Article

Receiving Foreign Aid Can Reduce Support for Incumbent Presidents

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Abstract

Foreign aid is thought to be useful, and therefore desirable, to recipient governments because it allows them to increase their support through the provision of goods or services. However, the effect of the provision of aid on vote choice has rarely been directly tested. I examine the effect of receiving foreign aid on incumbent electoral support in three African countries using a spatial difference in differences design. Surprisingly, receiving aid lowers support for incumbent presidents. I test two mechanisms that could produce this result and find support for a mechanism where aid reduces one's opinion of the incumbent, perhaps because aid does not live up to expectations. I discuss the implications of this result for research on the political effects of foreign aid and electoral accountability.

Keywords

foreign aid, electoral politics, distributive politics, retrospective voting

Foreign aid is thought to be useful, and therefore desirable, to recipient governments because it allows them to increase their support through the provision of goods or services. This claim assumes that voters will respond to the provision of aid-funded goods or services by increasing their level of support for the government. This assumption is reasonable and common and has rarely been tested. I test this assumption by examining how voters respond to the initiation of an aid project near their location using geotagged aid data and multiple geotagged survey waves from Nigeria, Senegal, and Uganda. I use a spatial difference in differences strategy, as in Knutsen et al. (2017). Causal identification in this strategy rests on the assumption that people who live near aid projects that were started just before the survey should be very similar to people that live near aid projects that will be started just after the survey. Contrary to expectations, I find that receiving aid lowers the probability of someone expressing a desire to vote for the incumbent by between 8 to 10 percentage points.

I propose three mechanisms that might cause aid to reduce incumbent support and am able to test two of them. I find little support for an explanation where aid empowers citizens and in doing so boosts opposition support. I find stronger support for a mechanism where aid fails to meet voter expectations and so leads to lower trust in the president and ruling party. Although I find that aid decreases trust, I find no evidence that people who receive aid are exposed to more corruption.

Although this paper's core finding is unexpected, it has some precedent. For example, Blattman, Emeriau, and Fiala (forthcoming) finds that participating in an anti-poverty program increases opposition support in Uganda and De Kadt and Lieberman (forthcoming) find that improvements in service provision lead to declines in incumbent support in southern Africa. Thus, while the present paper's results are surprising, they contribute to a growing number of surprising findings. When read together, this collection of work suggests that, at least in the context of low-income countries, the causal effect of economic improvements on political support may be more variable than is typically assumed.

Literature

Foreign aid is a transfer of resources to recipient governments or organizations in recipient countries. Recipient governments want aid because the additional resources that aid provides allow them to increase their support. In some regimes, this occurs when aid is appropriated by elites. For example, in Malawi, in the 1990s the country received aid to build schools, but procurement fraud by associates of members of the ruling party meant that few schools were built (Briggs 2015). Instead of funding public goods like schools, this aid was converted into private

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goods (cash) held by members of the ruling clique and their close associates. Because aid can be stolen or repurposed through fungibility, it could increase the durability of autocratic regimes in ways that are similar to an unencumbered resource like oil (Morrison 2007, 2011). Although there is at best mixed empirical support for the equivalence of aid and oil (Bermeo 2016), there is good evidence that aid increased the durability of autocratic regimes during at least the Cold War (Bermeo 2016; Dunning 2004).

Much aid, however, is not captured by elites and instead funds the creation of additional goods and services that are enjoyed by the citizens of recipient countries.[1] Consider again Malawi, where health aid has reduced malaria prevalence and improved self-reported health care quality (Marty et al. 2017). In this case, aid should help democratic leaders remain in power because it funds improvements in people's lives and so should increase incumbent support through a retrospective voting mechanism.[2] Donors often have an interest in ensuring that their aid effectively helps people in recipient countries (Bermeo 2017). To this end, they sometimes channel aid around recipient governments with weak institutions (Dietrich 2013). Citizens in at least one recipient country seem to be aware of donor efforts, and prefer development projects funded by donors to those funded by (and possibly captured by) their own government (Findley et al. 2017).

There is some evidence that aid helps leaders remain in power, possibly by increasing citizen support for incumbents. Briggs (2015) finds that increases in aid before elections help incumbents win reelection. Jablonski (2014) finds that receiving aid increases votes for the incumbent in Kenya. Licht (2010) finds that democrats benefit from aid, with the effect being most pronounced when democrats first take office. Yuichi Kono and Montinola (2009) find that aid improves leader survival for both dictators and democrats, though the mechanisms are somewhat different and current aid helps democrats most. Although not examining the effect of aid directly, Harding (2015) shows that road improvements increase incumbent support in Ghana.

Finally, leaders of both donor and recipient countries target aid as if it is a politically useful resource. Recipient governments take care to target aid to places where they think it is most politically valuable to them (Briggs 2014; Jablonski 2014; Masaki 2018). Donor countries give more aid to recipient countries when they are politically aligned with the donor and in an election year (Faye and Niehaus 2012). Even when leaders in recipient countries have no influence on aid targeting, they still often try to claim credit for an area getting aid (Cruz and Schneider 2017). In explaining this strategic action, all authors assume that aid is a desired good whose allocation is expected to cause an increase in support for the incumbent government.

Although the claim that aid will increase incumbent support is commonly assumed, its universality has recently been challenged. De Kadt and Lieberman (forthcoming) find that increases in service provision lead to decreases in incumbent political support in South Africa, Botswana, Lesotho, and Namibia. Blattman, Emeriau, and Fiala (forthcoming) examined the electoral effects of an aid-supported antipoverty program in Uganda and found that recipients of the program where economically better off but were also more likely to campaign for and vote for the opposition party.

The present article examines the effect of aid on incumbent support using individual-level survey data from multiple time periods and multiple countries. In line with the results of De Kadt and Lieberman (forthcoming) and Blattman, Emeriau, and Fiala (forthcoming), I find that receiving aid lowers the likelihood that one expresses a desire to vote for the incumbent president. In the following section, I propose a number of mechanisms that may explain this result.

Aid and Incumbent Support

The main contribution of this paper is to present an unexpected result. In proposing possible explanations for this result, it is worth considering both those that are unique to foreign aid and those that are due to a surprising relationship between improvements in welfare and voting behavior more generally. It should be emphasized that this section is speculative, and was written in response to the puzzling results below. Thus, the tests of these mechanisms should be understood as exploratory rather than confirmatory.

I first focus on a mechanism that is unique to foreign aid and then explain two mechanisms that may apply to other forms of service delivery as well as aid. The aid-specific mechanism is that of foreign dependence. Aid is unlike other service improvements because it is foreign-funded. It thus seems possible that receiving aid could delegitimize incumbent leaders or parties if it leads people to view their government as incapable of providing for their citizens without outside support. If receiving foreign aid causes people to view their government or leaders as less legitimate or more dependent, then it may reduce an incumbent's electoral support. Despite the coherence of this mechanism, evidence for it is lacking. Dietrich and Winters (2015), for example, show that in India learning that a health project was foreign-funded leads to a statistically insignificant decrease in support for the Prime Minister. In Bangladesh, a similar treatment has no effect on the level of confidence in the national government and increased confidence in local government (Dietrich,

Mahmud, and Winters 2018). Dolan (2017) tests one explanation for this divergence between theory and results, which is that people in low-income countries may expect their government to receive foreign assistance and so they will not see the receipt of such assistance as a sign of incompetence or neglect. She finds support for this explanation in Kenya.

It is also possible that there is nothing special about aid, but that receiving service improvements in general could lead voters to increase opposition support (De Kadt and Lieberman, forthcoming). Although this idea stands in contrast to standard accounts of economic voting, it is not wildly implausible. For example, voters close to subsistence may be more easily enmeshed in clientelistic exchanges of resources for political support than other voters. If aid helps people move away from subsistence then it could weaken these ties and so allow people to increase their support for opposition parties, which typically lack the resources of the party in power. I refer to these kinds of mechanisms, where aid frees people from systems that reproduce incumbent support, as an empowerment mechanisms.

Finally, it could be that experience with aid projects, or other government projects aimed at improving services, exposes citizens to a range of pathologies that lower incumbent support. For example, seeing service extensions being implemented may expose people to corruption, a mechanism noted in De Kadt and Lieberman (forthcoming). Alternatively, the ways that aid was implemented may cause disillusionment. For example, people may feel that their preferences were not adequately considered in the planning or implementation of a service extension or aid project, and this could reduce incumbent support. It could also be the case that people's expectations for an aid project or service extension may be higher than what is delivered, and so even if aid works it may fail to live up to expectations. I refer to this collection of mechanisms where aid harms one's opinion of the incumbent as disillusionment mechanisms. In these mechanisms, aid does not empower citizens to support the opposition but rather lowers their view of the incumbent government or leader. The next section discusses the data under study and explains the paper's identification strategy.

Data and Identification

To examine the effect of receiving foreign aid on voting intentions, I make use of geotagged survey data from Afrobarometer (2018a; BenYishay et al. 2017) and data on the location of aid projects from AidData (Strandow et al. 2011).[3] I examine multiple rounds of Afrobarometer surveys in three African countries, and the unit of analysis is the survey respondent. Afrobarometer survey respondents are nested within enumeration areas, which are themselves nested within first-level administrative (ADM1) regions. Respondents are selected randomly within enumeration areas, and enumeration areas are randomly selected within strata defined by region and whether or not a populated area is rural or urban with probability proportionate to population size.[4]

My dependent variable comes from Afrobarometer and is based on a question that asks respondents "If a presidential election were held tomorrow, which party's candidate would you vote for?" This question was asked starting in 2005, in Afrobarometer round 3, and so I do not use surveys before round 3. From this question, I create a binary variable that takes a value of one if the respondent would vote for the incumbent party's candidate and zero if the respondent would vote for any other party's candidate (and is otherwise missing). I also draw from Afrobarometer a number of control variables which are discussed later.

AidData provides information on the location of aid projects, and my key independent variables are based on measuring the distance from aid projects to Afrobarometer enumeration areas. I use these distance measures to create a variable that marks the people that are in close proximity to aid projects that started just before each Afrobarometer survey. I limit my sample of countries to those with an Aid Information Management System (AIMS). These systems are incorporated into recipient country government planning processes and allow one to track much of the aid flowing into a country. Only Nigeria (AIMS v.1.3.1), Senegal (AIMS v.1.5.1), and Uganda (AIMS v.1.4.1) had Afrobarometer surveys during the time in which they also had an AIMS, and so I focus on these three countries.[5]

Nigeria's AIMS tracked aid from 1988 to 2014. It covered twenty-eight different donors and records 1843 locations of aid projects. During this time, Afrobarometer ran surveys in Nigeria in 2005 (round 3), 2008 (round 4), and 2012 (round 5). Thus, Nigeria has three rounds of data. Olusegun Obasanjo was Nigeria's president at the start of the period until 2007, when he was term-limited and his hand-picked successor Umaru Musa Yar'Adua won the election. Umaru Musa Yar'Adua held power until his death in 2010, when the Presidency shifted to his then-Vice President Goodluck Jonathan who held power until the end of the period. Throughout this entire period (indeed from 1999 to 2015) the Presidency of Nigeria was held by the People's Democratic Party.

Senegal's AIMS tracked aid from 2000 to 2012 and records 1,124 project locations from seventy-nine donors. Senegal had Afrobarometer surveys in 2005 (round 3) and 2008 (round 4). Senegal's round 5 survey was carried out in 2013 and so was not included. Abdoulaye Wade

was the President of Senegal for the entire period of study, being reelected in 2007.

Finally, Uganda's AIMS includes aid from 1978 to 2014 and includes 2,426 project locations from fifty-six donors. Uganda had Afrobarometer surveys in 2005 (round 3), 2008 (round 4), and 2011 (round 5).[6] Throughout the period, Yoweri Museveni was the President of Uganda, being reelected in 2006 and 2011. In sum, I have eight country-rounds of Afrobarometer survey data and each country-round occurs during a time when the country's aid was being tracked in an AIMS. No country experienced a rotation of the party in power during the period under study.

Although AIMS data are currently the best source for geotagged information on foreign aid, they have a number of limitations. First, donor coverage is not complete. Second, the level of precision of the geocoding is uneven, with some projects only being geocoded to crude levels like ADM1 regions. These two issues mean that that while the AIMS data sets used in the present paper include many more aid projects than the data sets used in past work (e.g., Briggs 2014; Jablonski 2014), they are not exhaustive. This makes it difficult to be absolutely certain that an area received no aid, for example. Third, the data sets under analysis are built up from government data. This means that the quality of the data sets fundamentally depends on the buy in, relationships with donors, and technical skills in the recipient-government agency that manages the AIMS. Although this does not imply that data quality will inherently be low, it should probably instill some skepticism in the data.[7]

Finally, case selection was driven by data availability and this means that generalizing from the three countries under study to broader groupings like "aid recipients" should be done cautiously. However, the cases under study include an East African ex-British colony, a West African ex-British colony, and a West African ex-French colony. Thus, if we see consistent patterns across these diverse cases then we can perhaps read them as indicative of broader trends among aid recipients in sub-Saharan Africa. I find no evidence that aid increases incumbent support in any of these countries. This should make us less likely to believe that aid increases incumbent support in sub-Saharan Africa.

Identification

I identify the effect of aid on voting intentions using a spatial difference in differences design. Such an approach has been used to examine the effect of mining on corruption (Knutsen et al. 2017), the effect of Chinese aid on corruption (Isaksson and Kotsadam 2018a), the effect of mining on domestic violence (Kotsadam, Østby, and Rustad 2017), and the effect of foreign aid on infant mortality (Kotsadam et al. 2018). This approach takes advantage of the fact that I have temporal and spatial information on both project aid and the Afrobarometer enumeration areas.

Closely following the work of Kotsadam and coauthors, I create three exclusive groups of respondents. The first group is people who are near an aid project that was started in the year of the survey or the year before the survey. The second group is made up of people who are not near a present aid project but are near an aid project that will start one or two years after the survey. The third group includes all remaining people, who are by definition neither near a present nor future aid project. Thus, I have a group that is presently receiving aid, a group that will soon receive aid, and a group that does not receive any aid in the period just before or after each the survey.

I then make two comparisons. First, I compare those that are near a present aid project and those that never receive aid. This difference captures any causal effect of aid plus any selection effect. Second, I compare those that are near a future aid project and those that never receive aid. This difference captures only a selection effect, as the aid project has not yet started and so cannot have had a causal effect. Finally, taking the difference between these two differences allows one to subtract the selection effect from the combined selection and causal effect, leaving behind the causal effect of aid on voting intentions.

To implement the above procedure, I restrict both AidData project locations and Afrobarometer enumeration areas to places that are geocoded with a level of precision of three or below. This drops enumeration areas or aid projects that are only crudely geocoded and keeps those that are coded to the second-level administrative region or better. The pooled analyses use the standard cross-national Afrobarometer survey weights, but they have been reweighted so that each country contributes equally to the pooled results even though Senegal has one round of fewer data than does Uganda or Nigeria.

The central assumption supporting this approach to identifying causal effects is that the process leading to the selection of places to receive an aid project will be the same in the time period just around a survey. The most obvious way that this would be violated is if the incumbent president loses an election during the period under study. As noted above, this is not a problem in my sample of countries as no party lost an election during the periods under study. The "same selection process" assumption could also be violated by short-term fluctuations in need or political support over space. For example, if a drought occurred in an area that was full of regime supporters just after an Afrobarometer survey then aid may flow there and so the differences in differences would incorrectly show that aid caused a decrease in regime support. However, these exogenous fluctuations should be

Table 1. Main Results.

	(1)	(2)	(3)	(4)
Present aid, 50 km	−0.02 (0.03)	−0.04 (0.03)	−0.04 (0.03)	−0.04 (0.05)
Future aid, 50 km	0.07** (0.03)	0.06* (0.03)	0.06* (0.03)	0.05 (0.06)
Difference in differences	−0.10	−0.09	−0.09	−0.08
p value, present-future = 0	0.000	0.000	0.000	0.001
Mean dependent variable	0.59	0.59	0.59	0.61
Country-round fixed effects	Yes	No	No	No
Region-round fixed effects	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Sample	All	All	All	Geomatch
Observations	11,309	11,309	11,241	7,947
Enumeration areas	2,071	2,071	2,071	1,470

Robust standard errors clustered on enumeration areas in parentheses. All models include survey weights that are modified so each country counts equally. The *p* values of differences in differences are based on *F* tests.
*$p < .1$. **$p < .05$. ***$p < .01$.

averaged away if one compares over more countries or more survey rounds. I compare across three countries with a total of eight country-survey rounds, and so it is very unlikely that the results are driven by such fluctuations.

All regressions include either country-round or region-round fixed effects. Comparing within region-rounds increases the likelihood that I am comparing across groups of people that are similar except for their aid status, but many region-rounds lack observations in at least one of the three aid categories (near present aid, near future aid, not near aid) so relatively few observations contribute identifying variation when using region-round fixed effects. This is much less of an issue when using country-round fixed effects, but in this case I am less likely to be comparing groups of people that are similar aside from their aid status because I allow comparisons across regions within the same country and survey round. Neither approach is obviously better and so I report results from both approaches.

The people that are selected to receive aid in the present should be quite similar to those selected to receive in the near future aid, especially when including region-round fixed effects. Nevertheless, I also show that the results are stable when I include controls for age, age-squared, gender, and whether or not the enumeration area is urban, as well as interactions between all of the aforementioned control variables and country dummies so that the effect of each variable can vary over countries.[8]

I also show that the results are robust to dropping all enumeration areas that are further than 100 km from either future aid or present aid. This sample restriction is useful because it controls for unobserved factors that are similar over space but may vary within country- or region-survey rounds. Spatially matching in this way reduces the sample by about three thousand respondents nested within about four hundred enumeration areas, but it more closely ensures that I am comparing groups of people that are similar except for their treatment status.

Results

Table 1 presents the main results. Following prior research (e.g., Isaksson and Kotsadam 2018a, 2018b; Knutsen et al. 2017; Kotsadam et al. 2018), I start by using a 50 km cutoff for marking someone as being near aid projects. The coefficient for present aid shows the difference in incumbent support between people who were further than 50 km from a present or future aid project and those that were within 50 km of a present aid project, recalling that a present aid project is one that started in the survey year or the year before the survey. The coefficient for future aid shows the difference between people who were not within 50 km of present or future aid and those that were within 50 km of an aid project that was going to be started either one or two years after the survey. The key result is then the difference between these two differences, which reveals the causal effect of receiving aid on whether or not a respondent expresses a desire to vote for the incumbent president.

The coefficient for future aid reveals selection effects and shows that within countries (model 1) aid flows disproportionately to people who express a desire to vote for the incumbent president. This reinforces prior work finding that aid flows to incumbent supporters (Briggs 2012, 2014; Jablonski 2014). However, it should be emphasized that this result is a mere correlation and could easily be driven by unobserved variables influencing both aid targeting and incumbent support.

Turning to the causal effects, across the various specifications, people that receive aid in the year of the survey or the year before are 8 to 10 percentage points less likely to express a desire to vote for the incumbent president

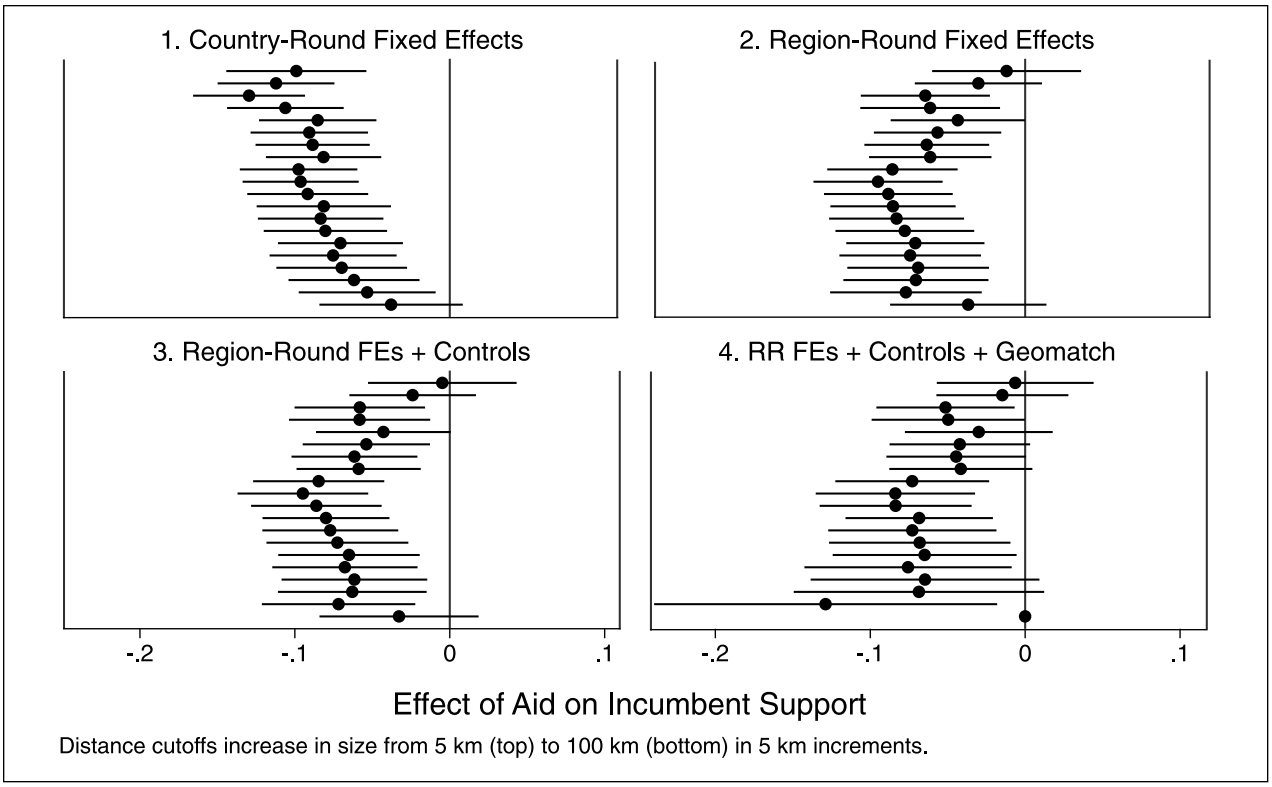

Figure 1. Robustness of pooled results over distance cutoffs.

than people who will receive aid over the following two years. This result holds in minimal models with only country-round (model 1) or region-round (model 2) fixed effects. It also holds when controls are added (model 3) and when the sample is restricted by dropping enumeration areas that are not within 100 km of both a present aid project and a future aid project (model 4). As noted above, this sample restriction implies that the base-level enumeration areas that were not within 50 km of either a present or future aid project were still within 100 km of both. It also implies that every enumeration area marked as being within 50 km of a present aid project was also within 100 km of a future one and vice versa. This sample restriction should thus increase balance on any omitted variables that cluster in space.

The results presented in Table 1 are robust across a wide range of cutoff distances. To demonstrate this robustness, I ran all of the models from Table 1 using distance cutoffs starting at 5 km and increasing in 5 km increments up to 100 km. The differences in differences point estimates and 95 percent confidence intervals are graphed in Figure 1. When comparing across country-rounds, there is a clear fading of the causal effect as cutoff distances increase. This is expected as larger cutoff distances include more people that are far enough from a project site that they probably should not be considered as having received aid. When comparing within regions this effect is mitigated.[9] Adding control variables does little to the results and the geomatching procedure mostly serves to widen the confidence intervals, which is expected as it also drops more than a quarter of the observations.[10] Overall, respondents who are near an aid project that was just started are between 5 to 10 percentage points less likely to support their incumbent president than respondents who are near an aid project that is soon to begin.

The results are also robust to a number of other modifications, which are presented in the online appendix. First, I reproduced Table 1 but used a logistic model instead of a linear probability model. The results are substantively very similar. Second, I estimated models 1 and 2 in Table 1 using Conley standard errors (Conley 1999) instead of clustering on the enumeration area.[11] The difference in differences results remain significant at $p < .05$. Third, the dependent variable has a large number of missing values, likely reflecting the sensitivity of asking people how they would vote. It is possible that people who recently received aid might be differently likely to reveal their voting intention than other people, and this could lead to bias. I tested the effect of such selective nonresponse by imputing first all ones (all incumbent support) and then all zeros to the missing respondents and then I reran the analyses used to produce models 1 and 2 in

Table 2. Results by Country.

	(1)	(2)	(3)
	Nigeria	Senegal	Uganda
Panel A: Country-round fixed effects			
Present aid	−0.03 (0.03)	−0.06 (0.04)	−0.06** (0.03)
Future aid	0.14*** (0.03)	0.10** (0.05)	−0.03 (0.03)
Difference in differences	−0.18	−0.16	−0.03
p value, present-future = 0	0.000	0.000	0.266
Panel B: Region-round fixed effects			
Present aid	−0.07* (0.04)	−0.03 (0.05)	−0.09*** (0.03)
Future aid	0.07** (0.04)	0.06 (0.05)	−0.07*** (0.03)
Difference in differences	−0.15	−0.10	−0.02
p value, present-future = 0	0.001	0.076	0.443
Mean dependent variable	0.48	0.63	0.67
Aid cutoff (km)	60	20	20
Controls	No	No	No
Sample	All	All	All
Observations	4,474	1,487	5,348
Enumeration areas	877	295	899

Robust standard errors clustered on enumeration areas in parentheses. All models include survey weights. The *p* values of differences in differences are based on *F* tests.
*$p < .1$. **$p < .05$. ***$p < .01$.

Table 1 using these new dependent variables. The results are again quite similar in terms of statistical significance. Substantively, the difference in differences effect sizes in these imputed models ranges from −0.04 to −0.09.

Next, I separately reproduce Table 1 using only the four country-rounds where the Afrobarometer survey occurred in either the same year as a Presidential election or in the year after an election, and then using only the four rounds where it did not. This is useful as the "same selection process" assumption could be violated by President's using different pre- and postelection resource targeting strategies. The negative effect of aid on incumbent support is present in both groups. However, the group with surveys that fall on top of elections shows somewhat stronger negative effects of aid on incumbent support. This is driven by an increase in the coefficient for future aid, suggesting that incumbents are more likely to target aid to areas of support immediately following an election. However, in interpreting these results it is useful to keep in mind that splitting the county-rounds in this way introduces imbalance across countries.[12] Thus, the main takeaway is simply that the core negative result exists across this sample split.

Both the aid data and Afrobarometer data used in the present paper were coded by AidData and in both cases I restricted the data sets to all aid projects or enumeration areas with a precision code of three or below. This could be problematic as places with a precision code of three are given the GPS coordinates of the centroid of the second-level administrative unit in which they are known to reside. Thus, an aid project and an enumeration area that are both coded to the same second-level unit will appear to be on top of one another when in reality they are simply somewhere within the same second-level unit. This quirk should not cause bias unless it varies across the present and future aid categories, but to ensure that there is no bias I reran the results of Table 1 but dropped all enumeration areas that were less than 0.5 km from an aid project.[13] The results are again very similar.

Finally, I replicated Table 1 but dropped regions holding capital cities. This ensures that these generally heavily populated regions are not driving the results. The results remain very similar without capital regions.[14]

The pooled results are informative for two reasons. First, as noted above, by combining an East African ex-British colony, a West African ex-British colony, and a West African ex-French colony, the pooled results provide a crude representation of the effect of aid on incumbent support across much of Africa. Second, by adding more country-rounds of survey data they help to reduce the fears that any one special country-round is driving the results. Nevertheless, it is also useful to separately examine each country to see if we can find variation in the extent to which aid harms incumbent support.

Examining countries separately reveals some country-level heterogeneity, but the results never reveal a point estimate suggesting that receiving aid increases incumbent support. Panel A of Table 2 shows the results with survey round fixed effect and panel B shows the results with region-survey round fixed effects. When examining

Table 3. Empowerment Results.

	(1)	(2)	(3)
	Interest	Raise issue	Protest
Present aid, 50 km	0.02 (0.05)	−0.04 (0.09)	0.03 (0.05)
Future aid, 50 km	0.02 (0.05)	0.00 (0.08)	0.08* (0.04)
Difference in differences	0.00	−0.05	−0.05
p value, present-future = 0	0.937	0.429	0.099
Mean dependent variable	1.84	1.88	0.64
Region-round fixed effects	Yes	Yes	Yes
Controls	No	No	No
Sample	All	All	All
Observations	16,378	16,329	16,045
Enumeration areas	2,079	2,079	2,079

Robust standard errors clustered on enumeration areas in parentheses. All models include survey weights that are modified so each country counts equally. The *p* values of differences in differences are based on *F* tests.
*$p < .1$. **$p < .05$. ***$p < .01$.

the effect of aid by country, I pick distance cutoffs that are more appropriate to each country. In particular, Nigeria has a larger distance cutoff while Senegal and Uganda have smaller cutoffs. The results are not particularly sensitive to the choice of cutoffs.[15] I do not subset the data as in model 4 of Table 1 because these tests already have much smaller sample sizes.

Looking first at selection effects, aid flows to the people within countries that are regime supporters in both Nigeria and Senegal. Uganda shows no statistically significant bias in terms of selection to receive aid within the entire country, though within subnational regions aid seems to favor opposition supporters. This latter effect could be seen as limited support for the opposition-favoring finding in Masaki (2018).

Turning to causal effects, the opposition-boosting effect of aid is strongest in Nigeria and weakest in Uganda, where it is not significantly different from zero. The lack of an effect in Uganda may be due to the fact that, unlike Senegal and Nigeria, Uganda is not electorally competitive. Lacking the ability to change the president through voting, Ugandans may be more likely to ignore information that would influence the vote choices of Nigerians or Senegalese, though this is post hoc conjecture.[16]

In sum, receiving foreign aid reduces the likelihood that someone will express a desire to vote for their incumbent president. This effect is present in each country under study, though the effect is not statistically significant in Uganda, and the pooled effect suggests that receiving aid lowers the odds of someone voting for their incumbent president by about 8 percentage points. The effect holds across a wide range of distance cutoffs and it survives a fairly large sample reduction aimed at increasing the comparability of respondents, as well as a number of other robustness tests.[17]

Mechanisms

This paper has presented the puzzling finding that receiving aid lowers incumbent support. There are at least three broad mechanisms that might cause aid to have this effect. First, if respondents see that their government is reliant on aid to provide basic goods or services it may delegitimize the government. Second, there could be an empowerment mechanism whereby aid enables people to more freely express their political preferences. Third, there could be a disillusionment mechanism whereby exposure to an aid project leads to disappointment with the government.

Afrobarometer includes questions that allow me to offer preliminary tests of the latter two explanations. I first test the empowerment mechanism. To do this, I draw on questions that measure if people who receive aid are more likely to: express an interest in public affairs, get together with others to raise an issue, and join a demonstration or a protest. The first question probes the idea that an increase in economic well-being may allow one to access more media, including political media, and so may allow one to develop an interest in public affairs. The second question examines if receiving aid increases the likelihood that one will work with others to make public claims. The third question asks specifically about one's willingness to attend a protest. Each question examines the underlying idea that if aid improves people's lives it may open opportunities for people to challenge the party in power. I use the same setup as model 2 in Table 1 but swap the incumbent support dependent variable for each of the three empowerment variables above.[18]

The results are in Table 3 and show no support for an empowerment mechanism. People who receive aid are no more likely to express an interest in public affairs, no more likely to be together with others to raise an issue,

Table 4. Disillusionment Results.

	(1)	(2)	(3)	(4)	(5)	(6)
	Trust: President	Trust: Ruling party	Trust: Opposition party	Corrupt: President	Performance: President	Manage economy
Present aid, 50 km	−0.04 (0.06)	−0.09 (0.06)	−0.05 (0.06)	−0.00 (0.05)	−0.02 (0.04)	−0.07* (0.04)
Future aid, 50 km	0.07 (0.06)	0.02 (0.06)	−0.12** (0.06)	−0.04 (0.05)	0.06 (0.04)	0.01 (0.04)
Difference in differences	−0.11	−0.11	0.08	0.04	−0.07	−0.07
p value, present-future = 0	0.018	0.005	0.090	0.307	0.025	0.019
Mean dependent variable	1.66	1.42	1.24	1.43	2.57	2.03
Region-round fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No	No
Sample	All	All	All	All	All	All
Observations	16,067	15,897	15,539	14,379	16,033	16,136
Enumeration areas	2,079	2,079	2,079	2,078	2,079	2,079

Robust standard errors clustered on enumeration areas in parentheses. All models include survey weights that are modified so each country counts equally. The *p* values of differences in differences are based on *F* tests.
*$p < .1$. **$p < .05$. ***$p < .01$.

and no more likely to protest.[19] These results suggest that aid is not enabling people to take more of an interest in public affairs, making people willing to join with others to raise issues, or making people more willing to join demonstrations or protests. Although these are admittedly only partial tests of the empowerment mechanism, finding no support on any of these tests should reduce our belief that an empowerment mechanism is causing aid to lower incumbent support.

I next test the disillusionment mechanism. I do this using variables that ask respondents if they trust the president, trust the ruling party, trust opposition parties, think the president is corrupt, approve of the president's performance over the past year, and think the government has done a good job managing the economy. If a disillusionment mechanism is at work, then I would expect aid recipients to trust the president and ruling party less, think the president has done a worse job in office and the government has done a worse job managing the economy. They may also trust the opposition more. Finally, experiencing corruption while aid was being implemented and believing that this corruption is tied to the president is one specific version of the disillusionment mechanism.

The results in Table 4 show that the disillusionment mechanism has some empirical support. People who receive aid are less likely to trust the president and less likely to trust the ruling party. These effects are small but meaningful, amounting to about a 7 percent decrease from either mean trust value. Recipients are more likely to trust the opposition party, though this result is only significant at $p < .1$. The more interesting results from the test of opposition support is the selection effect, which reveals that aid targets places that are less trusting of the opposition. Aid recipients think the president did a worse job over the past year and that the government did a worse job managing the economy. I find no evidence that aid recipients are more likely to believe that the president is corrupt.

This section examined possible mechanisms that would cause people who receive aid to lower their support for the incumbent president. I found no evidence supporting the idea that aid empowers people and so increases their willingness to support the opposition. I found stronger support for the idea that aid can lead to disillusionment with the party in power, lowering one's trust in the president and ruling party and lowering one's opinion of how the government manages the economy. I lack the data to test claim that aid may reveal foreign dependence that delegitimizes one's government in the eyes of voters. As noted above, the tests of these mechanisms should be seen as exploratory rather than confirmatory and as preliminary rather than final. Nevertheless, the mechanism tests suggest that aid is reducing incumbent support by lowering people's opinion of the incumbent president rather than by removing economic constraints that limit the ability of people to express themselves politically.

Discussion

This paper presented the surprising finding that receiving foreign aid lowers incumbent support. The results hold when the analysis uses country-round fixed effects, region-round fixed effects, when control variables are added, or when the data are limited so that all enumeration areas are within 100 km of present and future aid. The results also hold across essentially the entire range of plausible distance cutoffs for determining if an enumeration area is near aid.

The paper put forward three explanations for why aid might lower incumbent support and tested two of them. I find no support for an empowerment mechanism, whereby receiving aid enables people to more freely express their political opinions. The disillusionment mechanism, where aid fails to meet local expectations and so reduces one's opinion of the government, receives more support. Aid recipients express less trust in the president and ruling party than people that are about to receive aid. Aid recipients also think the president did a worse job in office and that the government did a worse job managing the economy. Receiving aid did not increase perceptions of corruption.[20] It should be noted that the disillusionment mechanism can exist whether or not aid actually led to improvements in people's lives, as it depends on an aid project not meeting local expectations rather than aid not meeting its own goals. Nevertheless, if the disillusionment mechanism is causing a decline in incumbent support then projects that are less effective at achieving their goals should, all else equal, cause a larger decline in incumbent support.[21] I lack the project outcome data required to test this proposition, but testing it would be a useful future contribution as it would be a stronger test of the claim that disillusionment with aid leads to declines in incumbent support.

One alternative explanation for this paper's results is that voters are being told about pending future aid projects and that this knowledge increases incumbent support. We can refer to this as an anticipation effect. If anticipation effects exist, then the future aid group will not capture only selection effects but will also show the effect of receiving (true) information about pending aid. This explanation for the results seems unlikely for two reasons. First, for this mechanism to work voters need to trust the representatives of high-level politicians when they claim that aid will soon be coming to the voters' location. Such trust is likely to be lacking in the countries under study, where the stereotype is that politicians often lie about plans for future development projects. Second, even if anticipation effects exists, they will not produce a negative causal estimate for the effect of aid on incumbent support unless the anticipation effect fades quickly and the effect of actually receiving aid on incumbent support is zero or negative (or significantly smaller than the anticipation effect). Thus, it seems unlikely that the results are due to anticipation effects on the part of voters.

The fact that receiving aid can reduce incumbent support has implications for our understanding of electoral accountability. Standard accounts of the differences in service delivery between democracies and autocracies stress the link between a government's electoral imperative to positively impact many voters and the better provision of public goods in democracies (Brown 1999; Lake and Baum 2001; Min 2015).[22] If voters in some contexts respond to service extensions by shifting their supporting away from the incumbent, then one fundamental utilitarian argument in favor of democracy—that it leads to better service provision for most people—is weakened. It is therefore important to understand where and when this kind of effect exists.

Despite the robustness of the results, this paper should still be viewed skeptically as its results stand in contrast to most prior research. In a review of distributive politics, Golden and Min (2013, 84) note that "studies overwhelmingly find that incumbent politicians are rewarded by voters for distributive allocations." However, recent work by De Kadt and Lieberman (forthcoming) and Blattman, Emeriau, and Fiala (forthcoming) has found results that are broadly consistent with those of the present paper, suggesting that there may be exceptions to the general finding that improvements in the quality of life of voters lead to increases in incumbent support. As noted above, an important goal of future research is to try to understand the conditions under which this perverse relationship between welfare improvements and democratic accountability exist.

The results also generate many new questions for research on the politics of foreign aid. If aid can reduce incumbent support, then why do recipients accept it? When does aid produce these effects, and when does it produce "normal" results? Does this perverse relationship between aid and incumbent support exist outside of Africa? Again, future research could do much more to examine these and other questions that delimit the scope of this paper's finding.

Conclusion

Drawing on data from three African countries, this paper showed that receiving foreign aid can reduce incumbent support. Although the main contribution of the paper is to document this anomalous finding, it also offered preliminary tests of two mechanisms that may explain this puzzling result. Although I found no support for an empowerment mechanism, I found support for a disillusionment mechanism. People that received aid expressed less trust in the president and ruling party and expressed a lower opinion of the president's performance in office or the government's ability to manage the economy.

Author's Note

Access to the data under study are restricted by Afrobarometer. Data and replication code can be made available by the author on request after one has secured clearance from Afrobarometer.

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Notes

1. For an explanation of why fungibility concerns around aid are likely overblown, see Altincekic and Bearce (2014).
2. African voters exhibit retrospective voting behavior (Briggs 2012; Harding 2015; Jablonski 2014; Posner and Simon 2002; Youde 2005). See Ashworth (2012) for a discussion of how retrospective calculations influence prospective voters. Note that aid can affect voting behavior in these models regardless of whether or not voters realize that improvements in their lives are foreign-funded.
3. The strategy of spatially linking Afrobarometer enumeration areas (EAs) to the location of aid projects is similar to that used in Briggs (2018).
4. For more information about sampling, see Afrobarometer (2018b).
5. Malawi had a somewhat similar aid management system in place but it lacks the standardized reporting of the other countries and so was not included. Although Nigeria, Senegal, and Uganda all had "bottom-up" reporting of aid data thanks to their Aid Information Management System (AIMS), Nigeria uniquely had much of their aid data geocoded by an outside AidData team. Uganda and Senegal did much more of their geocoding in-country.
6. In Uganda, Afrobarometer round 5 ran from December 2, 2011, to February 27, 2012, and I code it as occurring in the year 2011. My identification strategy compares people who recently received aid (or received it in the survey year) to those that are about to receive aid. If the start dates of aid projects are evenly spread over months and I record the survey as being in 2012, then I will miscategorize many people as having just received aid even though they are still months away from the project starting.
7. A useful source on the quality of statistical capacity in African governments is Jerven (2013). Of the group under analysis, Uganda likely has the most complete aid data. For more information on Uganda's aid management, see Kuteesa et al. (2010). For a ranking of these recipient's aid management policies and institutions, see Collaborative Africa Budget Reform Initiative (2008).
8. Note that these control variables are all likely to be unaffected by treatment status, which is important to avoid bias due to the inclusion of posttreatment controls Montgomery, Nyhan, and Torres (2018).
9. Note that changing the distance cutoff changes the location of the identifying variation in the data (Aronow and Samii 2016). This essentially means that some distance cutoffs are likely to weight, for example, (some) Ugandan observations more than are other distance cutoffs. For this reason, the main takeaway from Figure 1 should simply be that across many distance cutoffs the effect of receiving aid is never positive. It is unwise to make across-cutoff comparisons.
10. The bottom-most estimate (100 km cutoff) for model 4 is not estimated as there is no variation in the key independent variables when observations are geomatched with a cutoff of 100 km.
11. To do this, I make use of code from Hsiang (2010) and Fetzer (2014). I also fix a small error in the code from Hsiang (2010).
12. The group that has Afrobarometer surveys just after or during elections is Nigeria round 4 (2008), Nigeria round 5 (2012), Senegal round 4 (2008), and Uganda round 5 (2011). Thus, this group overrepresents Nigeria and underrepresents Uganda. This means that any difference that looks like an election-driven difference may simply be a difference between Nigeria and Uganda, which can be seen in Table 2.
13. These enumeration areas overwhelmingly had a precision code of 3.
14. For Nigeria, I also ran the analysis dropping Lagos instead of the Federal Capital Territory (Abuja). The results remain very similar.
15. The main issue in selecting cutoffs is that with large cutoffs there can be no people who never receive aid (the base category) in Senegal and especially Uganda.
16. Uganda shows no support for the claim that receiving aid boosts incumbent support. In particular, even in Uganda, I can reject the hypothesis that receiving aid increases incumbent support by small amounts. When using survey fixed effects, I can reject the null that present aid–future aid $= 0.03$ with $p = .048$. When using region-round fixed effects, I can reject the same null with $p = .053$. The results are obviously stronger for test values larger than 0.03 (3 percentage points).
17. I am unable to examine variation in the effect of aid on incumbent support across aid sectors due to uneven, inconsistent, and limited coding of aid sectors at the project level.
18. The online supplemental appendix reports results using the same setup as model 4 from Table 1, meaning that it includes all controls and uses geomatching.
19. Aid respondents appear to be less likely to protest in Table 3 but this difference is only significant at $p < .1$ and it is even weaker when controls are added and geomatching is used (see online appendix).
20. This finding contrasts with that of De Kadt and Lieberman (forthcoming), though they do not examine aid-funded service extensions.

21. I would like to thank a reviewer for making this point.
22. One countering finding is that of Ross (2006), who finds that democracies spend more on social services but that the poor benefit from this less than the middle class and the wealthy.

Supplemental Material

Supplemental materials for this article are available with the manuscript on the *Political Research Quarterly* (PRQ) website.

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