




Aid and institutions: Local effects of World Bank aid on perceived institutional quality in Africa

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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. The empirical results, which are robust across a wide range of specifications as well as to using alternative identification strategies, suggest a positive impact of World Bank aid on citizens' expressed willingness to abide by key formal institutions. This applies for overall World Bank aid, but as may be expected, 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.

Keywords Aid · Institutions · Africa · World Bank

JEL Classification F35 · O17 · O19 · O55

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

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countries is uncontroversial (North, 1990; Acemoglu & 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 Mauritania (International Development Association, 2019; World Bank, 2018).

The empirical evidence on the relationship between aid and institutions is mixed, and hence does little to resolve the controversy between aid optimists and aid sceptics (e.g. Bräutigam & Knack, 2004; Busse & Gröning, 2009; Djankov et al., 2008; Jones & Tarp, 2016; Knack, 2004; Okada & Samreth, 2012; Svensson, 2000; Tavares, 2003). 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 & Knack, 2004). Second, it is common to aggregate over aid flows that should have different effects, since they are provided for different purposes. (Clemens et al., 2012; Bourguignon & Gunning, 2016). Furthermore, even if taking steps to disaggregate aid and consider the impact of different aid flows (e.g. Jones & 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 & Lohmann, 2015). Indeed, many development projects are targeted at local development, reasonably suggesting 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 World Bank aid on perceived institutional quality in Sub-Saharan African aid receiving countries. We focus on citizens' expressed willingness to abide by key formal institutions (the courts, the police and the tax authority), and explore both the local effects of overall World Bank aid and the local effects of World Bank aid to a greater extent targeted at institutional development.

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. To account for the endogenous placement of World Bank projects, we use a spatial–temporal estimation strategy comparing the estimated effect of living near a site where a World Bank project was under implementation (*Ongoing*) or finalized (*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 (*Future*).¹ While the Afrobarometer is not a panel, with this estimation strategy we can still make use of the time variation in the data. The parameter differences between *Ongoing* and *Completed*, on the one hand, and *Future* on the other, provide us with difference-in-difference type of measures that control for unobservable time-invariant characteristics that may influence selection into being a World Bank project site. In alternative specifications, we instead use matching, with similar results.

The empirical findings, which are robust across a wide range of specifications, 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. all aid projects independent of focus area. As may be expected, however, the estimated effects are more pronounced when we restrict our attention to projects that are more relevant for 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 institutions (e.g. Bräutigam & Knack, 2004; Busse & Gröning, 2009; Djankov et al., 2008; Jones & Tarp, 2016; Knack, 2004; Okada & Samreth, 2012; Svensson, 2000; Tavares, 2003), 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 & 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 or perceptions thereof. 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

¹ The approach resembles that in Knutsen et al. (2017). See also Isaksson and Kotsadam (2018a, b).

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 perceived 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 variation, 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 institutions, on the empirical evidence available to date, and on the need for sub-national, disaggregated, analysis in the field.

2 Aid and institutions

Institutions can be defined as formal and informal rules that shape the incentives in human exchange, whether political, social or economic (North, 1990). As is clear from this often cited definition, the concept is very broad, including formal institutional arrangements to allocate and uphold political power and shape economic incentives (see e.g. Acemoglu et al., 2005) as well as informal customs guiding behavior. The multi-dimensional nature of the concept is reflected in the literature on the linkages between aid and institutions, which hardly suggest a simple, uni-directional effect of the former on the latter (Jones & Tarp, 2016). The studies discussed below focus on a wide range of outcomes relating to political institutions in a broad sense.

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 & Gunning, 2016). Through the infusion of resources and technical assistance, aid can potentially boost government effectiveness (see e.g. the discussion in Bräutigam & Knack, 2004; Djankov et al., 2008; 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 & 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 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. Deaton, 2013; Easterly, 2006). Fiscal contract theory (e.g. Dietrich et al., 2017; Baldwin & 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 & 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 (de la Cuesta et al., 2021).

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 (Bräutigam & Knack, 2004; Svensson, 2000) and worsen democratic institutions (Djankov et al., 2008) and other governance outcomes (Busse & Gröning, 2009).

Another concern is that aid contributes only to shallow institutional reform (Pritchett et al., 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’, which capture 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 & Smith, 2010; Ahmed, 2012; Faye & Niehaus, 2012; Dutta et al., 2013), as well as on the degree of aid fungibility, in turn relating to conditions imposed by donors (Altincekic & Bearce, 2014; Collier, 2006; Jones & 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 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 & 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, it should instead be negative. To begin with, however, we need to consider to what extent these mechanisms are likely to extend to local level institutions.

With respect to ‘aid as finance’, an infusion of resources and technical assistance can reasonably help governments build local as well as central government institutions. Indeed, considering the decentralization efforts in most African countries over the last few decades (see e.g. Bardhan, 2002; Smoke, 2003; Wilfahrt, 2018), assistance directed at local government institutional development should be especially relevant. Building institutional infrastructure that links central and local government, as well as functioning systems to reach out to citizens for the purpose of service delivery and enforcement of regulation, is clearly demanding (Smoke, 2003). Without proper checks and balances in place, this process involves a high risk for corruption and elite capture (Platteau & Gaspart, 2003). Moreover, and as highlighted by Bardhan (2002), capacity constraints are often most severe in local level institutions. Against this background, it seems ‘aid as finance’ can have an important role to play for local institution building, and indeed, several World Bank projects in our data focus on strengthening local government (for specific examples, see footnote 11 and 12).

With respect to ‘aid as reform’, according to which aid can be used to persuade states to embark on reform (Bourguignon & Gunning, 2016), it is arguably more common for donors to track developments in the central government of the partner

country than to follow progress (or the lack thereof) in local institution building. For example, the 'Performance Based Allocation' (PBA) rule used by the World Bank to allocate its concessional funds draws on a country level performance rating assessing e.g. public sector management (World Bank, 2021). That said, donor promoted priorities might of course spill over on local institution building initiatives.

Considering possible negative effects from rent-seeking and weakened democratic accountability, the former could arguably be visible in the local area in the early implementation phase of projects, when allocating funds and awarding contracts. At the same time, local donor presence may help to strengthen oversight and accountability during the active implementation phase. Forces acting to undermine democratic accountability, on the other hand, seem more likely to operate over the longer term. As will be discussed further below, assessing impacts of projects at different points in the project implementation cycle should thus be informative.

Focusing on local effects of World Bank aid there is arguably reason for optimism. Unlike e.g. China (see the discussion in Section 4.2), the World Bank emphasizes its commitment to promoting good governance and institution building. Furthermore, whereas weakened democratic accountability mechanisms according to the above arguments could potentially undermine institutions over the longer term, an infusion of resources and technical assistance should operate more directly and may thus result in tangible improvements observed by local citizens. Focusing on local aid impacts, we should be better able to detect such positive impacts, which may be obscured by other forces at the country level.

Indeed, considering 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. Exploring sub-national variation in aid and institutional outcomes not only enables us to capture effects of regionally targeted aid that may not be picked up at the country level, but also to explore variation across types of aid, and to some extent across donors.

Other donors do not routinely geo-code their aid. Hence, the geo-coded aid data at hand does not allow for a systematic assessment of donor heterogeneity, and of potential contamination of the treatment and control groups due to complementarities and substitution effects among aid projects from different donors. Considering that there is likely to be substantial donor heterogeneity in results, not least between multilateral and bilateral donors (see e.g. Charron, 2011; Dietrich, 2013; Dreher et al., 2021), this is unfortunate. However, thanks to the geocoding efforts of Aid-Data (Bluhm et al., 2018), who have tracked Chinese financial flows to Africa over multiple years, we are able to control for the presence of Chinese aid. Moreover, in the appendix we consider aid from the US and China to selected recipient countries (data permitting) in more detail.

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 in mind, our dependent variable focuses on citizens' expressed willingness to abide by key formal institutions. The idea is that this measure should capture perceptions of the effectiveness and fairness of institutions. Just like country level

institutional indices based on expert judgements, the indicator is based on perceptions, i.e. subjective accounts as opposed to objective criteria.² Unlike country level indices of institutional development, however, this variable arguably enables us to get a picture of how citizens experience de facto institutional quality at the local level. Importantly, citizen perceptions of institutions 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 most likely 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 should depend on own experiences and the experiences of friends and family in the surrounding area.

That said, focusing on citizen reports also brings challenges. Kurtz and Schrank (2007a, b) show that influential country level governance indices based on subjective expert assessments tend to conflate the quality of governance with economic policy and recent economic performance. Against this background, we seek to construct a dependent variable that, as far as possible, captures institutional assessments independent of political preferences (see Section 3). Nonetheless, a reasonable concern is that improvements in terms of e.g. economic growth or public goods provision bring halo effects that translate into more positive citizen assessments of institutional quality. With country-year (or region-year) fixed effects, we are able to control for general country (or region) level variation in government effectiveness and economic performance over time. However, some halo effects may be directly related to receiving aid.

In particular, one may be concerned that perceptions of institutional quality are susceptible to signaling effects of receiving aid. To claim credit for project achievements, donors often make themselves visible at project sites (Dietrich et al., 2018, 2019). Flags, logos and other donor signifiers work to make citizens aware of the sources of aid financing in the local area. 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, citizens might credit their local authorities for bringing a project into their community. Indeed, local politicians may actively seek to take credit for the receipt of foreign aid (see e.g. Cruz & Schneider, 2017) and 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., 2018). In an attempt to disentangle such signaling effects from perceptions of de facto institutional improvements, we

² For a discussion of objective versus subjective measures of institutions, see e.g. Olken (2006), who unlike studies focusing on perceived corruption uses an objective corruption indicator capturing the leakage of funds from a large development program in Indonesia.

compare the 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.

Similarly, if the timing of implementation and completion of aid projects are related to political considerations (Kersting & Kilby, 2016; Marx, 2018; Williams, 2017), improved institutional ratings may be driven by policies relating to, for example, re-election efforts rather than by the aid projects themselves. Also, a common argument is that African policy-makers tend to favor their own homelands and ethnic groups in the allocation of funds (see e.g. Wantchekon, 2003; Alesina et al., 2016; Dreher et al., 2019; Isaksson, 2020). One may thus suspect that co-ethnics of the president are more pleased with current economic and political conditions – potentially spilling over into more favorable assessments of state institutions – as well as more likely to receive aid than are members of other groups. To address these concerns, we account for the timing of elections, run estimations where we include only survey waves with no rotation in the party in power between survey dates, and control for the respondent belonging to the same ethnic group as the country president at the time of the survey.

In sum, we thus focus on the local effects of aid projects at different stages of implementation on confidence in institutions in African aid receiving countries, comparing the effects of overall aid with those of aid to a greater extent targeted at institutional development, and controlling for factors (other than aid induced institutional improvements) that may affect institutional assessments. 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 confidence in institutions, 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,³ interviewed between 2002 and 2015. The survey data is obtained from rounds 2–6 of the Afrobarometer survey (Afrobarometer, 2020; 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 US and Chinese projects in selected African recipient countries with sufficient data coverage (also from AidData).

The aid data contains latitude and longitude project co-ordinates, and provides 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

³ For the exact estimation sample we get after sample restrictions, see below.

as ‘near’, in the ‘area’ of, or up to 25 km away from an exact location.⁴ 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 from the cluster center 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 project sites and Afrobarometer survey clusters in our benchmark sample countries.

Our main outcome variable is an index focusing on citizens’ expressed willingness to abide by three key state institutions.⁵ Specifically, we use the first principal component 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.⁶ These questions have the advantage of broad coverage, both across countries and over time.

Focusing on the willingness to abide by key institutions, the index relates closely to the legitimacy of state institutions. In effect, Levi et al. (2009) use these Afrobarometer questions as measures of legitimacy to capture a sense of obligation or willingness to obey state authority, in turn suggested to depend on the perceived trustworthiness of government and procedural justice of its institutions. In line with this argument, we posit that the willingness to abide by key institutions should capture confidence in these same institutions, in turn presumably reflecting perceptions of their effectiveness and fairness. We deliberately focus on institutions not explicitly connected to any political party, or the executive branch of government, in order to capture confidence in institutions rather than political alliances and satisfaction with the government.⁷

Compared to alternative individual level indicators, most notably questions focusing on institutional trust, our measure stands out as less prone to pick up variation based in grievances and political attitudes, why we argue that it should be better suited to capture perceptions of how institutions actually function (see the discussion in Appendix B). Nonetheless, we run estimations using an alternative index focusing on institutional trust, with very similar results (see Tables B1, B2).

As discussed in Section 2, the distinction between national and local institutions is far from clear-cut. Media reports of developments at the national level will most likely

⁴ Precision categories 1 and 2 in Strandow et al. (2011).

⁵ Variable descriptions, summary statistics, and the number of project sites by country and year are presented in Tables A1–A8 in the Appendix.

⁶ Principal component analysis (PCA) is a technique to reduce the dimensionality of a dataset, while preserving as much of the variability as possible from the underlying variables (Bartholomew, 2004; Jolliffe and Cadima, 2016). In the appendix, we present results of estimations where we specify our dependent variable differently (see Tables A9–A10). In particular, we use Item Response Theory (IRT) to create an index based on a generalized partial credit model (GPCM) to ordinal items. The results remain very similar. We also use a simple additive index, as well as consider the index component variables (the expressed willingness to abide by the courts, the police and the tax authority) separately. Again, the results remain similar.

⁷ We find no indication of a substitution effect between willingness to abide by formal institutions and trust in informal/traditional rule. Rather, the correlation between our institutional index and trust in traditional leaders is positive, albeit weak (0.12).

influence citizen perceptions of the courts, the police and the tax authorities. However, even for institutions primarily governed centrally, such as the tax authority, the de facto implementation of their mission will take place at the local level throughout the country, in their day-to-day contact with citizens. Citizens' own experiences with local police, justice system and revenue collectors, should thus be central for their confidence in the same.

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*).⁸

We restrict the sample to countries with observations in all three treatment categories. When focusing on overall World Bank aid irrespective of focus area, this gives a benchmark estimation sample of 73,640 respondents across 12 countries (Benin, Burkina Faso, Cape Verde, Kenya, Lesotho, Madagascar, Malawi, Mali, Mozambique, Nigeria, Tanzania and Uganda).⁹ Our sample countries contain 2641 ongoing, future or completed World Bank project sites with precise geocodes and information on project dates.¹⁰ 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.

Our baseline set of estimations consider overall aid, i.e. any World Bank development project irrespective of focus area. 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, in a second set of estimations we restrict our attention to World Bank projects that to a greater extent target institutional development. In particular, we focus on projects for which at least 50 percent of the overall project theme share is dedicated to the 'Public Sector Governance' and/or 'Rule of Law' major themes (see World Bank, 2016). Restricting the sample to countries with observations

⁸ The classifications are based on Afrobarometer survey dates in relation to the project start and end dates from the aid project data. The project end date refers to the year in which transactions ended (rather than the fiscal year in which the project formally leaves the World Bank's active portfolio). We view this as the best available approximation of the end of the project implementation phase. For projects that were still under implementation when the World Bank aid project data was compiled in 2017, the end dates refer to planned completion. However, considering that the Afrobarometer survey data that we use covers only the period up to 2015, we draw only on the actual end dates and do not have to utilize such predictions.

⁹ Specifically, we include only countries that have at least 100 respondents connected to ongoing, completed and future projects within 25 km. For the most part, this restriction is far from binding (see Table A6). In some countries, however, there are few observations living within the cut-off distance of future project sites. In Madagascar, only 114 respondents live within 25 km of a future project, and with a 10 km cut-off, there are less than 100 respondents living within the cut-off distance of a future project in Burkina Faso and Madagascar. We run estimations where we exclude these countries, with no change in results (Table A11). Furthermore, since none of the sample countries have observations in all treatment groups in all individual years (Table A7), we run estimations where we restrict the sample so that for each country, we include only survey years with some (more than 0) observations in each treatment category. The key results do not change (Table A11).

¹⁰ We do not make a distinction between different lending instruments. In practice, however, 99 percent of the projects sites considered receive Investment Project Financing, and merely one percent receive Program-for-Results or Structural Adjustment/Development Policy Financing (for an overview of World Bank financing instruments, see World Bank, 2022).

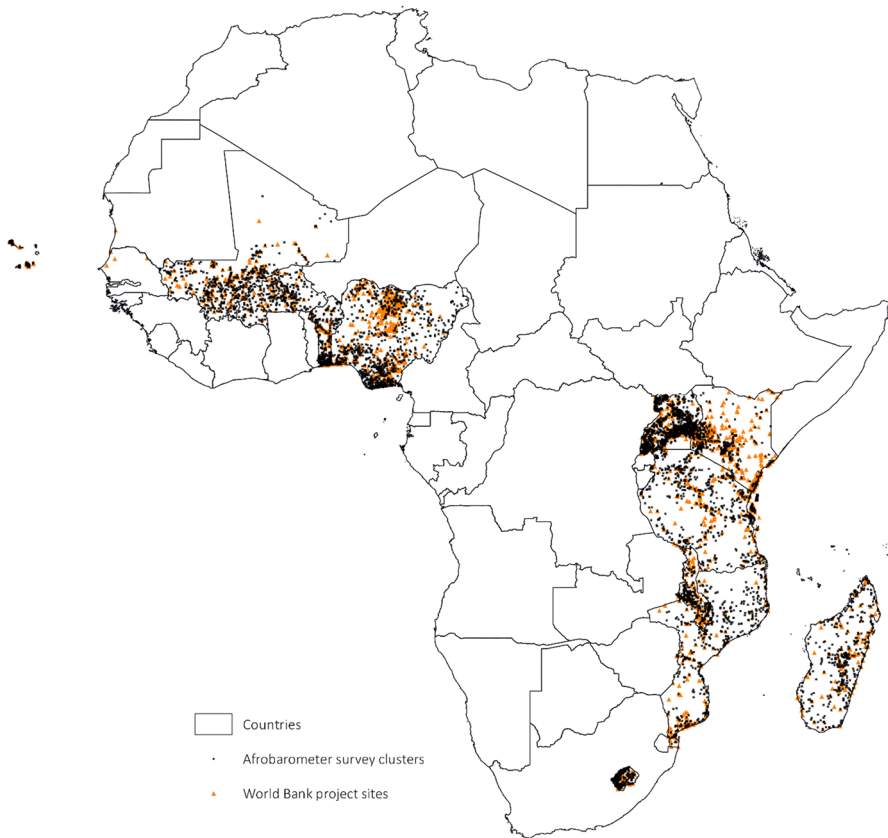


Fig. 1 Map of project sites and survey clusters in sample countries

in all three treatment categories, gives an estimation sample of 29,097 respondents from four countries (Ghana, Kenya, Madagascar and Uganda). Across these four countries, there are 99 projects fulfilling our institutional aid criteria. Considering that most projects tend to have multiple, cross-sectoral objectives (see the discussion in Cruz & Keefer, 2015), this classification is by no means precise. Rather, it is a rough approximation of aid relevant for institution building, narrowing down the spread of project objectives to some extent.

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 (see e.g. Briggs, 2017, 2021). 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.¹¹ 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),¹² 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:

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

where perceived institutional quality Y of 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.¹³ To account for correlated errors, the standard errors are clustered at the geographical clusters (i.e., at the enumeration area level).

¹¹ Resembling that in Knutsen et al., 2017. See also Isaksson and Kotsadam (2018a, b).

¹² 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.

¹³ Table A12 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 institutional aid, there are no statistically significant differences between the treatment and pre-treatment groups.

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 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.

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, completed projects start earlier and future projects start later on average.

Table A13 presents results of estimations where we explore variation in our dependent variable and balance in terms of covariates across different segments of our pre-treatment group. To examine the existence of time trends that could bias our estimated effects, we run estimations comparing people connected to future projects starting prior to 2010 (the median start year of projects awaiting our pre-treatment areas), and from 2010 and onwards, respectively. In the absence of time trends, these two groups should be comparable, since both will receive the treatment in the future, just at different points in time. Reassuringly, we observe no statistically significant differences between the groups, neither in terms of key covariates nor with respect to our dependent variable.

Next, we explore potential biases arising due to factors related to how far ahead of the survey date the future project in the area will start. Specifically, we use the median number of years from the interview year until project start in our pre-treatment group as cut-off and compare respondents living where a future project will start within 1–4 years to respondents living where a future project will start within 5–12 years. Again, we find no differences between the two groups in terms of covariates. However, they do in fact seem to differ in their reported willingness to abide by institutions. In particular, people living in areas with future projects starting sooner express a lower willingness to abide by institutions.¹⁴ In the immediate period ahead of project

¹⁴ Interacting the variable *Future* with a variable giving the number of years until project start, the interaction term parameter is indeed positive and statistically significant at the five percent level, suggesting that respondents living in areas with future projects starting far ahead in time are not as sceptical of the idea of abiding by institutions as those with future projects starting sooner. If restricting our attention to projects starting 2003–2007, which is the range of common support for project start years in the three treatment categories (see the discussion in Section 4.2), this interaction term is far from statistically significant, however, highlighting the importance of evaluating project timing.

start, anticipation effects as well as actual disruptions due to the upcoming project start may arguably affect confidence in institutions.¹⁵ However, even if considering projects starting a few years ahead, it is possible that certain conditions (relating e.g. to other policies or need) tend to precede the arrival of projects in the area.

With these considerations in mind, we take care to evaluate possible effects of project timing in the sensitivity analysis. In particular, we account for potential strategic timing of aid projects, explore the sensitivity of results to restricting the time-bandwidth considered when comparing respondents living near ongoing, completed and future projects, and consider possible effects of aid clustering in certain areas with long traditions of aid.

Using the above approach to study whether World Bank aid projects affect perceived 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

The results indeed indicate a positive impact of World Bank aid on perceived institutional quality. This applies if we consider overall World Bank aid independent of focus area. As may be expected, however, the estimated effects are more pronounced when restricting our attention to projects that to a greater extent focus on institution building. We find that the estimated effect grows with years since project completion, in line with the idea that building (confidence in) institutions takes time. Furthermore, whereas the effect of overall World Bank aid appears relatively local, the estimated effect of projects that to a greater extent target institutional development has a wider geographical reach.

4.1 Benchmark results

Table 1 presents the results of our benchmark regression for overall World Bank aid irrespective of specific objectives. To begin with, the estimation demonstrates the importance of taking the non-random selection of World Bank project sites into account. The coefficient on *Future* is 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.

People living within 10 km of a completed project site report a significantly higher willingness to abide by the concerned state institutions than people

¹⁵ Focusing on future projects starting within 2–4 years, i.e. leaving out projects starting in the year following the survey, there is still some indication of a parameter difference between the groups, but only statistically significant at the ten percent level.

Table 1 Benchmark results for overall World Bank aid (10 km cut-off). Dependent variable is *Inst_index*

Variables	Overall World Bank aid
Ongoing	-0.027 (0.019)
Completed	0.015 (0.020)
Future	-0.070** (0.027)
Diff. Ongoing-future	0.0427
F test: Ongoing-Future = 0	1.637
P value of F test: Ongoing-Future = 0	0.201
Diff. Completed-future	0.0845
F test: Completed-Future = 0	7.584
p value of F test: Completed-Future = 0	0.00591
Observations	73,640
R-squared	0.030

Robust standard errors (clustered by survey cluster) in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$; The regression includes baseline controls (age, age squared, urban, female) and year and country fixed effects

living within the cut-off distance of a future project site. The parameter difference $\beta_{\text{completed}} - \beta_{\text{future}}$ is equivalent to 8 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. $\beta_{\text{ongoing}} - \beta_{\text{future}}$ is positive, but the difference is smaller and not statistically significant.

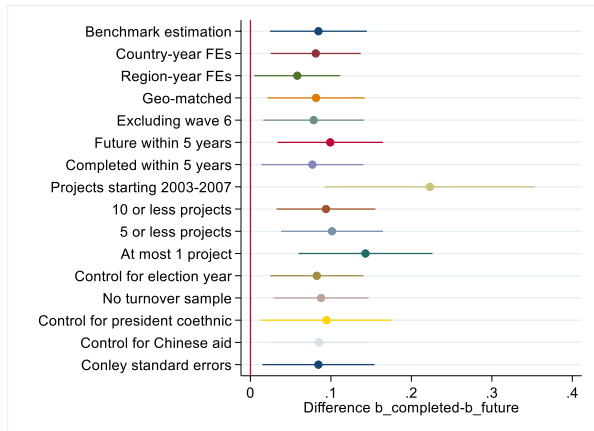
As we will see in the next section, this pattern is consistent across a wide range of alternative specifications.

4.2 Robustness tests

Figure 2 presents the results of robustness tests for overall World Bank aid (for the equivalent results in table format, see Table A14). Below we discuss each estimation in turn.

In the benchmark setup, country and year fixed effects account for variation in average levels of confidence in institutions across time and space. Central to our argument, however, perceived institutional quality likely varies systematically within as well as across countries, depending on differences in local governance and institutional infrastructure. Similarly, time trends in perceived institutional quality are likely to vary across as well as within countries, due to national or local policies and other developments. Controlling for temporal and regional variation involves a trade-off between losing variation and controlling for potential sources of bias. While fixed effects may absorb variation in perceived 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 variation unrelated to aid, that would otherwise bias

Fig. 2 Sensitivity estimations overall World Bank aid. Notes: Estimated effect with 95% confidence intervals; The estimations correspond to those presented in Table A14. Dependent variable is *Inst_index*; Treatment based on a 10 km cut-off around project sites



our aid estimates. Reassuringly, however, the benchmark result withstands controls for country-by-year fixed effects and sub-national region-by-year fixed effects.

Furthermore, the results are robust to dropping all respondents in enumeration areas further than 100 km away from World Bank projects sites. 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 (areas of) projects starting later differ systematically from (areas of) 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, completed projects start earlier and future projects start later on average.¹⁶ Hence, potential bias resulting from factors relating to project timing warrant careful consideration.

To begin with, at the time of wave 6 of the survey, 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, the observed pattern remains.

Furthermore, 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

¹⁶ The World Bank projects in the data start between 1995 and 2014. While the projects that are ongoing at the survey date start throughout this whole period, by construction, completed and future projects are restricted to projects ending prior to and starting after the survey date, respectively. For respondents in areas connected to completed projects, the first project within a 10 km cutoff starts between 1995 and 2007. By comparison, for respondents living in areas with only future project sites within the cutoff, projects start from 2003 onwards.

pre-treatment groups. Considering a narrower time bandwidth improves comparability, but comes at the cost of having to extrapolate from a smaller pre-treatment group.¹⁷ Nonetheless, we run estimations where we restrict 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 66 percent of the concerned group). The main results remain unchanged.

A similar argument can be made with respect to completed projects, i.e. that long time-bandwidths between project completion and survey date are a potential concern in terms of comparability. Hence, we also run estimations imposing a five-year time restriction on completed projects (45 percent of the concerned group fall within this category). Again, the main results do not change.

Another possible worry in this regard is that areas with projects starting early also tend to stand out in terms of the amount of aid they receive and have received over the years, from the World Bank and others. With aid clustering (see e.g. Aldasoro et al., 2010; Davies and Klasen, 2019; Fuchs and Öhler, 2021), areas with a long tradition of World Bank aid are not only likely to have seen multiple World Bank projects, but also to have been exposed to other sources of aid over an extended period. Since both project start dates and the number of projects a respondent has within the cutoff distance tend to differ by treatment category, such clustering could contaminate our treatment effect and bias our estimates upwards.

To explore whether the observed positive effect of having a completed as compared to a future World Bank project in the area is driven by completed projects starting earlier on average, we run estimations where we trim the sample in terms of project start dates. In particular, we focus on respondents connected to projects starting 2003–2007, which is the range of common support for project start years in the three treatment categories (see footnote 16). Reassuringly, the key results if anything become more pronounced.

Similarly, respondents connected to completed World Bank projects on average have a greater number of projects within the cutoff distance.¹⁸ We may thus worry that the estimated effect of living close to a completed project in effect picks up the effect of having many projects in the area. If aid begets more aid, as discussed above, having a great number of World Bank projects nearby could also pick up receiving more aid from other sources (not in our data). To explore if this is what drives our results, we run estimations where we trim the sample in terms of the number of projects within the cut-off distance. Restricting the sample to include only respondents with at most ten or five projects within the cut-off, thus excluding the most blatant ‘aid-darling’ areas, does not alter the results. Indeed, even if we restrict the sample to only include respondents in areas with at most one ongoing, future or completed project within the 10 km cut-off, the key result remains unchanged.

¹⁷ 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. 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 date to future projects starting within a year (Fig. A1).

¹⁸ While the majority of ever-treated respondents live in areas where 1–3 World Bank projects have started or will start within the cutoff distance, in some (primarily urban) areas the number of projects is significantly higher. For ever-treated respondents, the average number of projects within a 10 km cutoff is 7. For respondents connected to completed projects, the average number of projects within the cutoff is 12, and for respondents connected to ongoing projects, the equivalent figure is 10. For respondents connected to future projects, on the other hand, there are no current or past projects and on average only 2 upcoming projects part of the data.

Reassuringly, then, it does not seem that completed projects starting earlier on average and being located in areas traditionally attracting a lot of aid is what drives our results.

A related concern is strategic timing of aid projects. If the timing of implementation and completion of aid projects are related to political considerations (Kersting & Kilby, 2016; Marx, 2018; Williams, 2017), improved institutional ratings may be driven by policies relating to e.g. re-election efforts rather than by the aid projects themselves. To explore if our results depend on the electoral cycle, we run estimations where we control for it being an election year in the aid receiving country at the time of the survey. The results do not change.

Another potential issue in this regard is if changes in the political landscape during the surveyed period affect the comparability of our treatment and pre-treatment groups. In particular, if the party in power changes within the survey period, the selection process relevant to 'future' sites may differ from the selection process pertaining to sites of ongoing or completed projects. For this reason, we run estimations that include only survey waves where we, for our respective sample countries, see no party rotation between survey dates. Again, the results remain unchanged.

Furthermore, it is often suggested that African policy-makers tend to favor their own homelands and ethnic groups in the allocation of funds (Alesina et al., 2016; Dreher et al., 2019; Wantchekon, 2003). Considering the possibility that this may spill over into more favorable assessments of state institutions, we run estimations where we control for the respondent belonging to the same ethnic group as the country president at the time of the survey. Again, this does not alter the key results.¹⁹

Just as clustering of aid projects may contaminate the treatment group, substitution effects among projects of different origins may lead to contamination of the control group. While the principles and operating procedures guiding Western donors are relatively similar, recent years have seen the rise of new donors, partly adhering to different rules and objectives (Blair & Roessler, 2021). The most notable example is China.²⁰ If China does not coordinate with Western donors, or finances projects actively avoided by other donors, there may be substitution effects between Chinese and World Bank aid. Drawing on AidData's tracking of Chinese financial flows to Africa over multiple years, we are able to control for having an ongoing/completed/future Chinese project within the cutoff distance. The estimated effect of World Bank aid remains unchanged and we find no equivalent impact of Chinese aid.

Considering the high spatial resolution of the data, another potential worry is that spatial correlation in our treatment and outcome variables increases the risk of type 1 errors (see e.g. Kelly, 2019). The final estimation in Fig. 2 uses Conley standard errors (Conley, 1999) allowing for correlated standard errors within a radius of 500 km around each observation. Again, the key results do not change.²¹

¹⁹ As may be expected, co-ethnics of the president express a greater willingness to abide by the concerned institutions (for overall aid, the co-ethnic dummy parameter is approximately 0.06 and statistically significant at the one percent level).

²⁰ Recent evidence suggest that the demand-driven nature of Chinese development finance makes it particularly prone to elite capture (Dreher et al., 2019). Furthermore, a common argument is that since China has a policy of non-interference in the domestic affairs of recipient countries and rarely coordinates with Western donors, the presence of Chinese aid may undermine the conditionalities imposed by Western donors (see e.g. Bräutigam, 2011; Brazys et al., 2017; Hernandez, 2017).

²¹ Specifically, we use the Stata module 'acreg' developed by Colella et al. (2020) to compute standard errors corrected for arbitrary cluster correlation. Altering the distance cutoff, instead allowing for correlated errors within a radius of 250 or 750 km, does not change our key results.

Furthermore, in the Appendix we present results of estimations where we account for project quality (Appendix C), break down the treatment groups into sub-categories (Appendix D), restrict 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 (Appendix E), and use Coarsened Exact Matching rather than our benchmark spatial–temporal estimation strategy (Appendix F). The findings correspond with our benchmark results. Moreover, data permitting, we run equivalent estimations for selected alternative donors in selected recipient countries, the result suggesting both common ground and the existence of donor heterogeneity in results (Appendix G).

4.3 Projects focusing on institution building

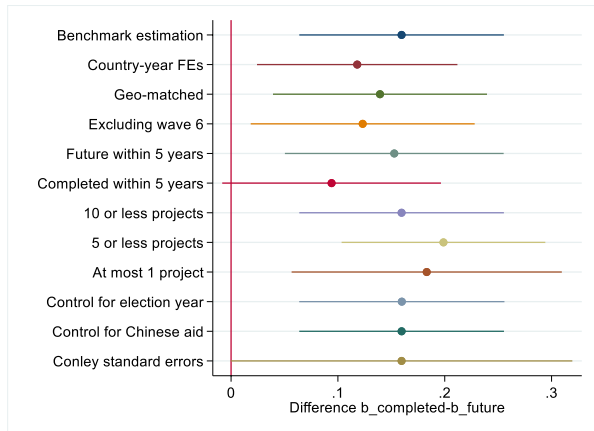
So far, we have considered all World Bank projects, irrespective of their specific objectives. Bearing in mind that aid is given for many purposes other than institutional development, in a second set of estimations we restrict our attention to World Bank projects that to a greater extent target institutional development. In particular, we focus on projects listing ‘Public Sector Governance’ and/or ‘Rule of Law’ as their major themes (see Section 3). Restricting the sample to countries with observations in all three treatment categories, gives an estimation sample of 29,097 respondents from four countries (Ghana, Kenya, Madagascar and Uganda). Across these four countries, there are 99 projects fulfilling our institutional aid criteria. Table 2 presents the key estimation results.

Table 2 Results for World Bank aid targeting institution building (10 km cut-off). Dependent variable is *Inst_index*

Variables	World Bank institutional aid
Ongoing	-0.031 (0.033)
Completed	0.037 (0.032)
Future	-0.122*** (0.036)
Diff. Ongoing-future	0.0917
F test: Ongoing-Future = 0	4.421
<i>p</i> value of F test: Ongoing-Future = 0	0.0356
Diff. Completed-future	0.160
F test: Completed-Future = 0	10.68
<i>p</i> value of F test: Completed-Future = 0	0.00110
Observations	29,097
R-squared	0.051

Robust standard errors (clustered by survey cluster) in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$; The regression includes baseline controls (age, age squared, urban, female) and year and country fixed effects

Fig. 3 Sensitivity estimations, projects targeting institutional development NEW. Notes: Estimated effect with 95% confidence intervals; The estimations correspond to those presented in Table A15. Dependent variable is *Inst_index*; Treatment based on a 10 km cut-off around project sites



Again, people living within 10 km of a completed project site report a 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 $\beta_{\text{completed}} - \beta_{\text{future}}$ is equivalent to 16 percent of a standard deviation on the institutional index. Furthermore, the difference $\beta_{\text{ongoing}} - \beta_{\text{future}}$ is again smaller, but for institutional aid actually statistically significant at the five percent level. Importantly, due to the different samples, the estimated effects are not directly comparable to those we obtain for overall aid.²² Nonetheless, it is interesting to note that the estimated effect of living near a completed institutional project is about twice the size of that for overall aid.

Figure 3 presents the results of robustness tests (for the equivalent results in table format, see Table A15). The fact that we here consider only a small subset of World Bank projects – 99 project sites across four countries as compared to 2641 project sites across 12 countries – means that we do not have sufficient variation in treatment status to run all robustness test involving further sample restrictions and additional break downs of aid into sub-categories.²³ Still, we can note that the results are robust across a broad range of the alternative specifications discussed for overall aid.

4.4 Duration of the treatment effect

To get a picture of how the treatment effect evolves after project completion, we run estimations where we interact the dummy variable for having a completed project

²² Running a regression on overall aid using the four-country institutional aid sample (ignoring the very small pre-treatment groups, i.e. respondents connected to future but no ongoing or completed project, we get in Madagascar and Ghana in this setup) the parameter difference $\beta_{\text{completed}} - \beta_{\text{future}}$ is not statistically significant.

²³ In particular, we do not have sufficient variation in all treatment categories to use region-by-year fixed effects (there are only two regions-years with variation across all three treatment categories), to focus only on respondents connected to projects starting 2003–2007 (there are no observations connected to future projects in three out of four sample countries), to use the no turnover sample (there are no observations connected to ongoing projects in Kenya and no observations connected to completed projects in Madagascar), and to control for belonging to the presidents ethnic group (not available for Ghana).

within the cut-off distance with an indicator for years-since-completion (see Table 3).²⁴ The interaction effect is positive, suggesting that the treatment effect grows with years since completion. Evaluated one year after project closing the estimated effect of overall World Bank aid is not statistically different from zero. After five years, however, it is 0.08 and statistically significant at the one percent level.²⁵ The pattern for aid targeted at institutional development is similar, but with a larger estimated effect (again twice the size of that for overall aid – 0.16 – evaluated five years after project completion).

These results are in line with the idea that real institutional development, and accompanying confidence in institutions, is a slow process, taking some time to materialize. On the other hand, it seemingly goes against the idea that the estimated effect is driven by signaling effects, since these should arguably be most pronounced when donors are visible, under the project implementation phase and soon thereafter.

4.5 Geographical reach of the treatment effect

Considering the impact of living in the immediate vicinity of a World Bank project, the treatment effects of both overall aid irrespective of focus area and aid focusing on institutional development were robust across a broad range of specifications. When considering the geographical reach of the estimated effects, however, the results focusing on projects to a greater extent targeted at institutional development are more encouraging.

Figures 4 and 5 compare the treatment effects we get when defining treatment as living within 10, 25 and 50 km of project sites, respectively. Whereas the estimated effect of living near a completed World Bank project, irrespective of focus, is only positive in a narrowly defined area around project sites, the estimated effect of living near completed projects focusing on institutional development has a wider geographical reach, remaining stable when using a 25 and a 50 km cut-off.²⁶

In sum, while the results indicate that even aid projects not necessarily targeted at institutional development come with measurable benefits for confidence in institutions, the effects are somewhat more pronounced, and with a wider geographical reach, when considering projects focusing more directly on institution building in estimations based on a smaller sample. The fact that it is completed projects rather than projects still under implementation that primarily come with positive effects, and that the estimated effect of completed projects grows with years since project completion, is in line with the idea that institutional development, and accompanying confidence in institutions, is a slow process, taking some time to materialize. Moreover, considering that signaling effects are likely to be most pronounced when donors are visible, during the project implementation phase, this seemingly goes against the idea that the estimated effect is driven by signaling effects.

²⁴ If there are several completed projects within the cutoff, this indicator refers to the project that was first completed.

²⁵ One may be concerned that a greater number of projects in areas with a longer tradition of aid drives this result. However, trimming the sample in terms of project start years (again considering start years in the range of common support for the three treatment categories, i.e. 2003–2007) and in terms of the number of projects within the cut-off (5 and 1 respectively), we observe the same pattern (see Table A16).

²⁶ Indeed, it remains intact when using a 75 and a 100 km cut-off as well (results available upon request).

Table 3 Evaluating treatment effects with years since project completion

Variables	Overall aid	Institutional aid
Any ongoing	-0.032 (0.020)	-0.029 (0.033)
Any completed	-0.045 (0.027)	-0.133** (0.054)
Any completed *Years since completed	0.010*** (0.004)	0.034*** (0.009)
Future	-0.075*** (0.027)	-0.118*** (0.036)
Diff. Completed-Future (1 year after completion)	0.0399	0.0198
F test Completed-Future (1 year after completion)=0	1.331	0.108
P value of F test Completed-Future (1 year after completion)=0	0.249	0.743
Diff. Completed-Future (5 years after completion)	0.0789	0.157
F test Completed-Future (5 years after completion)=0	6.61	11.21
P value of F test Completed-Future (5 years after completion)=0	0.0101	0.0008
Observations	73,640	29,097
R-squared	0.030	0.053

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

5 Conclusions

The aim of this study was to investigate the local effects of World Bank aid on perceived institutional quality in Sub-Saharan African aid receiving countries. We explored the local effects of overall World Bank aid as well as the local effects of World Bank aid to a greater extent targeted at institutional development.

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

Fig. 4 The estimated impact of overall World Bank aid when using different geographical cut-offs. Dep. var. is *inst_index*. Notes: Estimated effect with 95% confidence intervals Based on regressions using the benchmark set of controls, but altering the geographical cut-off around project sites, considering if respondents live within 10, 25 and 50 km of project sites

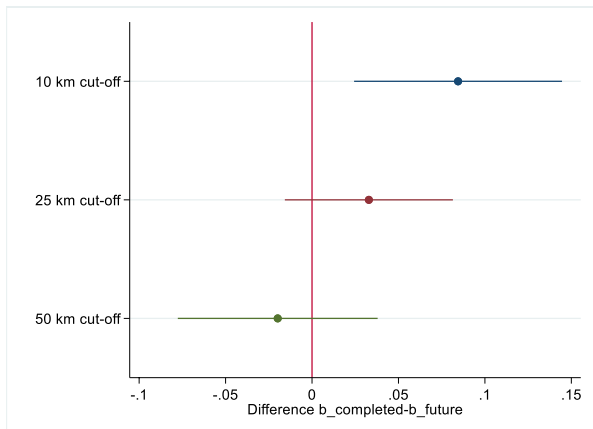
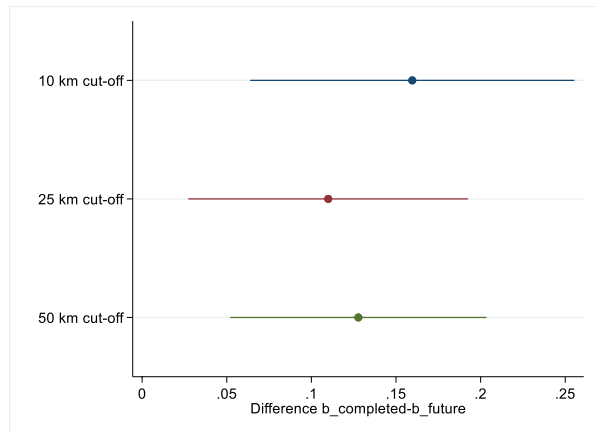


Fig. 5 The estimated impact of World Bank institutional aid when using different geographical cut-offs. Dep. var. is *inst_index* NEW. Notes: Estimated effect with 95% confidence intervals Based on regressions using the benchmark set of controls, but altering the geographical cut-off around project sites, considering if respondents live within 10, 25 and 50 km of project sites



picked up at the country level. Second, we are better able to account for the non-random allocation of aid. While identifying causal effects of course remains a challenge, 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 perceptions of 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, we address the concern that the aid effectiveness literature often aggregates 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. Our spatial–temporal estimation strategy, which compares the estimated effect of living near a site where a World Bank project was under implementation or finalized at the time of the interview, to that of living near a site where we know that a World Bank project will appear subsequently, helps us control for unobservable time-invariant characteristics that may influence selection into being a World Bank project site. While we cannot rule out the remaining presence of some selection issues, we can note that our key results are robust across a broad range of alternative specifications and sub-samples, including estimations accounting for project timing and drawing on alternative identification strategies (using coarsened exact matching and project fixed effects).

The positive estimated effect of World Bank aid on the reported willingness to abide by institutions applies if we consider overall aid independent of focus area. As may be expected, however, it is more pronounced when restricting our attention to projects that to a greater extent focus on institution building. For overall World

Bank aid, living within 10 km of a completed project comes with an institutional index score that is 8 percent of a standard deviation higher than if living within 10 km of a future project site. Focusing on World Bank projects targeting ‘Public Sector Governance’ and/or ‘Rule of Law’, the equivalent difference is 16 percent of a standard deviation.

Notably, the observed effects concern finalized projects, not projects still under implementation. Indeed, we find that the estimated effect grows with years since project completion, in line with the idea that building (confidence in) institutions takes time. Furthermore, whereas the effect of overall World Bank aid appears relatively local, the effect of projects to a greater extent targeting institutional development has a wider geographical reach.

The fact that we see no robust 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. Considering that signalling 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 perceptions of real institutional developments.

A thorough assessment of donor heterogeneity in the effects of aid on citizens’ confidence in institutions would require more complete geo-coded aid data. Based on the data at hand, 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 perceived institutional quality 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 different focus areas, donors and locations.

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Data availability With the exception of the geocodes in the Afrobarometer dataset, which users are required to apply for personally (information on how to do so can be found here), all data and replication codes needed to reproduce the analysis data is available from the authors on request.

Declarations

Conflicts of interest There are no conflicts of interest (in terms of funding, employment, financial and

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