wealth, literacy, employment, and education, gleaned from the 2008 census. Figures 2 and 3 report predicted values of the dependent variable in each treatment group, with standard errors clustered by neighborhood (for the urban sample) or community (for the rural sample).

Again, we find little evidence to suggest that information about foreign aid diminishes tax morale. If anything, the opposite appears to be true. Our point estimates are nearly identical across treatment groups in the rural sample, with almost fully overlapping confidence intervals. In the urban sample, in contrast, respondents are *more* likely to believe they have an obligation to pay taxes in the government and US groups. Respondents in the government group score an average of 3.38 out of 4 on our index of tax morale, compared to an average of 2.76 in the control group ($p = 0.05$) and 2.79 in the China group ($p = 0.04$). Similarly, respondents in the US group score a 3.29 on average, which is statistically larger than the average scores in either the control ($p = 0.06$) or China ($p = 0.02$) groups.

Priming urban respondents to reflect on the role taxation plays in public goods provision thus reinforces their belief that tax compliance is an obligation all citizens share. But priming them to reflect on US aid has the same effect, suggesting that, if anything, exposure to aid strengthens rather than weakens tax morale. Importantly, average scores on our tax morale index are substantively and statistically indistinguishable between the control and China groups. This suggests that learning about China's role in public goods provision neither boosts nor diminishes tax morale, at least relative to control.

4.1.3 Survey Results

Finally, Table 1 reports correlations between respondents' attitudes towards government and their self-reported exposure to Chinese and US aid in our rural (top panel) and urban (bottom panel) samples. We operationalize exposure as an additive index of dummies indicating whether respondents (1) know about or (2) have used any public goods provided by foreign donors, and whether (3) they or (4) any of their friends or family members have ever worked for a foreign contractor. (In the appendix we disaggregate this index to distinguish between users of foreign-funded projects and employees of foreign contractors.)

As discussed in Section 2, foreign aid is believed to affect tax compliance in part through its effect on citizens' perception of government as fair, transparent, and responsive to their needs. To operationalize this perception, the dependent variable in column 1 is an additive index of three dummies indicating whether respondents believe the Liberian government (1) treats all Liberians equally, (2) makes decisions in an "open and transparent" manner, and (3) is free from corruption. As an additional measure of perceived government responsiveness, the dependent variable in column 2 is an indicator for whether respondents express satisfaction with the quality of democracy in Liberia. In columns 3, 4, and 5, our dependent variables are indicators for whether respondents have ever evaded their taxes (a measure of tax compliance), believe the government has the right to collect taxes (tax morale), and believe taxes are easy to avoid, respectively.

The wording of the questions in columns 2 through 5 was taken from the Afrobarometer survey in order

to facilitate comparison with the other 37 countries in our study. By measuring not just whether citizens' believe government has a right to collect taxes, but also whether they believe taxes are easy to avoid, we are better able to disentangle whether citizens pay taxes because they believe they have an obligation to do so or because they fear coercion if they do not—or both (Blair 2017). All specifications include the same controls as in the survey experiment.

Again, we find little evidence to suggest that exposure to Chinese aid diminishes perceptions of government, reduces tax compliance, or weakens tax morale. If anything the opposite appears to be true: the correlation between exposure to Chinese projects and perceptions of government is positive and statistically significant among rural respondents (top panel, column 1), and is positive (though not statistically significant) among urban respondents as well (bottom panel, column 1). Exposure to US aid is positively correlated with respondents' belief that government has a right to tax in the rural sample (top panel, column 4), and with their approval of Liberian democracy in the urban sample (bottom panel, column 2). The former correlation is positive but not statistically significant in the rural sample as well, and the latter is positive but not statistically significant in the urban sample.

4.2   Cross-country Results

Table 2 extends our analysis by testing the effects of Chinese-funded projects across 38 African countries. To correct for possible siting effects, we compare projects that are either active or completed to projects that are planned but have not yet begun. We interpret the latter as the selection effect, and the difference between the former and the latter as the effect of the projects themselves during and after implementation, respectively.

The dependent variable in column 1 is an additive index of dummies indicating citizens' trust in six state institutions: the police, military, local council, parliament, president, and courts. All other dependent variables are identical to those in Table 1. Two of our taxation measures (columns 3 and 5) are available only for rounds 5 and 6 of the Afrobarometer survey. Our remaining dependent variables are available for rounds 2 through 4 as well. All specifications include country and Afrobarometer round fixed effects, as well as controls for gender, age, religion, and education; distance to the capital city; number of previous protests within a 30, 40, or 50km radius; a dummy for cities; and a dummy indicating whether the respondent lives in the president's home region. Standard errors are clustered by community.

A naive comparison of respondents living near completed projects to those living further away would suggest that Chinese aid reduces trust in government and diminishes perceptions of democracy (columns 1 and 2). But this appears to be an artifact of siting. After differencing away the selection effect, completed projects have no effect on trust in government (column 1), perceptions of democracy (column 2), tax compliance (column 3), or tax morale (column 4). Completed projects also appear to mitigate the perception that tax evasion is easy (column 5), perhaps due to increased government presence in areas where Chinese projects have already been completed. Active projects appear to improve perceptions of democracy (column 2), but only temporarily, and only marginally significantly.

Of course, these null aggregate effects may mask variation across the 38 countries in our sample. We

explore this possibility in Figure 4 by disaggregating our analysis by country. This requires stretching the AidData dataset rather thin. Due to sparseness in the data, we are only able to estimate disaggregated results for 20 of the 38 countries in our sample, and only by combining active and completed projects into a single category. Still, the results are revealing. As Figure 4 shows, there are indeed several countries in which Chinese aid appears to have deleterious effects on state legitimacy. For example, after differencing away the siting effect, we find that Chinese projects are negatively correlated with trust in government in Zimbabwe, Mozambique, and (weakly) Ghana. Likewise, Chinese projects are negatively correlated with belief in the government's right to tax in Nigeria, Egypt, and (weakly) Mali.

These negative effects, however, are not consistent across our five dependent variables. For example, while Chinese aid is negatively correlated with tax morale in Egypt, it is positively correlated with perceptions of democracy. Moreover, Chinese aid appears to have *beneficial* effects in a number of African countries. For example, Chinese aid is positively correlated with both trust in government and satisfaction with democracy in Zambia and Namibia (though in Zambia it is also *negatively* correlated with tax compliance). Taken together, these results suggest that Chinese aid has positive effects in about as many countries as it has negative effects; that it has mixed or ambiguous effects in some countries; and that it has null effects in most. This is consistent with the idea in Section 2.3 that aid induces positive and negative effects simultaneously, which, when aggregated, may offset one another to produce a net null.

In the appendix we explore several alternative specifications that add further nuance to these results. In particular, we find evidence of an adverse aggregate effect on trust in government when we expand our bandwidth from 30 to 40 or (especially) 50km. But this effect is specific to trust in government; we do not observe similarly negative effects on our other five outcomes. One possible explanation is that Chinese aid has detrimental effects on trust among respondents who live just far enough away that they cannot take advantage of the services China provides, though this is speculative, and expanding our bandwidth increases the risk of confounding.

We also find evidence of an adverse effect on trust in government and tax morale when we disaggregate the data to focus on infrastructure projects alone. A possible explanation for this finding is that governments cannot easily claim credit for large, highly visible infrastructure projects, though this too is speculative. Disaggregating by sector stretches AidData even thinner, and a comparison of effects across sectors is likely not causally identified. We therefore interpret these results with caution. Moreover, whatever the adverse effects of infrastructure projects, they seem to be offset by the null or positive effects of non-infrastructure projects in our aggregated specification.

5   Discussion

We expected that Chinese aid would diminish the legitimacy of recipient African states. Is it possible that our failure to detect such an effect is an artifact of flaws in our research design—measurement error, for example, or lack of statistical power? While we cannot definitively eliminate these concerns, there are several reasons to believe they do not explain our findings. First and most important, our results are consistent across multiple approaches to measurement, identification, and estimation, suggesting that

neither statistical power nor measurement error is likely to explain them (unless *all* of our approaches are equally biased, and in the same direction). Second, most of our nulls are precisely estimated, with narrow confidence intervals and point estimates either very close to zero (Table 1) or very close to one another (Figures 1, 2, and 3). If statistical power were a problem, we would expect to observe wider confidence intervals around more idiosyncratic point estimates. But we do not.

A more formal power analysis confirms these intuitions. In both our lab-in-the-field experiment and rural survey experiment, we have 80% power to detect an effect equal to roughly 0.29 standard deviations of the control group mean. While there are no obvious rules of thumb to apply here, this would generally be considered a small effect. In our urban survey experiment, we have 80% power to detect an effect of roughly 0.51 standard deviations. This would be considered a small to moderate effect. To detect the actual difference we observed between the China and control groups in our lab-in-the-field experiment, we would have needed a sample of approximately 32,000 participants—roughly the entire population of Gbarnga.[22] The actual difference in tax morale between the China and control groups in the urban survey experiment is very close to zero, and we could not have detected it even with millions of respondents. To detect the actual difference in tax morale between the China and control groups in the rural survey experiment, we would have needed a sample of over 3,000 respondents. This is nearly two-thirds the sample size needed to conduct a nationally representative survey (Vinck, Pham and Kreutzer 2011), applied in our case to just 38 rural communities across three of Liberia's 15 counties.[23]

Third, as we show in the appendix, while we do not find that Chinese aid diminishes Liberians' perceptions of government, we *do* find that it improves their perceptions of Chinese donors themselves. We find a similar relationship between US aid and perceptions of American donors.[24] These results are important not only because they contradict the conventional wisdom that China is fomenting backlash among African citizens, but also because they affirm that measurement error and lack of statistical power are unlikely to explain our nulls. If our research design can detect a correlation between Chinese aid and perceptions of China, then the most likely explanation for our failure to detect a correlation between Chinese aid and perceptions of government is that the correlation does not exist, or is very small.[25] We also show in the appendix that proximity to completed Chinese projects in AidData is highly positively correlated with our survey-based measures of exposure to Chinese aid in Liberia, while proximity to planned projects is not. This makes sense, and should further mitigate concerns about measurement error in the observational component of our study.

Is it possible that the hypotheses we tested were implausible in the first place, or that our research design

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[22]According to the 2008 census, Gbarnga has a population of 34,000.

[23]Of course, these power calculations vary if we adjust our parameters. The minimum detectable effect is smaller if we adjust for covariates, or larger if we correct for multiple comparisons. Regardless, the samples we would have needed to detect the effects we actually observe are very large.

[24]Interestingly, exposure to Chinese aid appears to improve perceptions of American donors as well. We consider possible explanations for this result in the appendix.

[25]Statistical power is a function of (1) the magnitude of the effect, (2) the sample size, and (3) the variance of the outcome. The stronger the effect, the larger the sample, and the smaller the variance of the outcome, the greater the power. In our case, respondents' perceptions of government are *less* variable than their perceptions of China or the US, and our sample sizes are identical for almost all of our outcomes. Statistical power could only explain the dramatic disparities in our results if the effects themselves are dramatically different.

somehow guaranteed a null result? Again, we view these explanations as unlikely. As discussed in Section 2, there are many reasons to expect foreign aid to diminish the legitimacy of recipient states, and many reasons to believe Chinese aid is especially likely to have these pernicious effects. Moreover, while our vignettes emphasized both the positive and negative aspects of Chinese (and US) aid, the fact that we find statistically significant *beneficial* treatment effects in the urban survey experiment suggests that nulls were not a foregone conclusion. In any event, this "bundled" treatment more accurately captures the mixed signals about aid that recipient populations actually receive. We could have manufactured adverse treatment effects by heavily priming the (purported) pathologies of Chinese aid, but this would have taught us little about the real world. The fact that neither our experimental *nor* our observational measures of exposure to Chinese aid are negatively correlated with perceptions of government lends additional credence to our results.

6   Conclusion

Scholars and policymakers have long debated the effects of foreign aid on state legitimacy. Understanding how the billions of dollars in aid delivered to developing countries each year affects the social contract between citizens and recipient states is of significant practical and theoretical importance. As "non-traditional" donors like China expand to compete with "traditional" (Western) ones, understanding the impact of their potentially distinctive approach—larger scale projects implemented at lower cost with fewer conditionalities and, allegedly, more abusive hiring and management practices—is an especially urgent concern.

Using multiple sources of data and multiple measurement and identification strategies at multiple levels of analysis, we find that Chinese aid has generally benign effects on citizens' relationships with recipient states. Cross-nationally, after differencing away siting effects, Chinese aid appears to have weak or null effects on trust in government, perceptions of democracy, and tax compliance and morale. In Liberia, exposure to Chinese aid is associated with more rather than less favorable attitudes towards government (among rural respondents at least). Vignettes about government or US provision of public goods appear to increase tax morale (among urban respondents). Vignettes about Chinese provision of public goods have no effect relative to control. These results are robust to our various methodological approaches, and are unlikely to be artifacts of selection bias, measurement error, or a lack of statistical power. Taken together, they belie the notion that China is a "rogue donor" whose aid to Africa is rupturing the social contract between citizens and recipient states.

If selection bias, measurement error, and statistical power do not explain our results, what does? We cannot say for certain, and because we expected to find evidence of adverse effects—an expectation that we publicly pre-registered—any explanation for our unexpected results is inevitably post-hoc. As we discuss in Section 2.3, however, it is possible that citizens are just as conflicted in their perceptions of Chinese aid as the policy and scholarly literature is. Citizens may be grateful to government for attracting foreign largess, but worried about corruption; appreciative of the above-market wages that foreign contractors pay, but concerned about dependence; etc. As we show in the appendix, these conflicted views emerged clearly in the focus groups we conducted after our tax compliance game. If Chinese aid is viewed as a

blessing and a curse simultaneously, then we should not be surprised that its net impact is a null.

Together, our results suggest that fears about the adverse effects of foreign aid on state legitimacy may be overblown. They also suggest, however, that hopes of any *beneficial* effects may be overblown as well. This is relevant not only to the academic literature on foreign aid, but also to the more practical question of how best to deliver assistance to the world's poorest countries. It is possible, for example, that donors could enhance the reputation of recipient governments by "co-branding" with them, or by more explicitly allowing government officials to claim credit for the services donors provide. Evidence on these possibilities is just beginning to emerge (Dietrich and Winters 2015; Dietrich, Mahmud and Winters 2017; Guiteras and Mobarak 2014). We view this as a promising avenue for future research to explore.

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Tables and Figures

Figure 1: Average treatment effects for tax compliance game in urban Liberia
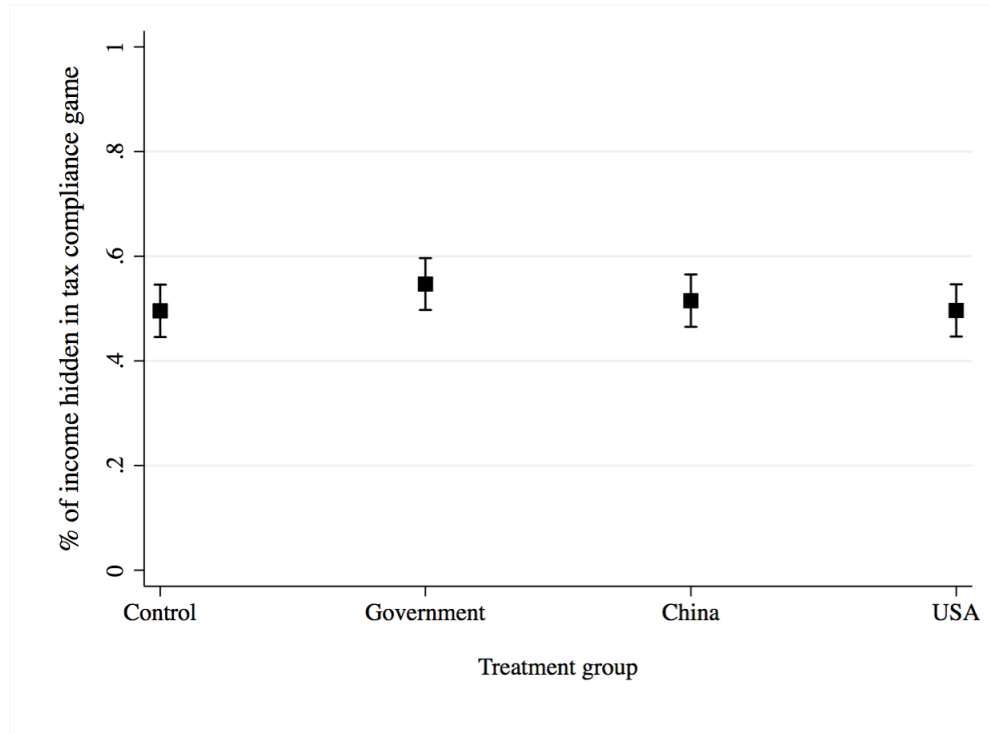
Notes: Average treatment effects on tax evasion in tax compliance game. Squares denote fitted values; lines denote 95% confidence intervals.

Figure 2: Average treatment effects for survey experiment in rural Liberia
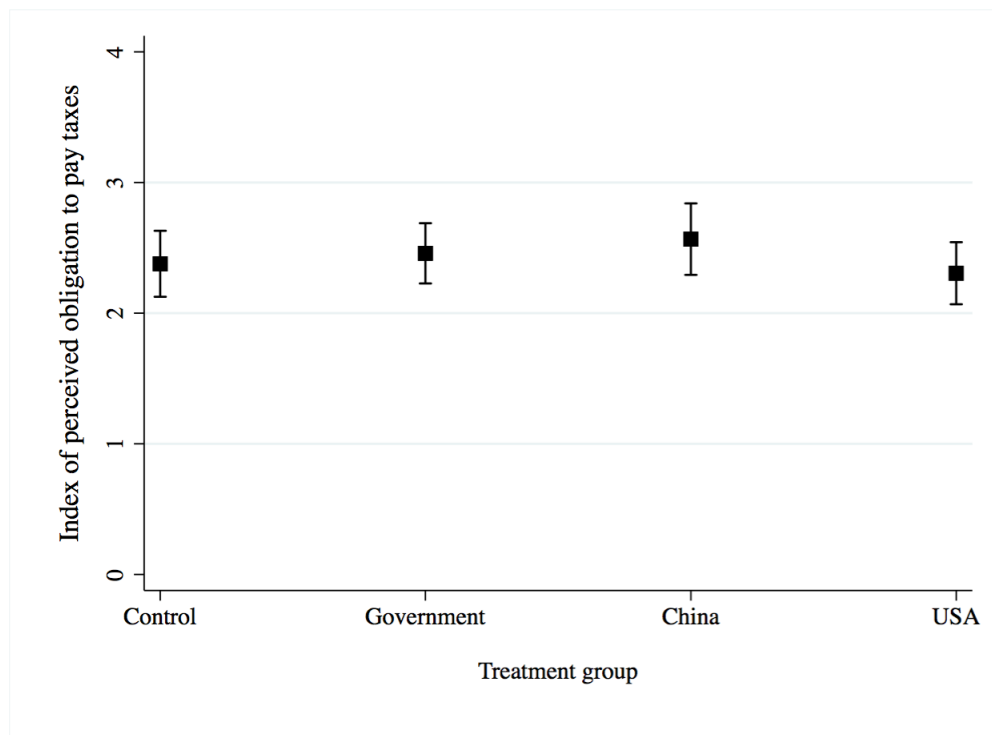
Notes: Average treatment effects on perceived obligation to pay taxes (indexed from 0 to 4) in survey experiment in rural Liberia. Squares denote fitted values; lines denote 95% confidence intervals. Standard errors are clustered by community.

Figure 3: Average treatment effects for survey experiment in urban Liberia
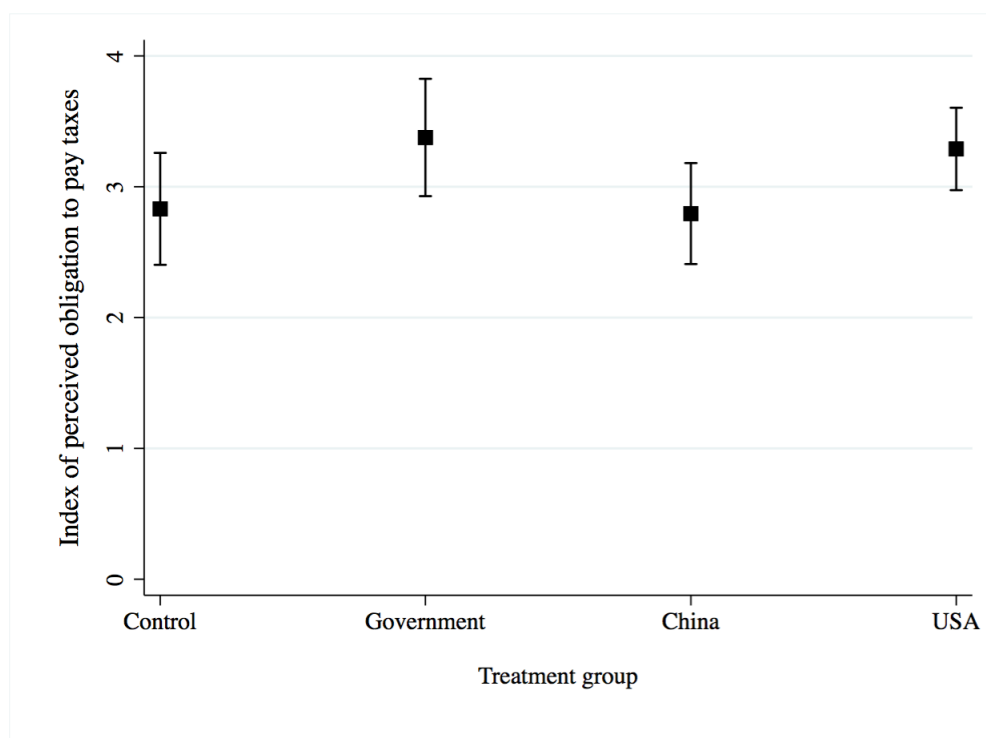
Notes: Average treatment effects survey experiment vignettes on respondents' perceived obligation to pay taxes in urban Liberia. Perceived obligation to pay taxes is operationalized as an additive index ranging from 0 to 4. Squares denote fitted values; lines denote 95% confidence intervals. Standard errors are clustered by neighborhood.

Table 1: Correlation between foreign aid and state legitimacy in Liberia using survey data

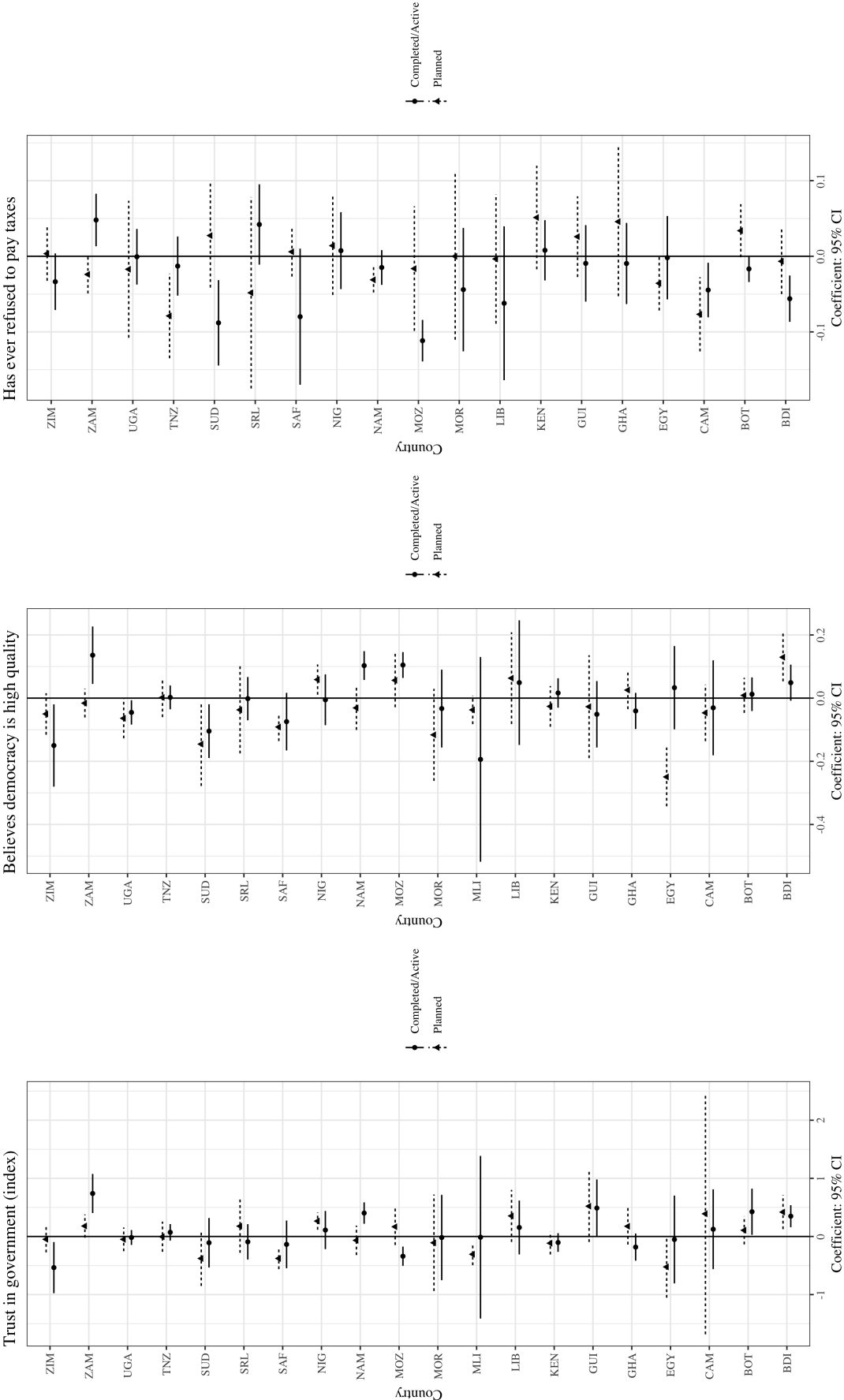|	Perceptions of government (index)	Believes democracy is high quality	Has ever refused to pay taxes	Believes government has right to tax	Believes it is easy to avoid paying taxes
Rural respondents					
Index of exposure to Chinese projects	0.11	0.01	-0.00	0.01	0.01
	(0.04)**	(0.02)	(0.01)	(0.02)	(0.02)
Index of exposure to US projects	0.00	0.01	0.01	0.05	-0.02
	(0.04)	(0.01)	(0.01)	(0.02)***	(0.02)
Observations	669	669	640	669	640
Individual-level controls	Y	Y	Y	Y	Y
Community-level controls	Y	Y	Y	Y	Y
District FE	Y	Y	Y	Y	Y
Urban respondents					
Index of exposure to Chinese projects	0.04	-0.01	0.01	0.02	-0.02
	(0.05)	(0.03)	(0.05)	(0.02)	(0.04)
Index of exposure to US projects	0.06	0.05	-0.00	0.01	-0.01
	(0.08)	(0.02)*	(0.02)	(0.02)	(0.02)
Observations	193	193	183	183	183
Individual-level controls	Y	Y	Y	Y	Y
Community-level controls	N	N	N	N	N
District FE	N	N	N	N	N

Notes: Correlation between exposure to foreign aid and investment and perceptions of government in Liberia. Exposure is operationalized as an additive index ranging from 0-4. Standard errors, clustered by community (rural sample) or neighborhood (urban sample), are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2: Correlation between Chinese aid and state legitimacy across Africa using 30km buffer

	Trust in government (index)	Believes democracy is high quality	Has ever refused to pay taxes	Believes government has right to tax	Believes it is easy to avoid paying taxes
Completed	-0.07	-0.03	-0.01	-0.01	-0.01
	(0.03)**	(0.01)***	(0.005)	(0.01)	(0.01)
Active	-0.07	0.03	-0.01	-0.01	0.01
	(0.09)	(0.02)	(0.04)	(0.02)	(0.05)
Planned	-0.05	-0.01	0.00	0.00	0.02
	(0.03)*	(0.01)	(0.01)	(0.01)	(0.01)**
Completed vs. planned p-value	0.681	0.129	0.264	0.277	0.025
Active vs. planned p-value	0.790	0.081	0.734	0.510	0.907
Completed vs. active p-value	0.953	0.021	0.888	0.981	0.671
Observations	162,920	180,173	127,103	185,746	98,286
Individual-level controls	Y	Y	Y	Y	Y
Community-level controls	Y	Y	Y	Y	Y
Country FE	Y	Y	Y	Y	Y
Buffer	30km	30km	30km	30km	30km

Notes: Correlation between exposure to foreign aid and investment and perceptions of government across 38 African countries. Exposure is operationalized as a dummy for any Chinese projects within a 30km radius. Standard errors, clustered by community, are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
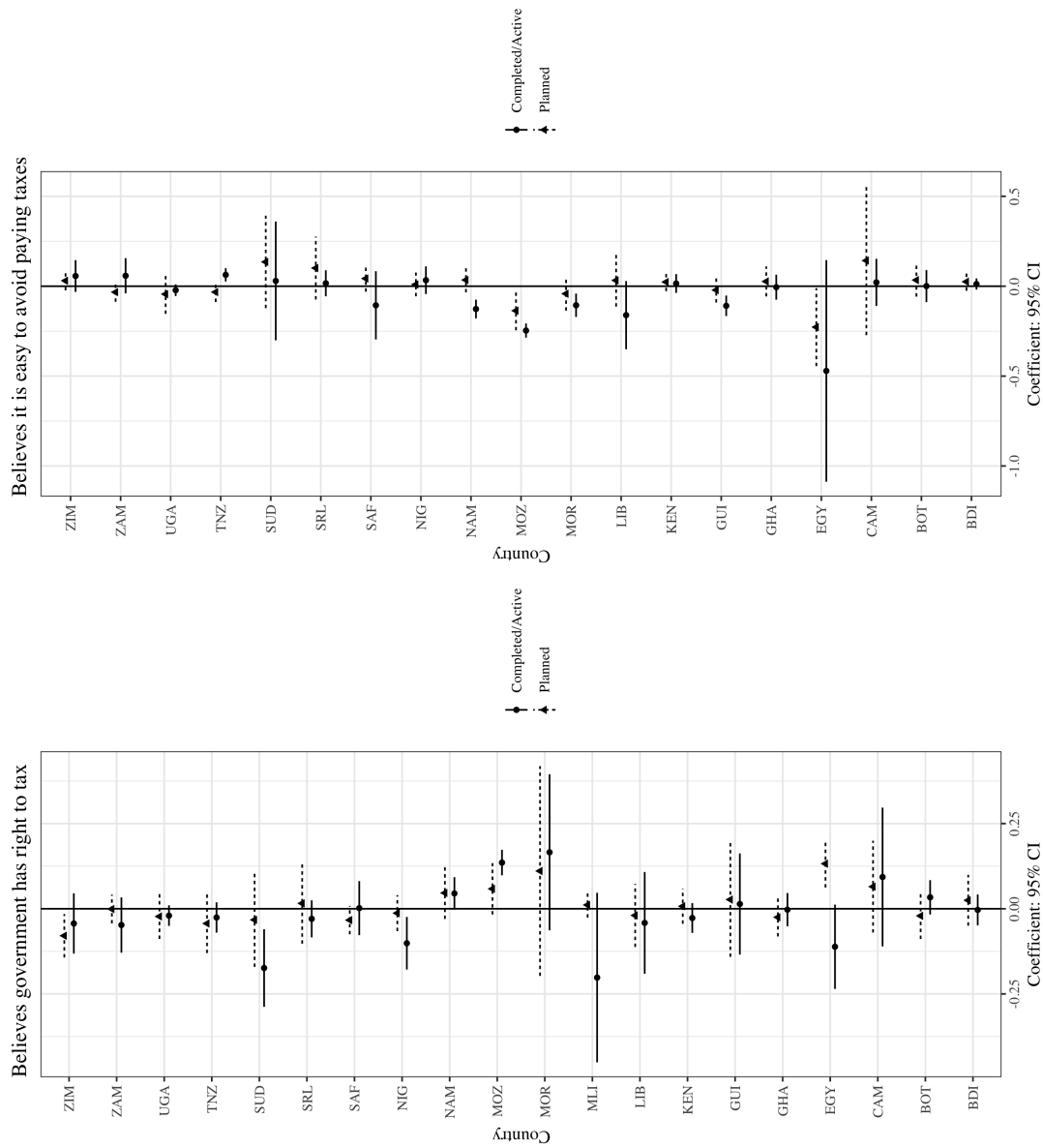
Figure 4: Correlation between Chinese aid and state legitimacy by country using 30km buffer

Notes: Correlation between exposure to foreign aid and investment and perceptions of government disaggregated by African country. Exposure is operationalized as a dummy for any Chinese projects within a 30km radius. Standard errors are clustered by community. Triangles and dashed lines denote point estimates and 95% confidence intervals for planned projects; circles and solid lines denote point estimates and 95% confidence intervals for active or completed projects.

Figure 4: Correlation between Chinese aid and state legitimacy by country using 30km buffer (cont.)

Notes: Correlation between exposure to foreign aid and investment and perceptions of government disaggregated by African country. Exposure is operationalized as a dummy for any Chinese projects within a 30km radius. Standard errors are clustered by community. Triangles and dashed lines denote point estimates and 95% confidence intervals for planned projects; circles and solid lines denote point estimates and 95% confidence intervals for active or completed projects.