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Aid and Institutions: Local Effects of World Bank Aid on Perceived Institutional Quality in Africa

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Local effects of World Bank aid on perceived institutional quality in Africa[1]

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Abstract:

Motivated by the lack of sub-national empirical evidence on the relationship between aid and institutional development, this study explores the local effects of World Bank aid on perceived institutional quality in African aid receiving countries. We combine geo-referenced data on the subnational allocation of World Bank aid projects to Africa over the 1995-2014 period with geo-coded survey data for 73,640 respondents across 12 Sub-Saharan African countries. To account for the endogenous placement of World Bank project sites, we compare the estimated effect of living near a site where a World Bank project was under implementation or completed at the time of the interview, to that of living near a site where we know that a World Bank project appeared after the survey date. The empirical results suggest a positive impact of World Bank aid on perceived institutional quality, as measured by citizens' expressed willingness to abide by key formal institutions. This applies even if we consider overall World Bank aid, i.e. not just projects specifically targeted at institutional development. As may be expected, however, the estimated effects are more pronounced when restricting our attention to projects focusing on institution building. Notably, the observed effects concern finalized projects, not projects still under implementation, highlighting that institutional change is a slow process.

JEL classification: F35, O17, O19, O55

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1 Introduction

One of the goals established in the 2030 Agenda for Sustainable Development is to 'build effective, accountable and inclusive institutions at all levels' (UN, 2015). While the importance of building well-functioning state institutions in developing countries is uncontroversial (North, 1990; Acemoglu and Robinson, 2012), the question of whether donors are actually helpful in this process is more contentious.

An aid optimist would argue that an inflow of resources and technical assistance can help recipient countries boost government effectiveness, and that aid (and the threat of not receiving aid) can persuade states to embark on reform. An aid skeptic, however, would argue that aid promotes rent-seeking behavior and reduces the incentives for democratic accountability and thus the democratic pressures to build effective institutions.

Donors themselves unsurprisingly fall in the former category. The World Bank, in focus in this paper, emphasize their commitment to promoting good governance and institution building. In their own words: "Capable, accountable and inclusive governance is at the heart of the World Bank's twin goals of ending extreme poverty and boosting shared prosperity" (International Development Association, 2021). Support to public institutions, public financial management, government revenue generation, corporate governance, decentralization and sub-national governance are some of the strategic priorities they highlight (World Bank, 2020). Examples of World Bank projects focusing on institution building in African partner countries include assistance to improve judicial services in Kenya, training in accountability and public financial management of government and civil society organization staff in Burkina Faso, and a decentralization program promoting citizen engagement in Mauretania (World Bank, 2018; International Development Association, 2019).

The empirical evidence on the relationship between aid and institutions is mixed, and so does little to resolve the controversy between aid optimists and aid sceptics (e.g. Svensson, 2000; Tavares, 2003; Bräutigam and Knack, 2004; Knack, 2004; Djankov et al., 2008; Busse and Gröning, 2009; Okada and Samreth; 2012; Jones and Tarp, 2016). In light of conflicting predictions, conflicting empirical evidence is arguably not surprising.

Nonetheless, an additional reason for the inconclusive results may be the tendency to study the relationship at the country level. While useful for uncovering broad patterns, the macro

literature on aid effectiveness, focusing on country level relationships between aid inflows and outcomes, face important challenges. First, it is notoriously difficult to establish causality. Receiving aid is associated with a multitude of country characteristics – known and unknown – that will tend to influence the estimates when seeking to establish the causal impact of aid (e.g. Bräutigam and Knack, 2004). Second, it is common to aggregate over aid flows that should have different effects, since provided for different purposes. (Clemens et al., 2012; and Bourguignon and Gunning, 2016). Furthermore, even if taking steps to disaggregate aid and consider the impact of different aid flows (e.g. Jones and Tarp, 2016), the cross-country literature is not able to account for heterogeneity within countries. While (specific forms of) aid may have effects in targeted areas, these effects may not be sufficiently large to be measurable at the country level or they may be obscured by omitted variable bias (Dreher and Lohmann, 2015). Indeed, many development projects are targeted at local development, arguably meaning they should be judged against location-specific outcomes (Findley et al., 2011). Against this background, a finer lens is arguably needed when studying the effect of aid on institutions.

The aim of this paper is to investigate the local effects of aid on local institutional quality, as measured by citizens' expressed willingness to abide by key formal institutions (the courts, the police and the tax authority), in Sub-Saharan African aid receiving countries. We explore a) the local effects of overall World Bank aid, b) the local effects of World Bank aid targeted specifically at institutional development, and c) potential donor heterogeneity in results using data on US and Chinese aid.

To address these questions, we geographically match a geo-referenced dataset on the subnational allocation of World Bank aid projects to Africa over the 1995-2014 period with Afrobarometer survey data for 73,640 respondents across 12 African countries. The main estimation strategy to account for the endogenous placement of World Bank project sites consists in comparing the estimated effect of living near a site where a World Bank project was either under implementation or completed at the time of the interview, to that of living near a site where we know that a World Bank project will appear subsequently. In alternative specifications we instead use matching, with very similar results.

The empirical findings, which are robust across a wide range of specifications, indeed suggest a positive impact of World Bank aid on perceived institutional quality in the local area. This applies even if we consider overall World Bank aid, i.e. not just aid projects specifically targeted at

institutional development. As may be expected, however, the estimated effects are more pronounced when we restrict our attention to projects focusing on institution building. Notably, the observed effects concern finalized projects, not projects still under implementation.

To our knowledge, this is the first study using geocoded aid data and geocoded institutional outcome data to systemically investigate the local effects of aid on perceived institutional quality in African aid-receiving countries. Broadly speaking, it contributes to two principal strands of literature. First, it makes an important contribution to the literature on the relationship between foreign aid and political institutions (e.g. Svensson, 2000; Tavares, 2003; Bräutigam and Knack, 2004; Knack, 2004; Djankov et al., 2008; Busse and Gröning, 2009; Okada and Samreth; 2012; Jones and Tarp, 2016), which to date has focused mainly on country level variation in aid and institutions.

Second, it contributes to the emerging literature evaluating sub-national effects of aid using geo-coded aid and outcome data (see e.g. Brazys et al., 2017; Civelli, et al., 2018; Isaksson and Kotsadam, 2018a,b; Kotsadam et al., 2018; Dreher et al., 2019). While rapidly growing, this literature has seen surprisingly few attempts to explore the effects of aid on institutional development. Isaksson and Kotsadam (2018a) find that Chinese aid stimulates local corruption in a sample of African countries, and Brazys et al. (2017) find that Chinese aid, unlike World Bank aid, fuels corruption in Tanzania. Furthermore, a couple of recent studies focus on the impact of aid on local institutional legitimacy in war-torn states. Based on panel data for around 5,000 Afghan villages, Parks et al. (2019) find evidence that aid responsive to citizen needs strengthens the legitimacy of local and district government. Similarly, Carnegie et al. (2019), focusing on Syria, find a positive relationship between aid inflows and perceptions of local institutions, except in areas dominated by an outside force (ISIS). The literature studying the effects of aid using geo-coded aid and outcome data has, however, yet to systematically explore the local effects of aid on institutional quality in a broad sample of countries.

Being the first to do so, this paper makes several more specific contributions. By studying local as opposed to country level aid and institutions we hope to be able to capture effects of targeted aid that may not be picked up at the country level. Moreover, while the aforementioned identification problems still apply – aid is not distributed at random, neither within nor across countries – the use of sub-national data improves the prospects for causal identification. In particular, geo-coded data on aid and institutional outcomes allow us to compare localities affected

and not affected by development projects – before and after development project implementation – while controlling for potential confounding and omitted variables at relatively fine geographic levels. Furthermore, focusing on citizens' willingness to abide by formal institutions should make us better able to capture de facto as opposed de jure institutions. And finally, by exploring effects of different forms of aid as well as donor heterogeneity in results, we address the concern that the aid effectiveness literature often aggregate over aid flows that, since provided for very different purposes, should have very different effects.

In the next section, we elaborate on the proposed theoretical mechanisms linking aid to political institutions, on the empirical evidence available to date, and the need for sub-national, disaggregated, analysis in the field.

2 Aid and political institutions

'Political institutions' is a broad concept that is often used interchangeably with 'governance' or 'quality of government'. We follow in this tradition and think of political institutions as "the traditions and institutions by which authority in a country is exercised" (Kaufmann et al., 2010, p. 4), which is the definition used by the World Bank when formulating their Worldwide Governance Indicators (WGI). The multi-dimensional nature of the concept is reflected in the theoretical and empirical discussions of the linkages between aid and institutions, which hardly suggest a simple, uni-directional effect of the former on the latter (Jones and Tarp, 2016).

2.1 Theoretical mechanisms and empirical evidence

Theoretically, the impact of aid on political institutions is inconclusive. According to one view, which can be summarized as 'aid as finance', aid helps simply by relaxing the budget constraint (Bourguignon and Gunning, 2016). Through the infusion of resources and technical assistance, aid can potentially boost government effectiveness (see e.g. the discussion in Bräutigam and Knack, 2004; Djankov et al., 2008; and Charron, 2011). It can release governments from binding revenue constraints, thereby enabling them to strengthen domestic institutions and pay higher salaries to civil servants, and it can provide training and technical assistance to build important government functions and institutions such as legal systems and accounting offices. Donors can potentially bring in expertise that may be lacking in developing countries facing severe capacity constraints.

According to another perspective, which can be thought of as 'aid as reform', aid can be used as an instrument for changing policies and institutions, by persuading states to embark on reform (Bourguignon and Gunning, 2016). Accountability could be enhanced due to international oversight, and conditionality measures stipulating that certain reforms must be in place to receive future aid may encourage institutional improvements.

A number of studies support these optimistic views on the potential of aid in building institutions. Goldsmith (2001) and Dunning (2004) find a positive relationship between aid and indicators of democracy and economic freedom in Africa. Tavares (2003) and Okada and Samreth (2012) find that receiving aid is associated with reduced corruption levels. The results of Charron (2011) suggest that the work of multilateral, but not bilateral, donors have helped combat corruption. Studying democratic transitions in Africa 1989-2008, Dietrich and Wright (2015) find that economic aid increases the likelihood of transition to multiparty politics, while democracy aid furthers democratic consolidation. The findings of Jones and Tarp (2016) suggest a small positive net effect of total aid on political institutions, primarily driven by stable inflows of governance aid.

Others, however, argue that aid undermines local institutions by promoting rent-seeking behavior and by reducing the incentives for democratic accountability and thus the democratic pressures to build effective institutions (e.g. Easterly, 2006; Deaton, 2013). Fiscal contract theory (e.g. Dietrich, et al.2017; Baldwin and Winter, 2020) predicts that when revenues do not depend on the taxes raised from citizens and business, there is less incentive for accountability. Citizens who pay taxes can threaten to withhold those taxes if the government does not fulfill its role in providing public goods and social services. In contrast, where citizens are not the main financiers of government, citizens supposedly have less leverage over government, and governments have less incentive to respond to their citizenry (Baldwin and Winter, 2020). As in the 'resource curse' literature, linking natural resource rents to weaker government accountability (Djankov et al., 2008; Morrison, 2012), foreign aid provides a windfall of resources to recipient countries, and may result in the same rent-seeking behavior.

And as it turns out, there is empirical evidence to support these negative predictions on the effects of aid on institutional development too, with studies suggesting that large aid inflows stimulate corruption (Svensson, 2000; Bräutigam and Knack, 2004) and worsen democratic institutions (Djankov et al., 2008) and other governance outcomes (Busse and Gröning, 2009).

Another concern is that aid contributes only to shallow institutional reform (Pritchett el a., 2013; Buntaine et al., 2017). When donors make access to financing contingent upon achievement of performance targets, recipient countries may choose easy and shallow institutional targets that signal commitment to institution-building, what Buntaine et al. (2017) refer to as 'form targets', rather than 'function targets' capturing effectiveness at addressing public problems. An analysis of post-project evaluations of World Bank projects, suggest that countries that receive concessional (IDA as opposed to IBRD) financing based on performance criteria are indeed less likely to report function targets.

Several studies highlight the heterogeneous effects of aid on institutions. The extent to which there is a 'political aid curse' is often suggested to depend on the strength of democratic institutions in the recipient country to begin with (see e.g. Bueno de Mesquita and Smith, 2010; Ahmed, 2012; Faye and Niehaus, 2012; Dutta et al., 2013), as well as on the degree of aid fungibility, in turn relating to conditions imposed by donors (Collier, 2006; Altincekic and Bearce, 2014; Jones and Tarp, 2016). The findings of Ear (2007), who disaggregate aid depending on its share of technical assistance and on its grant element, suggest that different forms of aid have different effects on different institutional outcomes at the country level. Exploring the intermediary role of institutions in the aid-growth relationship for a panel of African countries, Wako (2018) find significant donor heterogeneity.

2.2 Empirical takeaways

Given the conflicting theoretical mechanisms discussed above, it is reasonable to view the empirical impact of aid on institutions as a net effect, the sign of which is ambiguous a priori (Jones and Tarp, 2016). If the optimistic 'aid as finance' and 'aid as reform' mechanisms outweigh possible negative effects from rent-seeking and weakened democratic accountability, we would expect the overall effect of aid on institutions to be positive. If the reverse holds, however, it should instead be negative.

In light of the conflicting theoretical predictions and inconclusive empirical findings at the country level, a main takeaway from the above discussion concerns the importance of evaluating heterogeneity. The literature points to important heterogeneity across types of aid and institutions, as well as across donors. Against this background, it seems reasonable to explore sub-national

variation in aid and institutions. This enables us to capture effects of regionally targeted aid that may not be picked up at the country level, and to explore variation across donors and types of aid.

Another takeaway concerns the importance of capturing de facto as opposed de jure institutions, or put differently, to go beyond form targets that do not necessarily translate into effectiveness at addressing problems experienced by citizens.

With this is mind, we construct a proxy for local institutional quality based on citizens' expressed willingness to abide by key formal institutions. The idea is that this measure should capture confidence in, and thus perceived quality of, institutions. Just like country level institutional indices based on expert judgements (e.g. the aforementioned WGI), the indicator is based on perceptions, i.e. subjective accounts as opposed to objective criteria. Unlike country level indices of institutional development, however, this variable arguably enables us to get picture of de facto institutional quality – as experienced by citizens – at the local level. Importantly, perceived institutional quality is what should matter for economic behavior.

The distinction between national and local institutions is by no means clear-cut. Citizen perceptions of institutional quality will in all probability to some extent pick up media reports covering national institutions. Nonetheless, studying local institutional outcomes is clearly relevant, especially in a developing country context where the reach of central government institutions is often limited in remote rural areas. While de jure institutional arrangements may be formulated nationally, de facto implementation often takes place locally, and citizen assessments of whether institutions deliver will likely depend heavily on own experiences and the experiences of friends and family in the surrounding area.

That said, focusing on citizen reports also brings challenges. In particular, one may be concerned that perceptions of institutional quality are susceptible to signaling effects of receiving aid. These potential effects, too, are ambiguous. In line with fiscal contract theory, discussed above, a significant donor presence may signal that the government is performing badly, since it ostensibly has been unable to finance the concerned development project on its own, via taxes. Alternatively, however, citizens might credit their local authorities for bringing a project into their community. Indeed, the presence of externally funded projects may be seen as signals of the competence of local governments in providing goods and services to its community (Dietrich, et al.2017)

In an attempt to disentangle such signaling effects from de facto institutional improvements, we will compare effects of aid projects at different points in the project implementation cycle. The idea is that signaling effects are likely to be most pronounced when donors are visible, under the project implementation phase, and that real institutional development is a slow process, taking some time to materialize.

In sum, we thus focus on the local effects of aid projects at different stages of implementation on perceived institutional quality in African aid receiving countries, comparing the effects of overall aid with those of aid targeted specifically at institutional development, and evaluating donor heterogeneity in results. In the next section, we discuss how to approach these issues empirically.

3 Data and empirical strategy

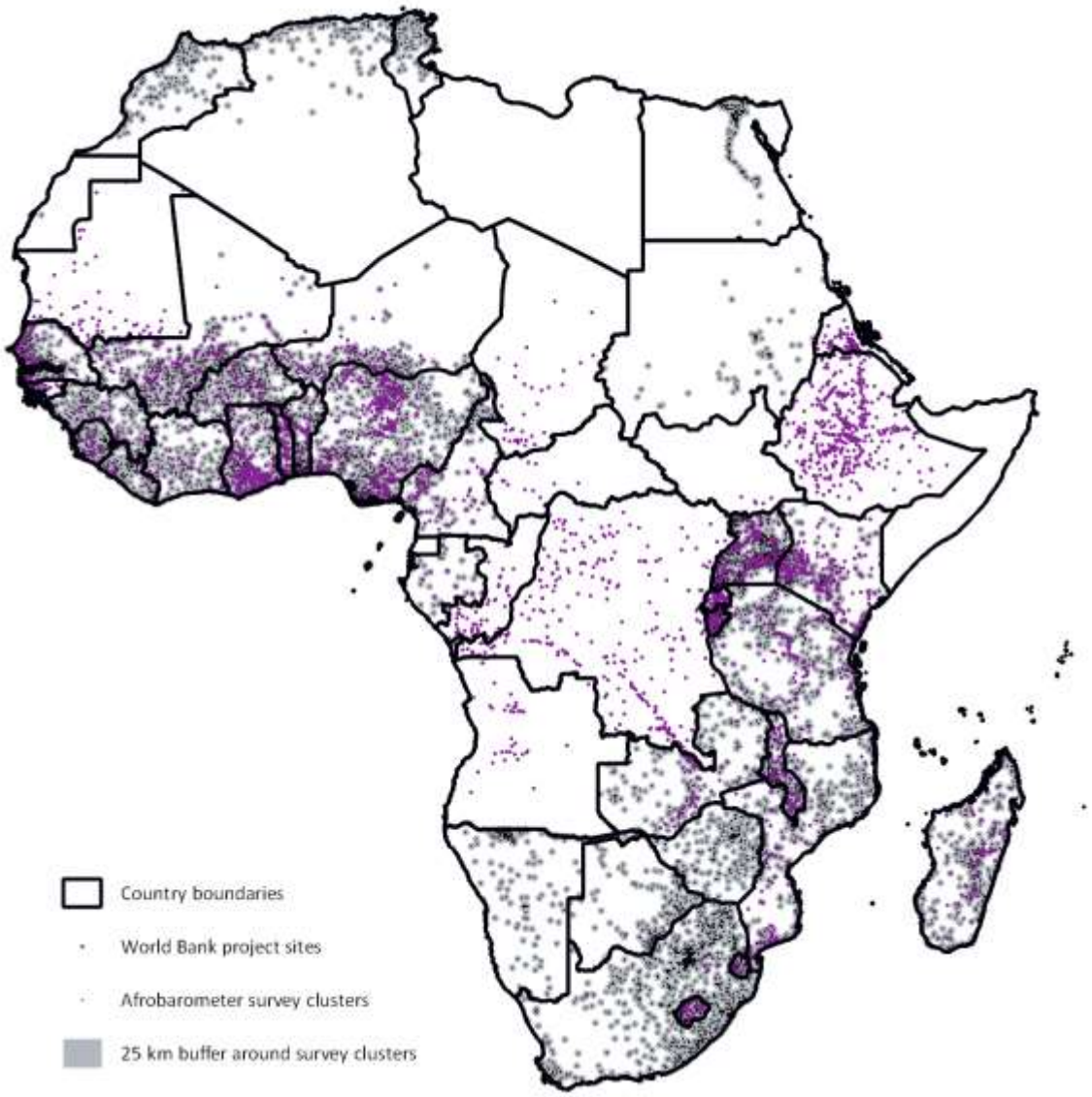
To explore the local effects of aid on perceived institutional quality in Africa, we geographically match spatial data on World Bank aid projects to the continent over the period 1995-2014 with survey data for a large sample of respondents from a broad range of African countries,[2] interviewed between 2002 and 2015. The survey data is obtained from rounds 2-6 of the Afrobarometer survey (geocoded by BenYishay et al., 2017). The aid project data is from AidData's World Bank Geocoded Research Release, Version 1.4.2 (AidData, 2017). Focusing on World Bank aid has the advantage that they routinely geocode all their projects. For donor comparisons, however, we will focus on geocoded projects of selected bilateral donors in selected African recipient countries with sufficient data coverage (also from AidData).

The aid data contains latitude and longitude project co-ordinates, and provide information about the precision of the location identified (AidData Research and Evaluation Unit 2017). Being interested in the local effects of aid projects, we focus on projects with recorded locations coded as corresponding to an exact location or as 'near', in the 'area' of, or up to 25 km away from an exact location.[3] We then use the coordinates of the surveyed Afrobarometer clusters, consisting of one or several geographically close villages or a neighborhood in an urban area, to match individuals to aid project sites with precise point coordinates. Specifically, we measure the distance

[2] For the exact estimation sample we get after sample restrictions, see below.
[3] Precision categories 1 and 2 in Strandow et al. (2011).

from the cluster centre points to the aid project sites and identify the clusters located within a cut-off distance – in the benchmark setup 10 km – of at least one project site. Figure 1 maps the World Bank projects along with the Afrobarometer survey clusters encircled by buffer zones, illustrating how we identify project sites within a cut-off distance of survey clusters.[4]

Figure 1: Map of project sites and survey clusters surrounded by 25 km buffer zones [IN COLOUR]



Our main outcome variable focuses on citizens' expressed willingness to abide by a range of key state institutions. Specifically, our dependent variable is an index created as the first principal

[4] We use a 25 km cut-off in the figure for better visibility of the buffer areas.

component (with mean of zero and a standard deviation of one) of the responses to questions on whether the respondents agree or disagree that a) the courts, b) the police and c) the tax authority has the right to make decisions that people always have to abide by, with response categories ranging from 1 for strongly disagree to 5 for strongly agree. These questions have the advantage of broad coverage, both across countries and over time. The idea is that the resulting index should capture confidence in, and thus perceived quality of, key institutions. We deliberately focus on institutions not explicitly connected to any political party, or the executive branch of government, in order to capture perceived institutional quality rather than political alliances and satisfaction with the government.

The main explanatory variables, which will be described in greater detail below, focus on living near a World Bank project site – either a site where a project is being implemented at the time of the survey (*Ongoing*), a site where a project has been completed (*Completed*), or a site where a project will be opened but where implementation had not yet been initiated at the time of the survey (*Future*).

We restrict the sample to countries with a sufficient number of observations connected to both completed, ongoing and future project sites. This gives a benchmark estimation sample of 73,640 respondents across 12 countries.[5] Our sample countries contain 2641 ongoing, future or completed World Bank project sites with precise geocodes and information on start-date. The estimation strategy, described further below, will rely on identifying survey respondents within a specified distance – 10 km in the benchmark estimation – of project sites, i.e. in clusters where a completed, ongoing or future project lies within the specified cut-off distance.

In a first set of estimations, we consider overall aid, i.e. any World Bank development project irrespective of sector. This is in line with cross-country studies considering the relationship between aggregate aid flows and institutional outcomes. Bearing in mind that aid is given for many purposes other than institutional development, however, in a second set of estimations we restrict our attention to World Bank projects targeting the sector 'Public Administration, Law, and Justice' (260 out of 2641 project sites). While we cannot be sure that all projects in this sectoral classification are targeted at institutional development, it should narrow down the spread of project objectives to some extent.

[5] Benin, Burkina Faso, Cape Verde, Kenya, Lesotho, Madagascar, Malawi, Mali, Mozambique, Nigeria, Tanzania and Uganda.

Variable descriptions and summary statistics are presented in Tables A1 and A2 in the Appendix.

3.1 Estimation strategy

Just like the distribution of aid across countries, the distribution of aid within countries is not random, implying that some individuals and sub-national areas, with certain characteristics, will be more likely than others to be targeted by aid. For instance, donors may allocate aid to areas that stand out in terms of pre-existing infrastructure and institutional arrangements. Hence, assuming that there is no relationship between project localization and the pre-existing characteristics of project sites and of the population residing in the surrounding areas is not reasonable.

In order to deal with these empirical challenges, we use a spatial-temporal estimation strategy.[6] In particular, rather than simply comparing people living close to and far away from project sites, we compare the estimated effects of living near sites where at some point a World Bank project has been, is or will be implemented. While the fact that the Afrobarometer is not a panel hinders us from following specific localities over time, before and after a project was initiated and later completed, with this estimation strategy, we can still make use of the time variation in the data

As such, we compare four groups of individuals: 1) those within 10 km of at least one ongoing project site (*Ongoing,* applying to around 30 percent of respondents), 2) those within 10 km of a site where a project has been completed prior to the interview date (*Completed*, applying to approximately 20 percent of respondents),[7] 3) those within 10 km of a site where a project will start, but where implementation was yet to begin at the survey date and not close to any ongoing projects (*Future,* applying to around 7 percent of respondents), and 4) those living more than 10 km away from any project site (the omitted reference category in the regressions, applying to 59 percent of respondents). The baseline regression takes the form:

[6] Resembling that in Knutsen et al. 2017. See also Isaksson and Kotsadam (2018a and 2018b).

[7] Note that ongoing and completed are not mutually exclusive categories. In particular, many who have ongoing projects nearby also have completed projects within the cut-off distance. Hence, these shares will not sum to one. In alternative estimations we break these treatment categories into three groups: those with only ongoing projects within the cut-off distance, those with both ongoing and completed projects within the cut-off distance, and those with only completed projects within the cut-off distance.

$$Y_{ivt} = \beta_1 Ongoing_{vt} + \beta_2 Completed_{vt} + \beta_3 Future_{vt} + \alpha_s + \delta_t + \gamma \cdot \mathbf{X}_{it} + \varepsilon_{ivt}$$

where the institutional outcome *Y* for an individual *i* in cluster *v* at year *t* is regressed on a dummy variable *Ongoing* capturing whether the individual lives within the specified cut-off distance of an ongoing World Bank project, *Completed* capturing whether s/he lives within the cut-off distance of a World Bank project that has been completed prior to the interview date, and a dummy *Future* for living close to a site where a World Bank project will take place but had not yet implemented at the time of the survey. To control for variation in average levels of perceived institutional quality across time and space, the regressions include country (and in alternative estimations region) fixed effects (α_s) and year fixed effects (δ_t). To control for individual variation in perceptions of institutional quality, a vector ($\mathbf{X}_i$) of individual-level controls from the Afrobarometer are included. The baseline set of individual controls are age, age squared, gender and urban/rural residence.[8] To account for correlated errors, the standard errors are clustered at the geographical clusters (i.e., at the enumeration area level).

The coefficients on *Ongoing* (β_1) and *Completed* (β_2) capture any causal effects of having an ongoing or completed project nearby, plus potential selection effects. The coefficient on *Future* (β_3), on the other hand, captures only a selection effect (since the project has not started yet and so should not have a causal impact). The idea is that by taking the difference between *Ongoing* and *Completed*, on the one hand, and *Future* on the other, we subtract the selection effect from the combined selection and causal effect, leaving behind the causal effect of ongoing and completed aid projects on the institutional outcome variable of interest. The parameter difference between *Ongoing* and *Future* ($\beta_1 - \beta_3$) and between *Completed* and *Future* ($\beta_2 - \beta_3$) thus give a difference-in-difference type of measure that controls for unobservable time-invariant characteristics that may influence selection into being a World Bank project site.

[8] Table A3 explores sample balance for the pre-treatment and treatment groups along these dimensions. Whereas people living close to World Bank projects tend to be somewhat younger than those with no World Bank project near them, there is no statistically significant age difference between the treatment and pre-treatment groups. Neither is there any gender imbalance between the groups. Next, people with World Bank projects nearby – whether ongoing, completed or future – are all more likely to live in urban areas. For overall World Bank aid, this tendency is slightly more pronounced for the treated groups. For World Bank aid targeted at institutional development the tendency is instead somewhat more marked in the pre-treatment group.

The key assumption behind this approach is that the selection process relevant for ongoing, completed and future projects sites is the same. A potential concern would be if completed/ongoing/future project status picks up project timing and projects starting later differ systematically from projects starting earlier. Here it is important to note that there is no direct correspondence between when a project was implemented and whether it is coded as *Completed*, *Ongoing* or *Future*; the classification depends on project status at the time the Afrobarometer survey covered the particular area in question. That said, however, there is an over-representation of respondents connected to ongoing projects in the later survey waves, why we will evaluate possible effects of project timing in the sensitivity analysis.

Using the above approach to study whether World Bank aid projects affect local institutional quality, one has to make an assumption about the geographical reach of the potential effect. How far from project sites citizens experience its potential rewards is essentially an empirical question. We use a 10 km cut-off in the benchmark estimation, but evaluate alternative cut-offs for comparison.

4 Results

4.1 Overall World Bank aid

Let us first consider the results of estimations focusing on overall World Bank aid, i.e. the impact of living near any World Bank project, regardless of whether their focus is on institutional development or not. The results indeed indicate that World Bank aid projects positively impact reported confidence in institutions, albeit subject to two important qualifiers: the observed effect appears relatively local and takes some time to materialize.

Table 1 presents the results using a 10 km cut-off. To begin with, the estimations demonstrate the importance of taking the non-random selection of World Bank project sites into account. The coefficient on *Future* is consistently negative and statistically significant, suggesting that World Bank projects tend to be located in areas where citizens had lower confidence in institutions prior to project implementation. To account for this tendency, we subtract the parameter on *Future* from that of *Ongoing* and *Completed*, respectively. The concerned parameter differences ($\beta_{\text{ongoing}} - \beta_{\text{future}}$ and $\beta_{\text{completed}} - \beta_{\text{future}}$) and associated test results are presented in the bottom rows of Table 1.

Table 1: The impact of World Bank aid projects, irrespective of sector, on perceived institutional quality within a 10 km cut-off. Dependent variable is *inst_index.*

VARIABLES	(1) Benchmark	(2) Region FEs	(3) Geo-matching	(4) Exclude wave 6	(5) Future within 5	(6) Country-year FEs	(7) Project FEs	(8) Control for co-ethnic with president	(9) Treatment sub-categories	(10) Completed vs. future sample	(11) Completed vs. future sample
Any ongoing	-0.027	-0.012	-0.025	-0.056***	-0.026	-0.043***	0.032	-0.004			
	(0.019)	(0.016)	(0.019)	(0.018)	(0.019)	(0.017)	(0.035)	(0.024)			
Any completed	0.015	0.031*	0.015	-0.003	0.015	0.032*	0.072	-0.006		0.044	
	(0.020)	(0.017)	(0.020)	(0.019)	(0.020)	(0.017)	(0.058)	(0.023)		(0.034)	
Future	-0.070**	-0.048*	-0.066**	-0.082***	-0.085**	-0.049**		-0.100***	-0.071***		
	(0.027)	(0.025)	(0.027)	(0.025)	(0.033)	(0.023)		(0.038)	(0.027)		
Only ongoing									-0.032		
									(0.024)		
Ongoing and completed									-0.009		
									(0.018)		
Only completed									0.002		0.099**
									(0.025)		(0.045)
Diff ongoing-future	0.0427	0.0363	0.0414	0.0253	0.0591	0.00573		0.0962			
F test ongoing-future=0	1.637	1.846	1.544	0.909	2.064	0.0490		4.487			
P value of F test	0.201	0.174	0.214	0.340	0.151	0.825		0.0342			
Diff completed-future	0.0845	0.0797	0.0816	0.0786	0.0991	0.0812		0.0946			
F test compl-future=0	7.584	7.400	7.031	6.065	8.824	8.095		5.27			
P value of F test	0.00591	0.00654	0.00803	0.0138	0.00298	0.00445		0.0218			
Observations	73,640	73,640	72,525	55,137	73,640	73,640	24,818	53,750	73,640	19,850	8,607
R-squared	0.030	0.075	0.031	0.028	0.030	0.050	0.104	0.027	0.030	0.041	0.036

Robust standard errors (clustered by survey cluster) in parentheses; *** p<0.01, ** p<0.05, * p<0.1; All regressions include baseline controls and year and country (or region, when specified) fixed effects; Column 3 excludes all respondents in enumeration areas further than 100 km away from any World Bank projects site; Column 4 excludes Afrobarometer survey wave 6 (which contains no observations connected to future project sites); Column 5 restricts the pre-treatment group to respondents living close to sites where projects will start within a maximum of five years of the interview date; Column 6 controls for country-year fixed effects; Column 7 restricts the sample to respondents in enumeration areas that the Afrobarometer (despite not having a panel structure) happened to revisit before and after a project was initiated or completed, allowing for the inclusion of project fixed effects (note that there is no need to compare with sites of future projects in this setup). Column 8 introduces a control for the respondent belonging to the same ethnic group as the country president at the time of the survey. The coefficient on the control (not presented) is 0.06, statistically significant at the 1 percent level; Column 9 breaks down the treatment groups (ongoing and completed) into sub-categories specifying if the respondent lives within the cut-off distance of only ongoing projects, both ongoing and completed projects, or only completed projects. The difference onlyongoing-future= 0.038, with p-value 0.283, the difference ongoingandcompleted-future=0.062, with p-value 0.0365, the difference onlycompleted-future=0.073, with p-value 0.0367; Columns 10 and 11 focus on a restricted sample consisting only of respondents living within the cut-off distance of a future or a completed World Bank project. Column 10 focuses on the parameter of having any completed project nearby (and includes respondents living in areas close to both a completed and an ongoing project). Column 11 focuses on the parameter of having only completed projects in the vicinity, and thus restricts the sample further (excluding also respondents living in areas close to both a completed and an ongoing project).

People living within 10 km of with a completed project site report significantly higher confidence in institutions than people living within the cutoff distance of a future project site (the Column 1 parameter difference $\beta_{\text{completed}} - \beta_{\text{future}}$ is equivalent to 8.5 percent of a standard deviation on the institutional index). Interestingly, and in line with the idea that institutional change is a slow process, the results do not suggest an equivalent effect of having an ongoing project in the vicinity. The estimated parameter on *Ongoing* is less negative than that on *Future* ($\beta_{\text{ongoing}} - \beta_{\text{future}}$ is positive) but the difference is not statistically significant.

This pattern is consistent across a wide range of alternative specifications. The benchmark estimation controls for variation in average levels of perceived institutional quality across countries. Central to our argument, however, institutional quality likely varies systematically within as well as across countries, depending on differences in local governance and institutional infrastructure. Controlling for sub-national variation involves a trade-off between losing variation and controlling for potential sources of bias. While sub-national fixed effects may absorb variation in institutional quality that is in fact due to aid, i.e. variation that we want our aid indicators to pick up, they also help control for regional variation unrelated to aid, that would otherwise bias our aid estimates. Reassuringly, including, sub-national region fixed effects (Column 2) does not change the interpretation of results.

Furthermore, the results are robust to dropping all respondents in enumeration areas further than 100 km away from World Bank projects sites (Column 3). This form of geographical matching is useful since it controls for unobserved factors that may vary within administrative sub-national regions (Briggs, 2019), thus helping to make the pre-, ongoing- and post-treatment groups more comparable with the no-treatment group.

As discussed in Section 3.1, a potential concern would be if future/ongoing/completed project status picks up project timing and projects starting later differ systematically from projects starting earlier. As noted, there is no direct correspondence between time of project implementation and project status. A project implemented comparatively early may well be coded as a future project, all depending on at what point in time the Afrobarometer surveyed that particular area. That said, however, there is an over-representation of respondents connected to ongoing and completed project sites in the later survey waves. And at the time of wave 6, all projects included in the dataset had already been initiated, meaning that there are no respondents connected to future project sites

in this round. Including wave 6 comes with the benefit of a significantly larger sample. Reassuringly, however, excluding observations from wave 6 (Column 4) the observed pattern remains.

In the benchmark setup, the variable *Future* captures respondents living close to a site where we know that a World Bank project will be implemented at a later stage. It places no restriction on how far ahead of the survey date project implementation starts. A potential concern is that circumstances in the area may change between survey date and project start, affecting the comparability of the treatment (ongoing and completed) and pre-treatment groups. The estimation in Column 5 restricts the pre-treatment group to respondents living close to sites where projects will start within a maximum of five years of the interview date (which applies for 67 percent of the concerned group). The main results remain unchanged. While considering an even narrower time bandwidth may further improve comparability, it comes at the cost of having to extrapolate from a small pre-treatment group.[9] With this caveat in mind, we can note that the results hold for varying the time restriction, from future projects starting within five years of the interview data to future projects starting within a year (Figure A1).

In the benchmark setup, country and year fixed effects account for variation in average levels of perceived institutional quality across time and space. However, time trends in institutional quality (and perceptions thereof) are likely to vary across countries, e.g. due to reforms. Reassuringly, the benchmark result withstands controls for country-year fixed effects (Column 6).

As noted, the Afrobarometer is not a panel, and only in some cases happens to revisit the same localities in different survey waves. Column 7 restricts the sample to respondents in enumeration areas that the Afrobarometer happened to revisit before and after a project was initiated or completed, allowing for the inclusion of project fixed effects. The advantage of using project fixed effects is that it allows us to evaluate variation in citizens' willingness to abide by institutions in the same location over time, before and after the World Bank implemented a project in the area (without having to compare with future project sites). This, however, means we lose a large share

[9] In the benchmark estimation, with no time restriction, 6.5 percent of respondents live within 10 km of a future project site (and not close to any ongoing projects). With a five year cut-off this share goes down to around 4.3 percent, and with a one year cut-off it is below 1 percent.

of our sample. Nonetheless, the estimated parameters follow the expected pattern, albeit less precisely estimated.

A common argument is that African policy-makers tend to favour their own homelands and ethnic groups in the allocation of funds (see e.g. Wantchekon 2003; Alesina et al. 2016; Isaksson, 2020). Against this background, one may suspect that co-ethnics of the president are more pleased with current economic and political conditions – potentially spilling over on a greater willingness to abide by state institutions – than are members of other groups. Moreover, taking into account that members of the same ethnic group often live geographically clustered, we cannot rule out that co-ethnics of the president also differ systematically in terms of aid exposure, and hence that ethnic affiliations could bias our estimates. The estimation presented in Column 8 introduces a control variable for the respondent belonging to the same ethnic group as the country president at the time of the survey. Reassuringly, the key results remain unchanged.[10]

In the benchmark setup we focus on dummies for having any ongoing or any completed project within the cut-off distance. These categories are not mutually exclusive; a respondent might well have both ongoing and completed projects in their vicinity. To get a more detailed picture of the effects of living near projects at different stages of the implementation process, Column 9 breaks down these treatment categories into three groups: those with only ongoing projects within the cut-off distance, those with both ongoing and completed projects within the cut-off distance, and those with only completed projects within the cut-off distance. In line with the benchmark results, and with idea that institutional change takes time, there is no statistically significant difference in scores on our institutional index between people living near future projects and people living near only ongoing projects. The parameters on the treatment variables capturing greater project maturity – having both ongoing and completed projects in the vicinity, or having only completed projects within the cut-off distance – are on the other hand both significantly larger than that on living near a future project. While as expected, the estimated parameter on living near only completed projects is larger than that of living near both ongoing and completed projects, the difference between the two is not statistically significant.

[10] As may be expected, co-ethnics of the president express a greater willingness to abide by the concerned institutions (the co-ethnic dummy parameter, not presented in the table, is approximately 0.06, and statistically significant at the one percent level).

Columns 10 and 11 focus on a restricted sample consisting only of respondents living within the cut-off distance of a completed or a future World Bank project site. Hence, rather than comparing the estimated effects in relation to those not living close to any project and subtracting the selection effect from our treatment effect, as in the benchmark setup, we here directly compare respondents with a completed or future project within the cut-off distance. Considering that our results thus far only suggest a positive impact of completed projects, we omit respondents connected to ongoing projects from this comparison. Column 10 focuses on the parameter of having *any* completed project nearby (and includes respondents living in areas close to both a completed and an ongoing project). Column 11 focuses on the parameter of having *only* completed projects in the vicinity, and thus restricts the sample further (excluding respondents living in areas close to both a completed and an ongoing project). Both come out positive, but the latter is more precisely estimated, again in line with the idea that institutional change takes time.

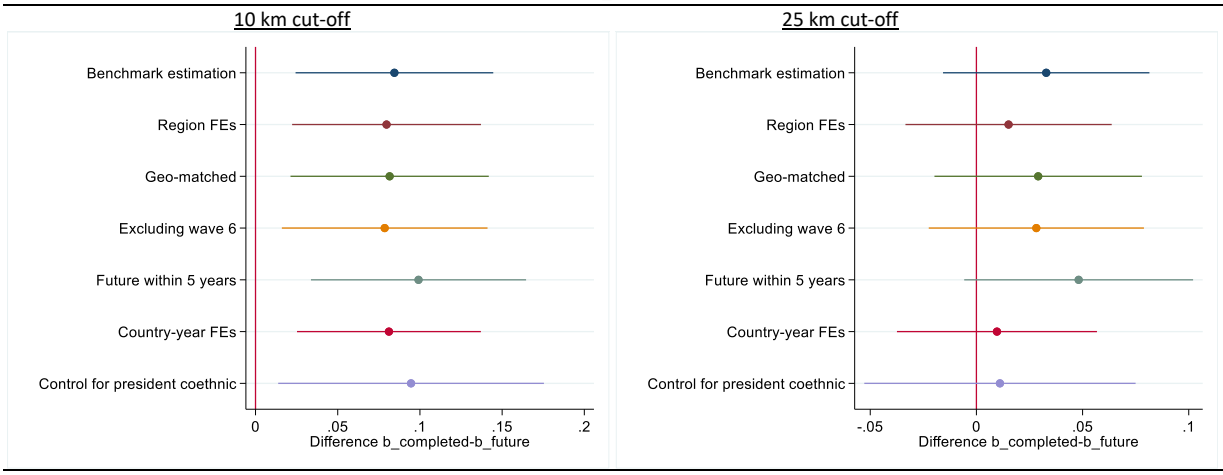
Furthermore, the key results remain unchanged when specifying our dependent variable differently (see Table A4 in the Appendix). First, using an additive institutional index rather than the original index based on principal component analysis does not change the results. And similarly, breaking down the institutional index into its component variables (the expressed willingness to abide by the courts, the police and the tax authority, respectively) and running separate regressions with these variables as dependent variable does not alter our main findings. The difference between people living near completed and future project sites ($\beta_{\text{completed}} - \beta_{\text{future}}$) is consistently positive and, in all but one estimation,[11] statistically significant.

As seen in Table 1, the positive effect of living within 10 km of completed as compared to future projects is consistent across a wide range of specifications. As it turns out, this effect is less precisely estimated when considering a wider geographical cut-off – 25 km instead of 10 km – around project sites. We summarize the estimated parameter differences between having a completed and a future World Bank project within a 10 and 25 km cut-off, respectively, in Figure 2 (a detailed presentation of the 25 km cut-off results can be found in Table A5). While $\beta_{\text{completed}} - \beta_{\text{future}}$ is consistently positive, it is only weakly statistically significant when restricting the pre-treatment group to respondents living close to sites where projects will start within a maximum of five years of the interview date. When focusing on a restricted sample

[11] The one focusing on the expressed willingness to abide by court decisions.

consisting only of respondents living within the cut-off distance of a future or completed World Bank project, however, the parameter on having only completed projects nearby is indeed positive and statistically significant (Column 11, Table A5).

Figure 2: Estimated effects of overall World Bank aid. Dep. var. is *inst_index* [IN COLOUR]



Notes: Estimated effect with 95% confidence intervals; The estimations correspond to those presented in Tables 1 and AX, but excluding the estimations not focusing on a the parameter difference between *any_completed* and *future* (i.e. the project fixed effect estimation, the estimation where we break down the 2 treatment groups into 3 sub-treatment-categories, and the estimations restricting the sample to respondents living near completed and future project sites).

In sum, the results from this set of estimations indicate that even aid projects not necessarily targeted at institutional development come with measurable benefits for local institutions. The observed effect concerns finalized projects, not projects still under implementation, however, and is precisely estimated only for a narrowly defined geographic area around project sites. The fact that we see no effect of projects during their implementation phase is in line with the idea that institutional change is a slow process. However, it could also reflect negative signaling effects of aid during the project implementation phase. As noted, according to fiscal contract theory, a significant donor presence may signal that the government is performing badly, since apparently it has been unable to finance the concerned development project on its own, via taxes. Wanting to disentangle such signaling effects from de facto institutional improvements, it is useful to compare effects of aid projects at different points in the project implementation cycle. In particular, considering that signaling effects are likely to be most pronounced when donors are visible, i.e. during the project implementation phase, we are more confident that improvements observed post-

project implementation, when the donor is not present in the area, capture real institutional developments.

Bearing in mind that aid is given for many purposes other than institutional development, and that such aid projects may obscure the effect of projects actually targeted at institution building, in the next section we will restrict our attention to projects focusing on institutional development more directly.

4.2 Aid projects targeted at institutional development

As it turns out, the estimated effects when focusing on World Bank aid more directly targeted at institutional development (Table 2, Figure 3, Table A6) are indeed stronger. Again, the coefficient on *Future* is consistently negative and statistically significant, suggesting that World Bank projects targeted at institutional development tend to be located in areas with lower perceived institutional quality prior to project implementation. And again, it is the effect of completed rather than ongoing projects that stands out. People with a completed project within the cut-off distance consistently report significantly higher willingness to abide by the concerned state institutions than people living within the cut-off distance of a future project site (the parameter difference $\beta_{\text{completed}} - \beta_{\text{future}}$ in the benchmark estimation in Table 2 is equivalent to 13 percent of a standard deviation on the institutional index).

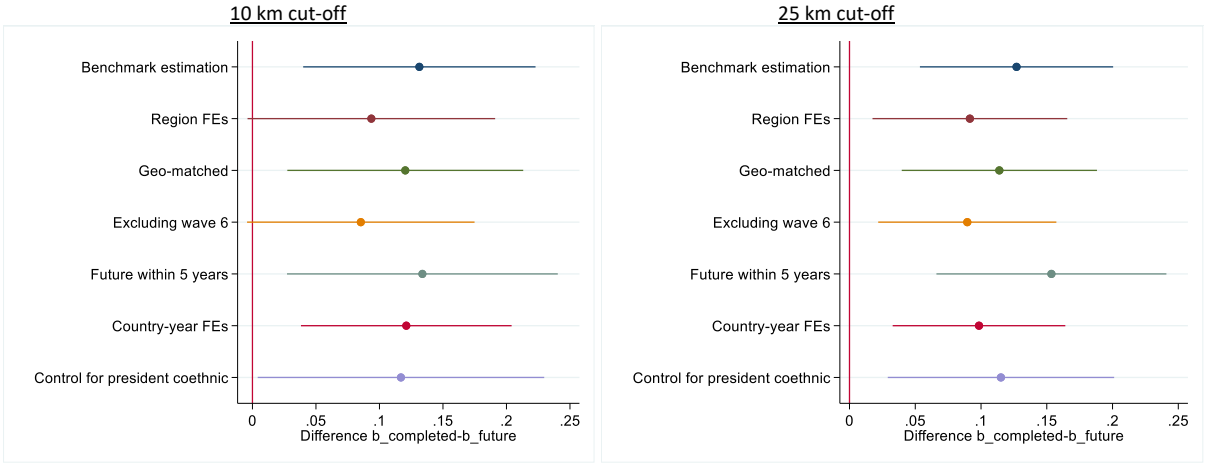
Not only are the results consistent across all specifications discussed in the previous section, i.e. all specifications in Table 2 as well as the estimations using different time restrictions on the pre-treatment group (Figure A1) and those specifying our dependent variable differently (Table A4), the effect of completed projects also has a larger geographical reach. As seen in Tables A6 and Figure 3, the results are consistent when using a 10 and 25 km cut-off. In addition, they display the same pattern when using a 50, 75 and 100 km cut-off (Figure 4).

Table 2: The impact of World Bank aid projects targeted at institutional development on perceived institutional quality within a 10 km cut-off. Dependent variable is *inst_index*

VARIABLES	(1) Benchmark	(2) Region FEs	(3) Geo-match	(4) Exclude wave 6	(5) Future within 5	(6) Country-year FEs	(7) Project FEs	(8) Control for president co-ethnic	(9) Treatment sub-categories	(10) Completed vs future sample	(11) Completed vs future sample
Any ongoing	-0.044	-0.018	-0.043	-0.014	-0.040	-0.012	-0.028	-0.070			
	(0.036)	(0.036)	(0.036)	(0.038)	(0.036)	(0.029)	(0.110)	(0.047)			
Any completed	0.054	0.042	0.047	0.003	0.055*	0.031	0.314**	0.075**		0.132**	
	(0.033)	(0.033)	(0.034)	(0.035)	(0.033)	(0.031)	(0.155)	(0.037)		(0.063)	
Future	-0.077**	-0.051	-0.074**	-0.082***	-0.079*	-0.090***		-0.042	-0.072**		
	(0.031)	(0.034)	(0.032)	(0.031)	(0.041)	(0.031)		(0.043)	(0.031)		
Only ongoing									-0.006		
									(0.039)		
Ongoing and completed									-0.035		
									(0.043)		
Only completed									0.104**		0.117*
									(0.045)		(0.070)
Diff ongoing-future	0.0333	0.0335	0.0303	0.0679	0.0384	0.0785		-0.0287			
F test ongoing-future=0	0.590	0.605	0.473	2.241	0.609	3.761		0.225			
P value of F test	0.442	0.437	0.492	0.135	0.435	0.0525		0.635			
Diff completed-future	0.131	0.0936	0.120	0.0853	0.134	0.121		0.117			
F test compl-future=0	7.933	3.546	6.453	3.490	6.057	8.200		4.13			
P value of F test	0.00488	0.0598	0.0111	0.0618	0.0139	0.00421		0.0422			
Observations	47,705	47,705	40,637	35,057	47,705	47,705	7,789	35,332	47,705	5,419	3,656
R-squared	0.025	0.058	0.024	0.022	0.024	0.037	0.098	0.022	0.025	0.033	0.047

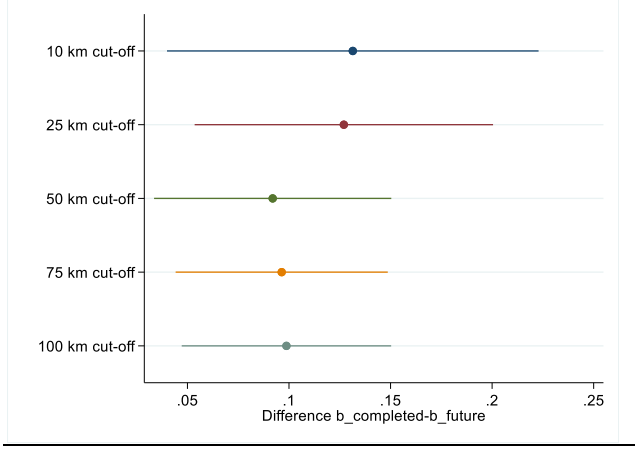
Robust standard errors (clustered by survey cluster) in parentheses; *** p<0.01, ** p<0.05, * p<0.1; All regressions include baseline controls and year and country (or region, when specified) fixed effects; Column 3 excludes all respondents in enumeration areas further than 100 km away from any World Bank projects site; Column 4 excludes Afrobarometer survey wave 6 (which contains no observations connected to future project sites); Column 5 restricts the pre-treatment group to respondents living close to sites where projects will start within a maximum of five years of the interview date; Column 6 controls for country-year fixed effects; Column 7 restricts the sample to respondents in enumeration areas that the Afrobarometer (despite not having a panel structure) happened to revisit before and after a project was initiated or completed, allowing for the inclusion of project fixed effects (note that there is no need to compare with sites of future projects in this setup). Column 8 introduces a control for the respondent belonging to the same ethnic group as the country president at the time of the survey. The coefficient on the control (not presented) is 0.07, statistically significant at the 1 percent level; Column 9 breaks down the treatment groups (ongoing and completed) into sub-categories specifying if the respondent lives within the cut-off distance of only ongoing projects, both ongoing and completed projects, or only completed projects. The difference onlyongoing-future= 0.067, with p-value 0.1447, the difference ongoingandcompleted-future=0.037, with p-value 0.4658, the difference onlycompleted-future=0.176, with p-value 0.001; Columns 10 and 11 focus on a restricted sample consisting only of respondents living within the cut-off distance of a future or a completed World Bank project. Column 10 focuses on the parameter of having any completed project nearby (and includes respondents living in areas close to both a completed and an ongoing project). Column 11 focuses on the parameter of having only completed projects in the vicinity, and thus restricts the sample further (excluding also respondents living in areas close to both a completed and an ongoing project).

Figure 3: Estimated effects of institutional World Bank aid. Dep. var. is *inst_index* [IN COLOUR]



Notes: Estimated effect with 95% confidence intervals; The estimations correspond to those presented in Tables 2 and AX, but excluding the estimations not focusing on a the parameter difference between *any_completed* and *future* (i.e. the project fixed effect estimation, the estimation where we break down the 2 treatment groups into 3 sub-treatment-categories, and the estimations restricting the sample to respondents living near completed and future project sites).

Figure 4: The impact of institutional World Bank aid projects when using different geographical cut-offs. Dep. var. is *inst_index* [IN COLOUR]



Notes: Estimated effect with 95% confidence intervals

Hence, while the results indicate that even aid projects not necessarily targeted at institutional development come with measurable benefits for local institutions, the effects are indeed stronger, and with a wider geographical reach, when considering projects focusing more directly on

institution building.[12] The result suggesting that it is completed projects rather than projects still under implementation that come with positive effects remains, however, in line with the idea that institutional change is a slow process. Considering that signaling effects are likely to be most pronounced when donors are visible, i.e. during the project implementation phase, we are, as noted, more confident that improvements observed post-project implementation, when the donor is not present in the area, capture real institutional developments.

4.3 Alternative identification strategy: Coarsened exact matching

Our estimation strategy relies on the assumption that a similar selection process applies to sites where projects were ongoing, completed or not yet initiated at the time the survey. We evaluated possible effects of project timing above. Nevertheless, it is useful to compare the results we get in our benchmark setup with results obtained when relying on an alternative identification strategy, in particular considering the limited number of respondents connected to future project sites in some countries and survey waves. In this section, we instead use coarsened exact matching (CEM), with similar results.

The key goal of matching is to prune observations from the data so that the remaining data have better balance between the treated and control groups (Iacus et al., 2012). While the strategy is no magic bullet in terms of causal identification – just as standard regression frameworks, it relies on controlling for observable factors – matching has the advantage that it makes the potential lack of common support, or overlap in terms of covariates, between treatment and control group explicit (Isaksson, 2017). Using matching, we thus avoid drawing conclusions based on unreasonable extrapolations.

The basic idea of CEM, specifically, is to temporarily coarsen the data into substantively meaningful groups, then use exact matching on these coarsened data, and finally run the analysis on the original (un-coarsened) matched data (Iacus et al., 2012). The procedure has the advantage that it is transparent in the sense that the imbalance between the matched treated and control groups

[12] Table A7 presents results of estimations focusing on World Bank projects to all sectors *other than* institutional aid. Interestingly, we observe some positive impacts here as well. The results are in line with those focusing on overall aid, i.e. suggesting a positive effect of living near completed projects when focusing on the smallest geographical cut-off around project sites (10 km), but not when considering a wider area (25 km).

will not be larger than what is ex ante specified by the user, based in intuitive and substantive information.[13]

Table 3: Estimating the impact of World Bank aid projects using coarsened exact matching (CEM). Dependent variable is *inst_index*

VARIABLES	(1) CEM 10 km cut-off	(2) CEM 25 km cut-off	(3) CEM + geo-matching 10 km cut-off	(4) CEM + geo-matching 25 km cut-off
Panel A: World Bank projects, irrespective of sector				
Only completed	0.012		0.012	
	(0.017)		(0.017)	
Only completed		0.028*		0.028*
		(0.015)		(0.015)
Observations	56,515	58,161	55,684	57,273
R-squared	0.010	0.014	0.010	0.013
Panel B: World Bank projects targeted at institutional development				
Only completed	0.087***		0.092***	
	(0.025)		(0.026)	
Only completed		0.038*		0.037*
		(0.019)		(0.020)
Observations				
R-squared	28,886	31,825	24,963	27,221

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

We focus on only having completed projects within the cut-off distance, i.e. the treatment capturing greatest project maturity, for which we get clear results in the original setup. Table 3 presents the results. We can note that for overall World Bank aid (Panel A), the estimated effect of living near a completed project is positive, and statistically significant at the ten percent level when focusing on a 25 km cut-off around project sites (Column 2). For aid targeted at institutional development (Panel B), the results are again stronger, the estimated effect being larger and statistically significant at the one percent level when using 10 km geographical cut-off, and statistically significant at ten percent when using a 25 km cut-off. In Columns 3 and 4 we narrow

[13] In comparison, and as discussed in Iacus et al. (2012), approximate matching methods such as propensity score matching require the user to set the matching algorithm ex ante, and then check for balance ex post. That is, the user checks for balance after the algorithm is finished, and if balance is not satisfied, re-specifies the matching model and re-checks balance, etc. until obtaining an acceptable amount of balance. The process often needs to be repeated multiple times because any change in the matching algorithm (e.g. including higher order terms and interactions) may alter balance in unpredictable ways. CEM is more transparent. In this specific case, for instance, we matched exactly on the country dummies, and on the urban and female dummies, but coarsened the age indicator into age brackets and grouped the interview years by survey wave, and then matched exactly on this coarsened data.

the comparison groups further, applying CEM on a geographically matched sub-sample (excluding respondents in enumeration areas further than 100 km away from any World Bank projects site, as in Column 3 in Tables 1-2). The results do not change.

Hence, when using coarsened exact matching rather than our benchmark spatial-temporal estimation strategy, the results again seem to suggest that even aid projects not necessarily targeted at institutional development come with benefits for local institutions, but the effects are once again stronger and more robust when considering projects focusing more directly on institution building.

4.4 Comparing across donors

The geo-coded aid data at hand does not allow for systematic donor comparisons. Other donors do not routinely geo-code their aid, meaning that the data available is patchy, at best. What we can do, however, is explore patterns for selected donors in selected recipient countries. Table 4 presents results of estimations focusing on US aid to Uganda and on Chinese aid to Kenya, Malawi, Mali, Mozambique, Tanzania and Uganda.

To begin with, there is relatively good coverage of US aid to Uganda; after sample restrictions, we have data on 324 project sites across the country, out of which 68 can be said to target institutional development (judging from their sectoral classification). Columns 1-2 present the results of estimations focusing on overall US aid to Uganda and Columns 3-4 on the subset of these projects targeted at institutional development. In line with the results for World Bank aid, people living near completed projects report a significantly greater willingness to abide by the concerned institutions than people living within the cut-off distance of a future project site. For overall US aid to Uganda, the estimated parameter differences ($\beta_{\text{completed}} - \beta_{\text{future}}$) are equivalent to 13-15 percent of a standard deviation on the institutional index. Considering institutional aid, the estimated impact is again more pronounced (28 and 44 percent of a standard deviation on the institutional index when using a 10 and 25 km cut-off, respectively), as well as more precisely estimated. Once again, the results do not suggest an equivalent effect of having an ongoing project in the vicinity. Indeed, for US aid to Uganda, the estimated effect of living near an ongoing project ($\beta_{\text{ongoing}} - \beta_{\text{future}}$) is negative, and statistically significant when using a 25 km cut-off. In line with previous discussion, this could reflect negative signaling effects (or for that matter, an obstructive influence) of aid during the project implementation phase.

Table 4: Donor comparison. Dependent variable is *inst_index*

VARIABLES	(1) Overall US aid to Uganda: 10 km cutoff	(2) Overall US aid to Uganda: 25 km cutoff	(3) Institutional US aid to Uganda: 10 km cutoff	(4) Institutional US aid to Uganda: 25 km cutoff	(5) Overall Chinese aid to six country sample: 10 km cutoff	(6) Overall Chinese aid to six country sample: 25 km cutoff
Any ongoing	-0.097*	-0.191***	-0.136*	-0.326***	-0.073*	-0.007
	(0.052)	(0.053)	(0.070)	(0.061)	(0.037)	(0.032)
Any (nearly alt. clearly) completed	0.139**	0.134**	0.185**	0.334***	0.025	-0.032
	(0.066)	(0.068)	(0.094)	(0.078)	(0.028)	(0.028)
Future	-0.014	0.007	-0.093**	-0.104***	-0.031	-0.066**
	(0.031)	(0.035)	(0.040)	(0.032)	(0.036)	(0.032)
Diff ongoing-future	-0.0826	-0.198	-0.0436	-0.222	-0.0426	0.0591
F test ongoing-future=0	2.002	9.773	0.286	9.920	0.687	1.485
p value of F test	0.157	0.00181	0.593	0.00168	0.407	0.223
Diff completed-future	0.153	0.127	0.278	0.438	0.0550	0.0345
F test completed-future=0	4.473	2.853	7.526	27.27	1.402	0.662
P value of F test	0.0347	0.0915	0.00617	2.09e-07	0.236	0.416
Observations	11,386	11,386	11,386	11,386	44,518	44,518
R-squared	0.029	0.030	0.029	0.033	0.058	0.058

Robust standard errors (clustered by survey cluster) in parentheses; *** p<0.01, ** p<0.05, * p<0.1; All regressions include baseline controls and year and region fixed effects; In the estimations focusing on US aid to Uganda (Columns 1-4), completed refers to 'nearly completed' projects that have ended in previous years *or within the same year* as the survey (due to very few projects ending in previous years). In columns 5-6 focusing on Chinese aid, we use the standard completed variable including only projects that have ended in years prior to the survey year. The sample in the estimations focusing on Chinese aid (Columns 5-6) include Kenya, Malawi, Mali, Mozambique, Tanzania and Uganda.

While the positive impact of completed US projects in Uganda are in line with our benchmark findings for World Bank aid, we cannot be sure that this result would hold in a large multi-country sample. Conversely, it should be noted that the World Bank results, which are robust in a large multi-country sample, do not hold when looking at Uganda alone. The reason for this is presumably that there are very few respondents living within the specified cut-off distances of future World Bank projects in Uganda.[14]

The estimations in Columns 5 and 6 instead consider overall Chinese aid to Kenya, Malawi, Mali, Mozambique, Tanzania and Uganda,[15] obtained from AidData's Geocoded Global Chinese Official Finance Version 1.1.1 dataset (Bluhm et al. 2018). Since the Chinese government does not release official, project-level financial information about its foreign aid activities, this data is based on an open-source media based data collection technique, synthesizing and standardizing information on Chinese development finance to African countries.[16] The results provide no indication of a positive effect of Chinese aid on local citizens expressed willingness to abide by institutions. This is arguably not surprising, seeing that the concerned Chinese aid flows considered contain very few projects targeted at institutional development (too few to consider them separately). Notably though, unlike recent empirical evidence suggesting negative externalities of Chinese aid to Africa (see e.g. Isaksson and Kotsadam, 2018a; Isaksson, 2020), there is no evidence of a negative effect either; the estimations suggest no statistically significant difference in institutional index scores between people living near future, ongoing and completed Chinese projects.

A thorough assessment of donor heterogeneity in the effects of aid on local institutional quality would require more complete geo-coded aid data. At this stage, we can merely note that compared to the findings we get for World Bank aid, estimations for other selected donors suggest both common ground (the positive impact of completed US projects in Uganda) and the existence of donor heterogeneity in results (the lack of evidence for an impact of Chinese projects).

[14] For World Bank institutional aid to Uganda, these figures are indeed too low to allow for sensible estimation using the estimation strategy at hand: 0.4 percent of respondents live within 10 km of a future project site, and 1.6 percent of respondents live within 25 km of the same.

[15] We limit the sample to the sub-sample of countries in the World Bank estimation dataset that also contain a sufficient number of observations within the cut-off distance of both future, ongoing and completed Chinese projects. Using this restricted sample in estimations focusing on World Bank aid, the results remain qualitatively the same as in the benchmark setup.

[16] Described in detail in Strange et al. (2013) and (2017).

5 Conclusions

Motivated by the lack of sub-national empirical evidence on the relationship between aid and institutional development, this study explored the local effects of aid on perceived institutional quality in African aid receiving countries.

While the importance of building well-functioning state institutions in developing countries is uncontroversial, the question of whether donors are actually helpful in this process is more contentious. Aid optimists argue that an inflow of resources can help recipient countries boost government effectiveness and persuade states to embark on reform. Aid sceptics, on the other hand, maintain that aid promotes rent-seeking behavior and reduces the incentives for democratic accountability and thus the democratic pressures to build effective institutions. The mixed empirical evidence on the relationship between aid and institutions does little to resolve the controversy. One reason for the inconclusive results may be the tendency to study the relationship at the country level.

The aim of this study was to estimate the local effects of aid on institutions in African aid receiving countries, using citizens' expressed willingness to abide by key formal institutions as a proxy for de facto institutional quality. We explored a) the local effects of overall World Bank aid, b) the local effects of World Bank aid targeted specifically at institutional development, and c) potential donor heterogeneity in results.

Doing so, we believe that we make a number of contributions. First, focusing on sub-national variation allows us to capture effects of targeted aid that may not be picked up at the country level. Second, we are better able to account for the non-random allocation of aid. The sub-national geo-coded data on aid and institutional outcomes enabled us to compare localities affected and not affected by development projects – before and after development project implementation – while controlling for potential confounding and omitted variables at relatively fine geographic levels. Third, by focusing on citizens' expressed willingness to abide by key formal institutions we hope to capture de facto as opposed de jure institutions, and thus avoid merely picking up shallow 'form targets', rather than 'function targets' capturing the actual effectiveness of institutions in addressing public problems. And finally, by exploring effects of different forms of aid as well as

donor heterogeneity in results, we address the concern that the aid effectiveness literature often aggregate over aid flows that, since provided for very different purposes, should have very different effects.

The empirical results, drawing on geocoded aid project data combined with geocoded survey data for 73,640 respondents across 12 African countries, suggest a positive impact of World Bank aid on perceptions of local institutional quality. This applies even if we consider World Bank aid to any sector, i.e. not just aid projects specifically targeted at institutional development. As may be expected, however, the estimated effects are indeed more pronounced when restricting our attention to projects focusing on institution building. Notably, the observed effects concern finalized projects, not projects still under implementation. Unlike people living near an ongoing project, people with a completed project within the cut-off distance consistently report significantly higher willingness to abide by the concerned state institutions than people living within the same distance of a future project site. This pattern is stable across a wide range of alternative specifications.

The fact that we see no effect of projects during their implementation phase is in line with the idea that institutional change is a slow process, and thus that empirical studies focusing on institutional outcomes need to allow for changes to take time. However, it could also reflect negative signaling effects of aid during the project implementation phase; a significant donor presence may signal that the government is performing badly. Considering that signaling effects are likely to be most pronounced when donors are visible, i.e. during the project implementation phase, we are more confident that improvements observed post-project completion, when the donor is not present in the area, capture real institutional developments.

A thorough assessment of donor heterogeneity in the effects of aid on local institutional quality would require more complete geo-coded aid data. At this stage, we can merely note that compared to the findings we get for World Bank aid, estimations for other selected donors suggest both common ground and the existence of donor heterogeneity in results.

The results are encouraging in that they suggest a positive impact of World Bank aid, and particularly so of aid targeted at institutional development, on local institutions in African aid receiving countries. For studies of aid effectiveness more generally, our findings call attention to the need for a disaggregated approach that compares aid impacts across sectors, donors and locations.

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Appendix

Table A1: Variable descriptions

Dependent variable

Inst_index: The first principal component of the responses to questions on whether the respondents agree or disagree that a) courts, b) the police and c) the tax authority has the right to make decisions that people always have to abide by (with response categories ranging from 1 for strongly disagree to 5 for strongly agree).

Key explanatory variables: Proximity to World Bank project sites

Any_ongoing: Dummy variable equal to one if the respondent lives within XX km of a site where a World Bank aid project is being implemented at the time of the interview, zero otherwise.

Any_completed: Dummy variable equal to one if the respondent lives within XX km of a completed World Bank project, zero otherwise. Note that any_ongoing and any_completed are not mutually exclusive; respondents could have both ongoing and completed projects within the cut-off distance.

Future: Dummy variable equal to one if the respondent lives within XX km of a World Bank project site where the implementation of the project had not yet started at the time of the interview and do not have any ongoing or completed project within this same distance, zero otherwise.

Only_ongoing: Dummy variable equal to one if the respondent lives within XX km of a site where a World Bank aid project is being implemented at the time of the interview but do not have any completed World Bank projects within the same distance, zero otherwise.

Ongoing_and_completed: Dummy variable equal to one if the respondent lives within XX km of both ongoing and completed World Bank aid projects, zero otherwise.

Only_compled: Dummy variable equal to one if the respondent lives within XX km of a completed World Bank project and do not have any ongoing projects within the same distance, zero otherwise.

Individual control variables

Female: Dummy variable equal to one if the respondent is female; zero otherwise.

Urban: Dummy variable equal to one if the respondent lives in an urban area; zero otherwise.

Age: Age in years

Age squared: Age in years squared (/100)

Presethnic: Dummy variable equal to 1 if the respondent belongs to the same ethnic group as the country president at the time of the survey. Based on self-reported group affiliation using the question: "What is your ethnic community, cultural group or tribe?". coupled with externally compiled data on the ethnic background of the president at the time of the survey. For the ethnic groups of president, we consult at least two sources for each country, drawing most heavily on the compilation in Dreher et al (2015), when necessary updated with more recent data from other sources (e.g. encyclopedia britannica, wikipedia, aljazeera, washington post, africareview.com etc.).

Alternative dependent variables

Additive inst. index: An additive index of the responses to questions on whether the respondents agree or disagree that a) courts, b) the police and c) the tax authority has the right to make decisions that people always have to abide by (with response categories ranging from 1 for strongly disagree to 5 for strongly agree).

*Abide by court: W*hether the respondent agrees or disagrees that the courts have the right to make decisions that people always have to abide by, with response categories ranging from 1 for strongly disagree to 5 for strongly agree.

*Abide by police: W*hether the respondent agrees or disagrees that the police has the right to make decisions that people always have to abide by, with response categories ranging from 1 for strongly disagree to 5 for strongly agree.

*Abide by tax authority: W*hether the respondent agrees or disagrees that the tax authority has the right to make decisions that people always have to abide by, with response categories ranging from 1 for strongly disagree to 5 for strongly agree.

Year and spatial fixed effects

Year dummies: Dummies for interview year, 2002-2015

Country dummies: Dummies for the 12 countries in the sample

Country-year dummies: interacting the full set of year dummies with the full set of country dummies

Region dummies: 308 sub-national region dummies

Table A2: Summary statistics for key variables

	Overall World Bank aid					World Bank aid projects targeted at institutional development				
Variable	Obs	Mean	Std. Dev.	Min	Max	Obs	Mean	Std. Dev.	Min	Max
Dependent variable										
Inst_index	73,640	0.000	1	-2.949	1.317	47,705	0	1	-3,059	1,263
Aid variables										
Any_ongoing10	73,640	0.295	0.456	0	1	47,705	0,090	0,287	0	1
Any_completed10	73,640	0.204	0.403	0	1	47,705	0,075	0,264	0	1
Future10	73,640	0.065	0.247	0	1	47,705	0,038	0,192	0	1
Only_ongoing10	73,640	0.143	0.350	0	1	47,705	0,053	0,225	0	1
Ongoing_and_completed10	73,640	0.153	0.360	0	1	47,705	0,037	0,189	0	1
Only_compled10	73,640	0.052	0.221	0	1	47,705	0,039	0,192	0	1
Any_ongoing25	73,640	0.563	0.496	0	1	47,705	0,192	0,394	0	1
Any_completed25	73,640	0.401	0.490	0	1	47,705	0,162	0,368	0	1
Future25	73,640	0.095	0.294	0	1	47,705	0,074	0,261	0	1
Only_ongoing25	73,640	0.231	0.421	0	1	47,705	0,106	0,308	0	1
Ongoing_and_completed25	73,640	0.333	0.471	0	1	47,705	0,086	0,280	0	1
Only_completed25	73,640	0.069	0.253	0	1	47,705	0,076	0,264	0	1
Individual controls										
Age	73,640	35.874	14.032	18	100	47,705	36,005	13,810	18	99
Age squared (/100)	73,640	14.838	12.303	3.24	100	47,705	14,871	12,070	3.24	98.01
Female	73,640	0.488	0.500	0	1	47,705	0,487	0,500	0	1
Urban	73,640	0.332	0.471	0	1	47,705	0,277	0,447	0	1
Presethnic	53,750	0.142	0.349	0	1	35,332	0,160	0,367	0	1
Alternative dependent variables										
Additive inst. index	73,640	11.283	2.813	3	15	47,705	11,477	2,778	3	15
Abide by court	73,640	3.785	1.165	1	5	47,705	3,853	1,157	1	5
Abide by police	73,640	3.828	1.149	1	5	47,705	3,900	1,144	1	5
Abide by tax authority	73,640	3.670	1.222	1	5	47,705	3,724	1,243	1	5

Table A3: Sample balance

	Overall World Bank aid			World Bank aid projects targeted at institutional dev.		
Dependent variable is:	Age	Female	Urban	Age	Female	Urban
any_ongoing10	-0.649***	0.003**	0.288***	-1.290***	0.004	0.425***
	(0.174)	(0.001)	(0.017)	(0.346)	(0.003)	(0.033)
any_completed10	-1.021***	0.003*	0.268***	-1.841***	0.005	0.359***
	(0.197)	(0.001)	(0.020)	(0.342)	(0.003)	(0.039)
future10	-0.373	0.002	0.169***	-2.373***	0.006	0.473***
	(0.346)	(0.003)	(0.033)	(0.429)	(0.004)	(0.042)
Diff ongoing-future	-0.276	0.000995	0.119	1.083	-0.00180	-0.0480
F test ongoing-future=0	0.545	0.148	14.46	4.349	0.134	0.994
P value of F test	0.461	0.701	0.000144	0.0371	0.715	0.319
Diff completed-future	-0.648	0.000526	0.0990	0.532	-0.000839	-0.114
F test completed-future=0	2.666	0.0328	5.861	0.893	0.0249	4.071
P value of F test	0.103	0.856	0.0155	0.345	0.875	0.0437
Observations	79,546	79,546	79,546	51,358	51,358	51,358
R-squared	0.043	0.000	0.245	0.029	0.000	0.224

Robust standard errors (clustered by survey cluster) in parentheses; *** p<0.01, ** p<0.05, * p<0.1; All estimations include country and year FEs

Table A4: Using alternative dependent variables (and a 10 km cut-off around project sites)

	Overall aid				Institutional aid			
VARIABLES	(1) Additive inst. index	(2) Abide by courts	(3) Abide by police	(4) Abide by tax authority	(5) Additive inst. index	(6) Abide by courts	(7) Abide by police	(8) Abide by tax authority
Any ongoing	-0.075	-0.035*	-0.024	-0.012	-0.123	-0.066*	-0.008	-0.039
	(0.055)	(0.020)	(0.018)	(0.023)	(0.100)	(0.038)	(0.030)	(0.043)
Any completed	0.043	-0.027	0.022	0.044*	0.155*	0.016	0.053*	0.077**
	(0.055)	(0.021)	(0.018)	(0.024)	(0.093)	(0.035)	(0.031)	(0.039)
Future	-0.197**	-0.068**	-0.054**	-0.077**	-0.204**	-0.159***	-0.061*	0.018
	(0.077)	(0.032)	(0.027)	(0.031)	(0.085)	(0.039)	(0.035)	(0.032)
Diff ongoing-future	0.122	0.0331	0.0300	0.0647	0.0809	0.0931	0.0533	-0.0570
F test ongoing-future=0	1.668	0.660	1.163	2.620	0.447	3.641	1.521	1.222
P value of F test	0.197	0.417	0.281	0.106	0.504	0.0564	0.217	0.269
Diff completed-future	0.240	0.0411	0.0764	0.121	0.359	0.175	0.114	0.0587
F test completed-future=0	7.789	1.598	4.945	12.77	7.681	10.18	5.884	1.328
P value of F test	0.00527	0.206	0.0262	0.000355	0.00561	0.00143	0.0153	0.249
Observations	73,645	76,705	77,649	74,857	47,705	49,508	50,113	48,518
R-squared	0.030	0.030	0.033	0.023	0.024	0.021	0.025	0.026

Robust standard errors (clustered by survey cluster) in parentheses; *** p<0.01, ** p<0.05, * p<0.1; All regressions include baseline controls and year and country fixed effects

Table A5: The impact of World Bank aid projects, irrespective of sector, on perceived institutional quality within a 25 km cut-off. Dependent variable is *Inst_index.*

VARIABLES	(1) Benchmark	(2) Region FEs	(3) Geo-matching	(4) Excluding wave 6	(5) Future within 5	(6) Country-year FEs	(7) Project FEs	(8) Control for president coethic	(9) Treatment sub-categories	(10) Completed vs. future sample	(11) Completed vs. future sample
Any ongoing	-0.056***	-0.032**	-0.054***	-0.057***	-0.055***	-0.048***	-0.026	-0.041**			
	(0.014)	(0.014)	(0.015)	(0.015)	(0.014)	(0.013)	(0.026)	(0.018)			
Any completed	-0.015	-0.007	-0.016	-0.034**	-0.016	-0.022	0.012	-0.045**		-0.020	
	(0.015)	(0.015)	(0.015)	(0.016)	(0.015)	(0.014)	(0.040)	(0.018)		(0.030)	
Future	-0.048**	-0.022	-0.045**	-0.063***	-0.064***	-0.031		-0.056*	-0.044**		
	(0.021)	(0.021)	(0.021)	(0.021)	(0.024)	(0.020)		(0.030)	(0.021)		
Only ongoing									-0.047***		
									(0.018)		
Ongoing & completed									-0.070***		
									(0.016)		
Only completed									0.003		0.118***
									(0.023)		(0.039)
Diff ongoing-future	-0.00769	-0.0095	-0.0094	0.00543	0.00859	-0.0162		0.0156			
F test ongoing-future=0	0.124	0.195	0.184	0.0681	0.115	0.643		0.233			
P value of F test	0.725	0.659	0.668	0.794	0.734	0.423		0.629			
Diff completed-future	0.0329	0.0151	0.0291	0.0282	0.0482	0.00968		0.0111			
F test comp.-future=0	1.759	0.374	1.363	1.193	3.067	0.162		0.116			
p value of F test	0.185	0.541	0.243	0.275	0.0799	0.687		0.734			
Observations	73,640	73,640	72,525	55,137	73,640	73,640	42,883	53,750	73,640	36,578	12,079
R-squared	0.031	0.075	0.031	0.028	0.031	0.050	0.096	0.028	0.031	0.043	0.042

Robust standard errors (clustered by survey cluster) in parentheses; *** p<0.01, ** p<0.05, * p<0.1; All regressions include baseline controls and year and country (or region, when specified) fixed effects; Column 3 excludes all respondents in enumeration areas further than 100 km away from any World Bank projects site; Column 4 excludes Afrobarometer survey wave 6 (which contains no observations connected to future project sites); Column 5 restricts the pre-treatment group to respondents living close to sites where projects will start within a maximum of five years of the interview date; Column 6 controls for country-year fixed effects; Column 7 restricts the sample to respondents in enumeration areas that the Afrobarometer (despite not having a panel structure) happened to revisit before and after a project was initiated or completed, allowing for the inclusion of project fixed effects (note that there is no need to compare with sites of future projects in this setup). Column 8 introduces a control for the respondent belonging to the same ethnic group as the country president at the time of the survey. The coefficient on the control (not presented) is 0.06, statistically significant at the 1 percent level; Column 9 breaks down the treatment groups (ongoing and completed) into sub-categories specifying if the respondent lives within the cut-off distance of only ongoing projects, both ongoing and completed projects, or only completed projects. Only the difference between only completed and future is close to statistically significant (with p-value 0.101); Columns 10 and 11 focus on a restricted sample consisting only of respondents living within the cut-off distance of a future or a completed World Bank project. Column 10 focuses on the parameter of having any completed project nearby (and includes respondents living in areas close to both a completed and an ongoing project). Column 11 focuses on the parameter of having only completed projects in the vicinity, and thus restricts the sample further (excluding also respondents living in areas close to both a completed and an ongoing project).

Table A6: The impact of World Bank aid projects targeted at institutional development on perceived institutional quality within a 25 km cut-off. Dependent variable is *Inst_index*

VARIABLES	(1) Benchmark	(2) Region FEs	(3) Geo-matching	(4) Excluding wave 6	(5) Future within 5	(6) Country-year FEs	(7) Project FEs	(8) Control for president co-ethnic	(9) Treatment sub-categories	(10) Completed vs future sample	(11) Completed vs future sample
Any ongoing	-0.055**	-0.026	-0.053**	-0.038	-0.052**	-0.020	0.007	-0.071**			
	(0.023)	(0.023)	(0.023)	(0.026)	(0.023)	(0.020)	(0.074)	(0.029)			
Any completed	0.054*	0.042	0.046	0.007	0.052*	0.006	0.222**	0.066**		0.113**	
	(0.029)	(0.027)	(0.029)	(0.026)	(0.028)	(0.024)	(0.088)	(0.029)		(0.049)	
Future	-0.073***	-0.050*	-0.068***	-0.082***	-0.102***	-0.093***		-0.049	-0.073***		
	(0.025)	(0.026)	(0.025)	(0.025)	(0.032)	(0.025)		(0.037)	(0.025)		
Only ongoing									-0.055**		
									(0.028)		
Ongoing and completed									-0.002		
									(0.041)		
Only completed									0.054*		0.094*
									(0.032)		(0.052)
Diff ongoing-future	0.0186	0.0237	0.0153	0.0443	0.0498	0.0731		-0.0221			
F test ongoing-future=0	0.345	0.582	0.224	1.795	1.740	5.281		0.246			
P value of F test	0.557	0.446	0.636	0.180	0.187	0.0216		0.620			
Diff completed-future	0.127	0.0915	0.114	0.0896	0.154	0.0985		0.115			
F test compl-future=0	11.49	5.876	9.058	6.722	11.85	8.647		6.884			
P value of F test	0.000707	0.0154	0.00264	0.00957	0.000582	0.00330		0.00874			
Observations	47,705	47,705	40,637	35,057	47,705	47,705	16,014	35,332	47,705	11,218	7,114
R-squared	0.025	0.059	0.025	0.022	0.025	0.038	0.081	0.022	0.025	0.038	0.040

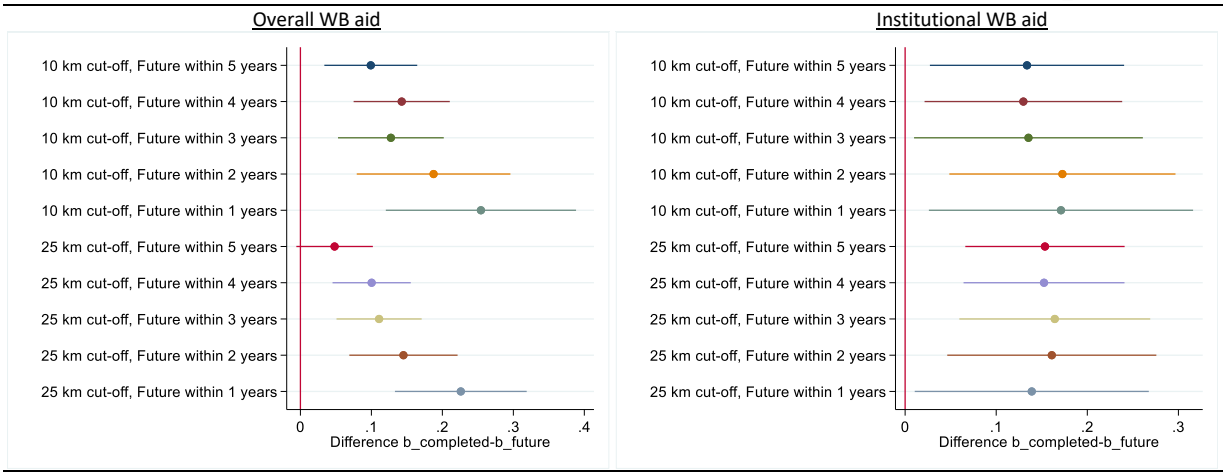
Robust standard errors (clustered by survey cluster) in parentheses; *** p<0.01, ** p<0.05, * p<0.1; All regressions include baseline controls and year and country (or region, when specified) fixed effects; Column 3 excludes all respondents in enumeration areas further than 100 km away from any World Bank projects site; Column 4 excludes Afrobarometer survey wave 6 (which contains no observations connected to future project sites); Column 5 restricts the pre-treatment group to respondents living close to sites where projects will start within a maximum of five years of the interview date; Column 6 controls for country-year fixed effects; Column 7 restricts the sample to respondents in enumeration areas that the Afrobarometer (despite not having a panel structure) happened to revisit before and after a project was initiated or completed, allowing for the inclusion of project fixed effects (note that there is no need to compare with sites of future projects in this setup). Column 8 introduces a control for the respondent belonging to the same ethnic group as the country president at the time of the survey. The coefficient on the control (not presented) is 0.07, statistically significant at the 1 percent level; Column 9 breaks down the treatment groups (ongoing and completed) into sub-categories specifying if the respondent lives within the cut-off distance of only ongoing projects, both ongoing and completed projects, or only completed projects. The difference onlyongoing-future= 0.019, with p-value 0.584, the difference ongoingandcompleted-future=0.072, with p-value 0.136, the difference onlycompleted-future=0.123, with p-value 0.001; Columns 10 and 11 focus on a restricted sample consisting only of respondents living within the cut-off distance of a future or a completed World Bank project. Column 10 focuses on the parameter of having any completed project nearby (and includes respondents living in areas close to both a completed and an ongoing project). Column 11 focuses on the parameter of having only completed projects in the vicinity, and thus restricts the sample further (excluding also respondents living in areas close to both a completed and an ongoing project).

Table A7: Estimations focusing on World Bank aid to all sectors other than 'institutional'. Dependent variable is *Inst_index*

VARIABLES	(1) 10 km cut-off	(2) 25 km cut-off
Any ongoing	-0.017	-0.036**
	(0.020)	(0.015)
Any completed	0.013	-0.018
	(0.021)	(0.016)
Future	-0.092***	-0.034
	(0.031)	(0.022)
Diff. ongoing-future	0.0747	-0.00205
F test ongoing-future=0	4.046	0.00753
P value of F test	0.0443	0.931
Diff completed-future	0.104	0.0161
F test completed-future=0	9.022	0.368
P value of F test	0.00268	0.544
Observations	70,640	70,640
R-squared	0.030	0.030

Robust standard errors (clustered by survey cluster) in parentheses; *** p<0.01, ** p<0.05, * p<0.1; All regressions include baseline controls and year and country fixed effects.

Figure A1: Estimated effects of World Bank aid when restricting the time span of the pre-treatment group. Dep. var. is *inst_index*



Notes: Estimated effect with 95% confidence intervals; Estimations equivalent to Column 5 in Tables 1-2, where we restricting the pre-treatment group to respondents living close to sites where projects will start within a maximum of five years of the interview date, but varying the year span.