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Unbinding Ties: Foreign Aid and the Erosion of Social Trust in Africa

Philip Akrofi Atitianti
Africa-China Centre for Policy and Advisory
Michael A. Appiah-Kubi
mickey146g@gmail.com

MAFLO African Urban Research Center

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Abstract

Foreign aid dominates public investment in many African economies, yet its consequences for the social foundations of development remain poorly understood. This paper estimates the causal effect of foreign aid on social trust across 26 African countries. Matching geocoded aid projects from China and the World Bank with Afrobarometer survey data, we exploit supply-side instruments derived from Chinese industrial production inputs and World Bank financial capacity indicators, augmented by a shift-share instrument. We find that proximity to Chinese aid projects reduces social trust by 3.6 percentage points and proximity to World Bank projects by 8.8 percentage points. Both estimates are stable across alternative instruments, distance thresholds, and outcome measures. Mechanism tests show that aid reduces participation in four forms of collective action, and that collective action effects appear rapidly after project completion while trust effects accumulate gradually. The adverse trust effect is significantly attenuated in communities with strong pre-existing collective action, consistent with a social fragility hypothesis. These results imply that aid design which builds rather than replaces community cooperation can preserve the social infrastructure on which long-run development depends.

Keywords: Foreign aid; social trust; collective action; Africa; China; World Bank

JEL codes: F35, O12, O55, Z13

Introduction

Dambisa Moyo's charge that aid destroys the social fabric of recipient communities has provoked fierce debate but remarkably little empirical scrutiny. "In a world of aid," she writes, "there is no need or incentive to trust your neighbour, and no need for your neighbour to trust you. Thus aid erodes the essential fabric of trust that is needed between people in any functioning society" (Moyo, 2009, p. 59). The mechanism she invokes is intuitive: when external actors solve local problems, the social interactions that solving those problems together would have generated never occur. Yet the empirical record on this specific pathway is quite scarce.

This paper addresses that gap. We study whether foreign aid causally reduces interpersonal trust in Sub-Saharan Africa, and whether the effect operates through the collective action channel Moyo implicitly invokes. Our empirical strategy exploits sub-national variation in aid placement across 26 African countries, using three independent sources of supply-side variation in aid to identify causal effects: detrended Chinese production inputs interacted with historical exposure probabilities (Bluhm et al., 2025), World Bank financial capacity indicators interacted with regional exposure probabilities (Dreher et al., 2021),and a Bartik shift-share instrument that combines predetermined cluster-level exposure shares with leave-one-out Africa-wide aid intensity as an aggregate shift. The consistency of estimates across these independent identification strategies substantially strengthens our causal claims.

The theoretical logic connecting aid to trust runs through collective action. When communities lack external assistance, they must cooperate to address common problems such as broken wells, degraded roads, or inadequate schools. These episodes of cooperation are the primary context in which interpersonal bonds form and trust accumulates (Ostrom, 1990; Putnam, 1995). Aid substitutes for this cooperation through two channels. First, the direct crowding-out effect eliminates the marginal benefit of organizing: if a Chinese contractor is building the road, the returns to mobilizing a community work party fall to zero. Second is the expectation effect, which operates on beliefs: once communities observe that external actors address local challenges, rational updating implies future challenges may similarly be resolved externally, further eroding incentives to cooperate (Buchanan, 1975). Both channels reduce the frequency of cooperative interactions, severing the primary mechanism through which trust is built and maintained.

Our main findings are as follows. Individuals residing within 50 km of a Chinese aid project are 3.6 to 4.4 percentage points less likely to report trusting others, against a sample mean of 16.1 percent. The World Bank effect is substantially larger: 8.8 to 8.9 percentage points, against a sample mean of 19.4 percent. Both estimates are robust to alternative instrument lags, alternative distance thresholds (10, 25, 75, and 100 km), project count measures of aid intensity, and an alternative outcome measuring trust in neighbors specifically. For Chinese aid, a placebo falsification test in which we instrument aid from projects not yet present during the survey produces a near-zero first-stage F-statistic of 0.097, confirming that the production-input instrument does not predict pre-

treatment assignment. For World Bank aid, the analogous exercise reveals that areas slated to receive future projects exhibit significantly *higher* current trust, direct evidence of positive selection in World Bank placement that explains why the OLS estimate is attenuated toward zero relative to the IV estimate.

The mechanism evidence is consistent and specific. Aid reduces participation in four collective action activities: membership in voluntary groups (Chinese aid: 6.4 percentage points; World Bank: 13.7 percentage points), attendance at community meetings (6.2 and 5.0 percentage points, respectively), participation in demonstrations or protests (Chinese: 9.1 percentage points), and willingness to join others in raising an issue (6.8 and 6.2 percentage points). A dose-response analysis using project counts confirms that these effects scale continuously with the number of projects. A temporal precedence test exploiting variation in time since project completion finds that collective action effects are detectable among recently completed (0 to 2 years) projects, while trust effects are more diffuse, consistent with the sequential mechanism in which cooperation opportunities are eliminated first, then trust erodes subsequently.

Two heterogeneity findings sharpen the mechanism interpretation. First, the adverse trust effect of Chinese aid is significantly attenuated in communities with strong pre-existing collective action. In the interaction specification, the effect of aid in communities with below-median baseline collective action (measured from an earlier Afrobarometer round) is 14.4 percentage points, while the interaction with above-median baseline collective action offsets 11.7 percentage points of that effect, leaving a net effect near zero in high-capacity communities. This social fragility pattern suggests that communities whose cooperative capacity is already thin are the most vulnerable to further displacement. Second, the effect of Chinese aid is larger and more precisely estimated for women than for men, consistent with evidence that women bear a disproportionate share of community coordination responsibilities in African villages (Fearon et al., 2009).

This paper contributes to four literatures. First, it extends the study of aid's unintended consequences beyond macroeconomic outcomes (Burnside & Dollar, 2000; Easterly, 2003) to the social fabric of communities, joining the literature on aid and governance perceptions (Atitianti, 2023; Atitianti & Asiamah, 2023; Isaksson & Kotsadam, 2018). Second, it adds to the few sub-national causal estimates of the effect of foreign aid on interpersonal trust across a large set of African countries, and stimulates a debate on aid's effect on social trust, given the contrary conclusions drawn by Sun and Xu (2026) and D'Onofrio and Maggio (2015). It also places the negative association that Adera (2023) reports for Chinese aid on a causal footing and extends the analysis to a second donor. More broadly, our work also relates to studies on aid and social cohesion (Danquah & Ouattara, 2023; Fearon et al., 2009). Third, it contributes to the growing literature on the social determinants and consequences of collective action (D'Exelle et al., 2018; Ostrom, 1990; Turiansky, 2021) by showing that aid's effect on trust operates precisely through the collective action channel. Fourth, by comparing Chinese and World Bank aid, it demonstrates that trust erosion is not specific to

one donor's modality but is a general feature of externally provided development assistance, though the magnitude differs substantially across donors.

The remainder of the paper is organized as follows. Section 2 presents the theoretical framework and reviews the relevant literature. Section 3 describes the data. Section 4 presents the empirical strategy. Section 5 reports the main results. Section 6 investigates the collective action mechanism. Section 7 presents heterogeneity analysis and considers alternative explanations. Section 8 analyzes project lifecycle effects. Section 9 concludes.

Theory and Literature

Foreign Aid and Its Unintended Consequences

Foreign aid has been found to advance the strategic and geopolitical interests of donor nations while addressing humanitarian crises and fostering economic development in recipient countries (Dreher et al., 2018; Galiani et al., 2017; Milner & Tingley, 2013; Pham, 2025). Yet the record of aid's social consequences is increasingly troubling. Buchanan's (1975) Samaritan's Dilemma identified a fundamental tension: altruistic transfers can create dependency among recipients rather than spurring self-improvement. Subsequent evidence has documented adverse effects including increased corruption perceptions (Isaksson & Kotsadam, 2018), reduced trust in government and governance evaluations (Atitianti, 2023; Atitianti & Asiamah, 2023), higher crime and violence (Appiah-Kubi & Jarrett, 2023; Wood & Sullivan, 2015), and reduced economic optimism (Zhu et al., 2025).

Social trust has received comparatively little attention in aid literature. Sun and Xu (2026) is one of the few studies that assess aid's effect on social trust. The authors, focusing on countries across continents, argue that Chinese aid improves social trust by strengthening governance capacity in recipient countries. D'Onofrio and Maggio (2015) similarly find that in Uganda, Chinese aid improves social trust by lowering inequality. A few other related studies approach this theme through the broader concepts of social cohesion or social capital (Danquah & Ouattara, 2023; Fearon et al., 2009). Danquah and Ouattara (2023) find that aid reduces social capital in Africa. Fearon et al. (2009) conclude that community-driven development programs in Liberia produce limited gains in social cohesion outside narrow institutional settings. Closest to our own question is Adera (2023), who reports that Chinese aid projects in Africa are negatively associated with generalized trust and with participation in associational life. Those estimates are conditional correlations rather than causal effects, they cover a single donor, and the author finds no comparable pattern for World Bank aid.

Though related, our study differs from existing work on the aid-social trust nexus. D'Onofrio and Maggio (2015)'s single-country conclusion for Uganda is unlikely to generalize; hence, our focus on

26 African countries yields a more generalizable conclusion. Our focus on Africa also distinguishes this study from Sun and Xu (2026), who focus on a select number of countries across continents. Since aid's effect is likely influenced by recipient-country characteristics (Dreher et al., 2024), our findings from 26 African countries provide a more reliable indication of aid's consequences on the African continent. In addition, both Sun and Xu (2026) and D'Onofrio and Maggio (2015) argue that aid positively affects social trust, with improved governance capacity and lower inequality as channels of effect, respectively. We, however, argue the opposite, theorizing that aid substitutes for collective action, which is essential for building trust and therefore erodes social trust. Our contribution is therefore not to report a negative association but to identify one causally. We recover the effect from three independent sources of supply-side variation, document the collective action channel through which it operates, and show that it holds for two donors with very different delivery modalities, including the World Bank, for which Adera (2023) finds no comparable pattern. We confirm our theory using a broader set of sub-national analyses and instruments, and assess the effect for two distinct donors: China and the World Bank, using a broader scope of aid project information (between 2000 and 2021 for Chinese aid; 1995 and 2014 for World Bank aid).

Social trust matters for our purposes because it is a robust determinant of long-run economic growth (Bjørnskov, 2012, 2022; Knack & Keefer, 1997), through its contributions to institutional quality (Knack, 2002; Putnam, 1993), capital accumulation (Zak & Knack, 2001), and regulatory effectiveness (Aghion et al., 2010). A decline in interpersonal trust can therefore impede the very development that aid seeks to promote.

Collective Action as the Bridge: The Theoretical Framework

We propose a three-stage mechanism through which foreign aid erodes social trust. The mechanism is grounded in classical theories of cooperation and social capital accumulation.

Stage 1: The collective action imperative

In the absence of external assistance, communities facing common challenges must rely on collective action to address them. Ostrom (1990) identifies common pool resource problems as the canonical context in which cooperation becomes essential for survival. When the well breaks, the road washes out, or the school lacks supplies, communities must organize to respond. Voluntary group memberships, community meetings, and joint issue-raising are the institutional forms through which this cooperation occurs (Claibourn & Martin, 2000). These forms of cooperation are functional and repeated interactions through which individuals assess the reliability and reciprocity of their

neighbors, thereby forming the experiential basis of interpersonal trust (Putnam, 1995; Tadesse & Kassie, 2017).

Stage 2: Aid as substitute for collective action

External provision of aid effectively eliminates the marginal benefit of community cooperation. This substitution operates through two channels. First, the crowding-out effect is direct: when a Chinese contractor builds the road, organizing a community work party yields no additional return. Second, the expectation effect is forward-looking: once communities observe that external actors address local challenges, rational updating suggests future challenges will also be solved externally, preemptively eliminating incentives to build the cooperative habits that would otherwise produce trust (Buchanan, 1975). The World Bank's emphasis on conditionality and programmatic delivery may amplify these expectations, which is consistent with the larger trust effects we find for World Bank aid.

Stage 3: Reduced interaction and trust erosion

Putnam asserts that "the more we connect with other people, the more we trust them" (Putnam, 1995, p. 665), suggesting that social trust develops through repeated cooperative interactions. When aid substitutes for these interactions, it severs the primary mechanism through which trust is built and maintained. Reduced frequency of cooperative engagement limits opportunities for individuals to demonstrate reliability and reciprocity. The absence of shared problem-solving experiences weakens the social bonds that facilitate generalized trust. A rural farming community facing chronic water shortages, for instance, may organize collective labor to repair the communal well. This exercise generates bonds of mutual dependence. If a foreign donor funds the repair instead, that bond-forming opportunity is foreclosed.

This framework generates four testable predictions. First, aid proximity should reduce social trust (H1). Second, aid should reduce collective action participation (H2). Third, the trust effect should vary with communities' pre-existing cooperative capacity (H3). Fourth, collective action reductions should appear before trust reductions, because trust erodes only after the missing cooperative interactions have accumulated over time (H4).

The existing evidence on community-driven development provides supporting context for this mechanism. Studies find that programs which require communities to engage in collective action to receive benefits improve social cohesion precisely because they generate cooperative interactions (D'Exelle et al., 2018; Fearon et al., 2009; Tadesse & Kassie, 2017; Turiansky, 2021). This literature documents the trust-building potential of cooperation; our study tests whether aid that bypasses such cooperation destroys it.

Focusing on Chinese and World Bank aid in Africa allows a comparative test of the mechanism across two donors with contrasting philosophies. China adheres to a no-strings-attached policy, delivering aid without conditionalities and often through state-owned enterprise contractors with limited community engagement. The World Bank relies on conditionalities and programmatic delivery mechanisms, including community-driven development (Holmlund & Rao, 2021). Despite these differences, both modalities substitute external provision for community self-help, so the mechanism should apply to both. The substantially larger World Bank effect we observe may reflect the greater scale and transformative ambition of World Bank projects relative to Chinese bilateral transfers.

Data

We combine three primary datasets. AidData's Global Chinese Development Finance Dataset (Version 3.0) (Custer et al., 2023) provides geocoded records of Chinese aid commitments in Africa between 2000 and 2021. AidData's World Bank Geocoded Research Release (Version 1.4.2) (AidData, 2017) covers World Bank projects in Africa between 1995 and 2014. Both datasets use a precision-coding scheme ranging from 1 (exact point location) to 8 (national boundary). We retain only precision codes 1 and 2 to measure localized treatment exposure. For Chinese aid, we restrict to projects satisfying official development assistance (ODA) criteria and drop projects without a precise start date. The outcome and control variables come from three rounds of the Afrobarometer geocoded survey: rounds 3 (2005), 5 (2011–2013), and 8 (2019–2021) for Chinese aid; rounds 3 and 5 for World Bank aid, which is limited by the data coverage period. The Afrobarometer geocodes represent enumeration areas (survey clusters) for which we know precise geographic coordinates.

We construct our primary treatment indicator by merging project coordinates with survey cluster coordinates. A cluster is treated if at least one aid project is active within 50 km of the cluster centroid at the time of the survey. We also construct a project count variable measuring the number of active projects within 50 km, which serves as our intensive-margin treatment measure. We test robustness to alternative thresholds of 10, 25, 75, and 100 km.

Outcomes

Social trust is measured using the standard Afrobarometer item: "Generally speaking, would you say that most people can be trusted, or that you must be very careful in dealing with people?" Responses are coded as binary: 0 for "must be very careful" and 1 for "most people can be trusted." An alternative outcome, trust in neighbors, is coded as a binary indicator from the item asking respondents to rate how much they trust their neighbors.

Collective action measures

We construct indicators for four forms of collective action participation, harmonized across rounds.

The first variable is membership in a voluntary or community group, with responses coded as 0=Not a member, 1=Inactive member, 2=Active member, and 3=Official leader. We recode the responses into a dummy indicating 1 for responses 1 to 3 and 0 otherwise. For the remaining collective action variables, the preamble reads "For each of these, please tell me whether you, personally, have done any of these things during the past year. If not, would you do this if you had the chance?" The activities are attending community meetings, joining others to raise an issue, and attending a protest or demonstration. Responses to these activities are coded as 0=No, would never do this; 1=No, but would do if had the chance; 2=Yes, once or twice; 3=Yes, several times; 4=Yes, often. We generate dummy versions of these variables: 1 for responses 1 to 4 and 0 otherwise for use in the estimations.

Controls

Individual-level controls include age, age squared, gender, and urban or rural residence. All specifications include country and year fixed effects or country-by-year fixed effects. The final Chinese aid sample contains 90,204 individual observations across 26 countries. The World Bank sample contains 44,753 observations. Appendix Table A1 reports summary statistics.

Table 1 presents balance tests comparing respondents near (within 50 km) aid project sites to those without nearby projects. For Chinese aid, treated clusters have lower social trust (14.7 percent trusting, versus 17.9 percent in untreated clusters), lower group membership, and lower community meeting attendance, but are substantially more urban (56 percent urban versus 30 percent). For World Bank aid, treated clusters have higher meeting attendance, higher group membership, and higher issue-raising than untreated clusters. These contrasting imbalances confirm the non-random placement of aid. Because trust and participation are measured after projects arrived, the differences combine the choice of where to place aid with any effect of aid itself and cannot be read causally; they motivate our instrumental variable strategy.

Table 1: Balance Table: Treated versus Control Areas

	No aid	Aid	Difference	SE	*p*-value
Panel A: Chinese aid (50 km)					
Social trust	0.179	0.147	0.032	0.002	0.000
Trust neighbours (dummy)	0.875	0.849	0.026	0.003	0.000

Member of voluntary group	0.320	0.302	0.017	0.005	0.000
Attend community meeting	0.864	0.849	0.015	0.002	0.000
Attend demonstration	0.340	0.371	−0.031	0.005	0.000
Join others to raise issue	0.821	0.815	0.006	0.004	0.119
Age	37.047	37.187	−0.141	0.099	0.157
Male	0.502	0.501	0.001	0.003	0.758
Urban	0.304	0.563	−0.259	0.003	0.000
Observations			90,204		
Panel B: World Bank aid (50 km)					
Social trust	0.192	0.195	−0.003	0.004	0.498
Trust neighbours (dummy)	0.861	0.866	−0.005	0.003	0.135
Member of voluntary group	0.224	0.345	−0.122	0.005	0.000
Attend community meeting	0.791	0.894	−0.104	0.003	0.000
Attend demonstration	0.375	0.343	0.033	0.005	0.000
Join others to raise issue	0.747	0.852	−0.105	0.004	0.000
Age	37.397	36.979	0.418	0.149	0.005
Male	0.503	0.502	0.000	0.005	0.960
Urban	0.512	0.400	0.113	0.005	0.000
Observations			44,753		

Notes: Binary collective action measures shown; differences are (control −treated). Standard errors in the fourth column.

Empirical Strategy

Estimating Equations

We estimate a two-equation system. The first stage is:

$$Aid_{c,t} = \pi_1 Z_{c,t} + \gamma X_{i,t} + \delta_s + \phi_t + u_{c,t} \qquad (1)$$

The second stage is:

$$Trust_{i,c,t} = \alpha\, Aid_{c,t} + \gamma X_{i,t} + \delta_s + \phi_t + \varepsilon_{i,c,t} \qquad (2)$$

where $Aid_{c,t}$ is the aid treatment indicator for cluster c at time t, $Z_{c,t}$ is the instrument, $X_{i,t}$ is a vector of individual controls (age, age squared, gender, urban or rural residence), δ_s denotes country fixed effects, and ϕ_t denotes year fixed effects. Standard errors are clustered at the level of second-order administrative regions (ADM2). We also report a specification replacing country and year fixed effects with country-by-year fixed effects to absorb all country-level time-varying confounders.

Our identification assumption is that, conditional on country fixed effects, year fixed effects, and individual controls, the instruments affect social trust only through their effect on aid.

Supply-Side Instruments

Chinese aid instrument (Bluhm et al., 2025)

China's aid commitment volumes are strongly influenced by excess production of the primary inputs used in Chinese construction projects. Dreher et al. (2021) first identified the connection between Chinese steel over-production and subsequent aid commitments. Bluhm et al. (2025) extend this logic to a broader set of production inputs, including cement, aluminum, glass, timber, and iron. We extract the principal factor of these detrended log production inputs annually and interact it with each cluster's historical probability of receiving Chinese aid, defined as the fraction of years between 2000 and 2021 during which the cluster had at least one active Chinese project within 50 km. Formally, the instrument is $Z_{c,t}^{China} = \hat{F}_{t-2} \times p_c$ where $\hat{F}_{t-2}$ is the lagged principal factor of production input and p_c is the cluster's historical exposure probability. The first-stage Kleibergen-Paap F-statistics of 471 (country and year fixed effects) and 484 (country-by-year fixed effects) are far above conventional weak instrument thresholds.

World Bank aid instrument (Dreher et al., 2021)

The World Bank's lending capacity is captured by two indicators, following Dreher et al. (2021): the equity-to-loans ratio, which measures the Bank's capacity to extend new loans out of its own capital and is reported in its annual financial statements, and the funding position of the International Development Association (IDA), which measures the extent to which IDA can commit to new loans given its financial position (IDA, 2015). These financial indicators affect lending volumes but have no direct connection to social outcomes in recipient communities. Since 2008, IDA captures the funding position in its report. We follow Dreher et al. (2021) to calculate the funding position for the pre-2008 years. We interact each indicator with the cluster's historical probability of receiving World Bank aid. Kleibergen– Paap F-statistics of 395 and 413 confirm instrument strength.

Shift-Share Instrument as Independent Validation

As an independent check on the supply-side identification, we construct a Bartik-type shift-share instrument following Goldsmith-Pinkham et al. (2020). The share component is the cluster's predetermined historical aid exposure probability (pc), the same measure used in the production-input and financial capacity instruments. The shift component is the Africa- wide average aid intensity in survey year *t*, computed as the mean number of active projects per cluster across all countries, excluding the focal country (leave-one-out). Formally:

$$Z_{C,t}^{Bartik} = p_c \times LOO_{-j,t} \qquad (3)$$

Where $LOO_{-j,t}$ is the Africa-wide mean cluster-level project count in year t excluding country j. The validity of this instrument rests on two assumptions: that the exposure shares p_c are predetermined and uncorrelated with local trust trajectories, and that Africa-wide aid intensity is driven by donors' supply-side decisions rather than demand conditions in any single country. The leave-one-out construction removes own-country endogeneity while preserving the supply-side variation driven by China's or the World Bank's aggregate Africa-wide lending portfolio decisions. The shift-share F-statistics of 1,089 and 1,136 for Chinese aid and 389 and 383 for World Bank aid confirm strong first-stage relevance from an entirely independent source of identifying variation.

Exclusion Restriction Validation

We conduct three exercises to evaluate the exclusion restriction; the results appear in Appendix Table A2.

First, we implement a falsification test using future aid: we instrument for aid from projects that had not yet started at the time of the survey but would eventually operate in the area. For Chinese aid, if the production-input instrument captured anything other than actual aid exposure (for example, pre-existing cluster characteristics that attract aid), it should predict this future placement. The near-zero first-stage F-statistic of 0.097 confirms that the Chinese instrument does not predict pre-treatment assignment. For World Bank aid, the analogous exercise yields a different and informative result: areas slated to receive future World Bank projects exhibit significantly higher current trust (coefficient of 0.431; first-stage F-statistic of 42). Rather than a violation of the exclusion restriction, this pattern constitutes direct evidence of positive selection in World Bank project placement, consistent with the balance table showing that treated World Bank clusters have higher collective action participation. Positive selection biases the OLS estimate of the trust effect toward zero, which is exactly what we observe in Section 5.1, and implies that the IV estimate recovers a negative causal effect that the raw correlation conceals. The reduced-form relationship between the Chinese instrument and trust is itself negative and significant (Appendix Table A3), consistent with the second-stage results.

Second, the balance table (Table 1) shows that treated and control clusters are balanced on age and gender, the individual-level characteristics that are most plausibly correlated with trust through channels other than aid.

Third, the consistency of second-stage estimates across three independent identification strategies (Bluhm production inputs, Dreher financial capacity, and the Bartik shift-share) implies that any bias from a violation of the exclusion restriction would need to operate through all three distinct instruments simultaneously, which is implausible.

Main Results

Ordinary Least Squares

Table 2 reports OLS estimates. For Chinese aid, the coefficient of −0.017 (standard error 0.004) is negative and significant at the one percent level. Respondents living within 50 km of a Chinese project are almost two percentage points less likely to trust others. The estimate is nearly identical whether we use country and year fixed effects (column 1) or country-by-year fixed effects (column 2), suggesting the result is not sensitive to how we absorb time-varying country confounders. For World Bank aid, the OLS coefficient is −0.008 (standard error 0.008) in column 3 and is not statistically significant. The absence of a significant OLS effect for World Bank aid, combined with the large and significant IV estimate reported below and the positive future-aid correlation documented in Section 4.4, points to positive selection into World Bank project siting: the Bank locates projects in areas with higher baseline trust or social capital, which attenuates the negative causal effect toward zero in the raw data.

Table 2: Aid and Social Trust: OLS Results

	Chinese aid		World Bank aid	
	(1)	(2)	(3)	(4)
	Social trust	Social trust	Social trust	Social trust
Aid (50 km)	-0.017***	-0.016***	-0.008	-0.013
	(0.004)	(0.004)	(0.008)	(0.008)
Observations	90,204	90,204	44,753	44,753
R-squared	0.055	0.062	0.066	0.069
Baseline controls	Yes	Yes	Yes	Yes
Country FE	Yes	No	Yes	No
Year FE	Yes	No	Yes	No
Country-Year FE	No	Yes	No	Yes

Notes: The baseline controls include: urban/rural residence, age, age squared, and gender. Robust standard errors clustered at ADM2 level are in parentheses; *** p < .01, ** p < .05, * p < .1.

Instrumental Variable Results

Table 3 presents the baseline IV results. Panel A reports estimates for Chinese aid. The second-stage coefficients of −0.036 (column 1, country and year fixed effects) and −0.044 (column 3, country-by-year fixed effects) indicate that residing near a Chinese aid project reduces the probability of trusting others by 3.6 to 4.4 percentage points. Against a sample mean of 16.1 percent, this represents a 22 to 27 percent decline. The instrument performs strongly: the first-stage coefficient on the production inputs factor interacted with historical exposure probability is 0.489 (column 2), and the Kleibergen–Paap F-statistics of 471 and 484 rule out weak instrument concerns. The IV estimates are substantially larger in magnitude than the OLS estimates. This gap is unlikely to reflect the siting of Chinese projects in low-trust areas, since such negative selection would make the OLS estimate more negative, not less. A more plausible source is measurement error: an indicator for any project within 50 km is a coarse measure of exposure, and classical measurement error attenuates OLS estimates toward zero while leaving IV estimates consistent. The IV estimates are also local to communities whose exposure responds to fluctuations in Chinese supply conditions, which are communities with a history of receiving Chinese aid.

World Bank results appear in Panel B. The IV coefficient is −0.088 in column 1 and −0.089 in column 3, both significant at the one percent level. These estimates imply that World Bank project

proximity reduces trust by 8.8 to 8.9 percentage points, a 45 to 46 percent decline from the World Bank sample mean of 19.4 percent. The World Bank effect is approximately 2.4 times larger than the Chinese aid effect. This difference may reflect the greater scale and institutional reach of World Bank projects, their longer duration and conditionality structures, or the different composition of projects the Bank finances. The equity-to-loan ratio and funding position instruments both exhibit positive and significant first-stage coefficients, and the Kleibergen–Paap F-statistics of 395 and 413 are well above conventional thresholds.

As shown in Appendix Table A4, the negative effect of aid on social trust holds under probit estimation.

Table 3: Aid and Social Trust: Baseline IV Results

	(1)	(2)	(3)	(4)
	2nd stage	1st stage	2nd stage	1st stage
Panel A: Chinese aid				
Chinese aid (50 km)	-0.036**		-0.044***	
	(0.013)		(0.012)	
Input (t-2) × Prob. of aid		0.489***		0.495***
		(0.023)		(0.023)
Observations	90,204	90,204	90,204	90,204
Kleibergen-Paap F	471.144		484.012	
Panel B: World Bank aid				
World Bank aid (50 km)	-0.088***		-0.089***	
	(0.018)		(0.017)	
EL ratio (t-2) × Prob. of aid		0.022***		0.022***
		(0.0011)		(0.0011)
FP (t-2) × Prob. of aid		0.003***		0.003***

		(0.0003)		(0.0003)
Observations	44,753	44,753	44,753	44,753
Kleibergen-Paap F	395.375		413.159	
Baseline controls	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	No	No
Year FE	Yes	Yes	No	No
Country-Year FE	No	No	Yes	Yes

Notes: Baseline controls are urban/rural residence, age, age squared, and gender. EL ratio denotes the World Bank's equity-to-loans ratio; FP denotes the IDA funding position. Robust standard errors clustered at the ADM2 level in parentheses. ***p<0.01, **p<0.05, *p<0.1.

Robustness

Alternative instrument lags

Table 4 reports estimates with contemporaneous, one-year, and three-year instrument lags alongside the two-year baseline. For Chinese aid, the second-stage coefficient ranges from −0.034 to −0.038 across lag specifications, with the Kleibergen–Paap F-statistic declining from 813 (contemporaneous) to 331 (three-year lag). For World Bank aid, the coefficient is stable at −0.087 to −0.089 across all lags, with F-statistics remaining above 395 throughout. The stability of these estimates across lag structures suggests the baseline finding does not reflect a particular timing assumption.

Table 4: Robustness: Alternative Instrument Lags

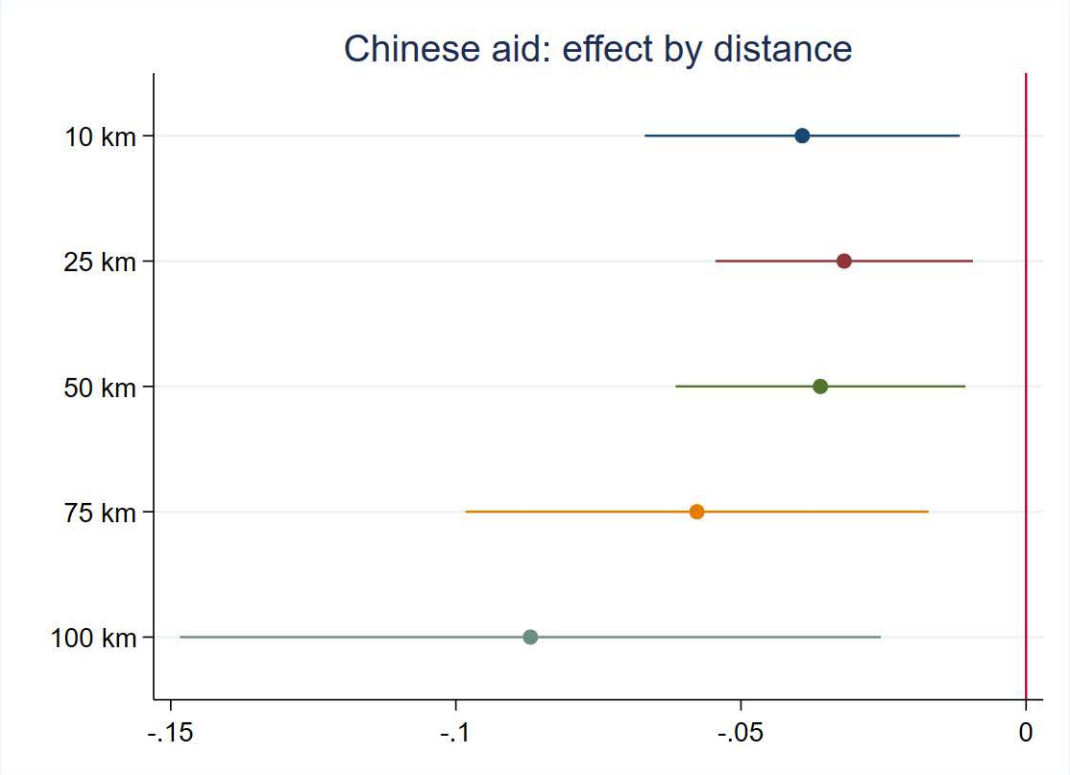
	(1)	(2)	(3)	(4)
	Contemp.	Lag 1	Lag 2	Lag 3
Chinese aid (50 km)	−0.036***	−0.034**	−0.036**	−0.038*
	(0.010)	(0.011)	(0.013)	(0.016)
Kleibergen–Paap F	812.58	639.11	471.14	331.05
Observations	90,204	90,204	90,204	90,204
World Bank aid (50 km)	−0.089***	−0.088***	−0.088***	−0.087***

	(0.017)	(0.017)	(0.018)	(0.018)
Kleibergen–Paap F	398.72	395.21	395.38	397.63
Observations	44,753	44,753	44,753	44,753

Notes: All specifications include baseline controls and country and year fixed effects. Column (3) is the baseline two-year lag. Robust standard errors clustered at the ADM2 level in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1.

Distance thresholds

We re-estimate the IV baseline at thresholds of 10, 25, 75, and 100 km (Appendix Table A5 and Figure 1). The trust-reducing effect of Chinese aid is consistently negative and significant across all five thresholds, with the point estimate ranging from −0.032 at 25 km to −0.087 at 100 km. The pattern for World Bank aid is similar in sign and statistically robust across distances, though the first-stage weakens at the widest thresholds. The absence of a sign reversal or threshold-dependence in these estimates is inconsistent with explanations based on place-specific unobservables that vary discontinuously with distance.



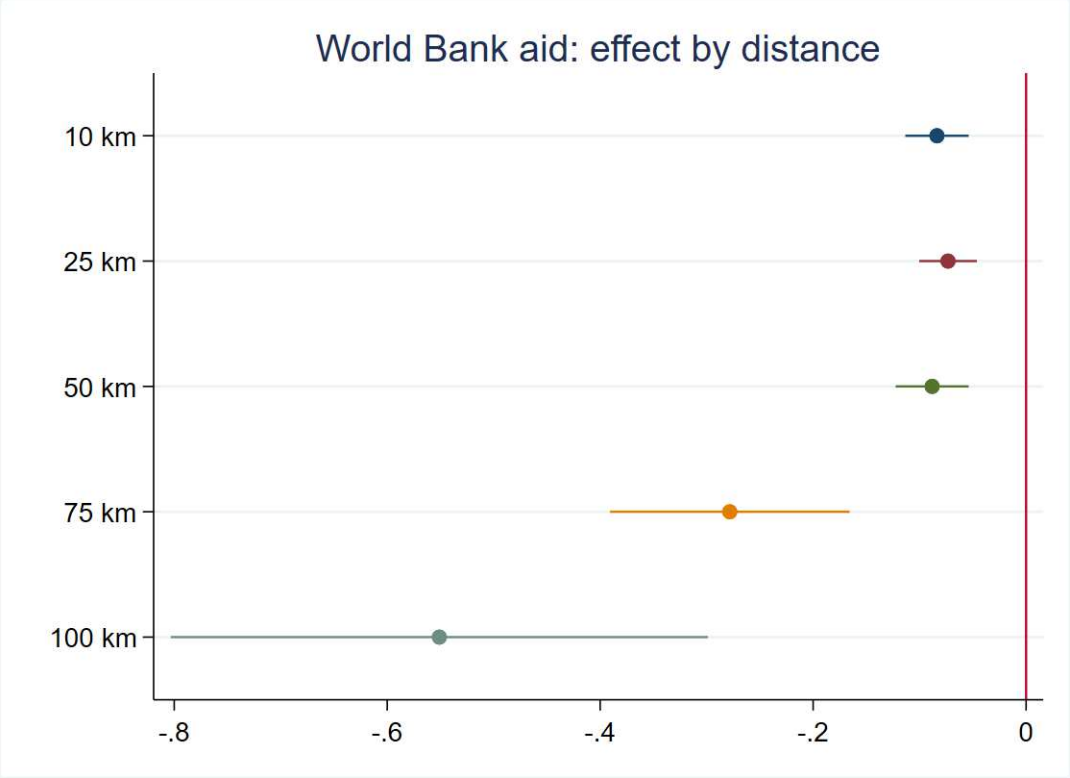


Figure 1: Second-stage coefficients and 95 percent confidence intervals across distance thresholds. Top panel: Chinese aid. Bottom panel: World Bank aid.

Project count

Table 5 replaces the binary treatment with a project count measure. Each additional Chinese project within 50 km reduces social trust by 0.05 percentage points ($p < 0.05$). For World Bank aid, each additional project reduces trust by 0.15 percentage points ($p < 0.01$). These dose-response patterns are consistent with a cumulative crowding-out mechanism: additional projects compound the substitution of external provision for collective action.

Table 5: Robustness: Project Count Measure

	Chinese aid		World Bank aid	
	(1)	(2)	(3)	(4)
	Social trust	Social trust	Social trust	Social trust
No. of aid projects (50 km)	-0.0005**	-0.0006***	-0.0015***	-0.0015***

	(0.0002)	(0.0002)	(0.0003)	(0.0003)
Observations	90,204	90,204	44,753	44,753
Kleibergen-Paap F	477.145	497.227	2404.625	2363.944
Baseline controls	Yes	Yes	Yes	Yes
Country FE	Yes	No	Yes	No
Year FE	Yes	No	Yes	No
Country-Year FE	No	Yes	No	Yes

Notes: IV estimates; the instrument is the respective supply-side shifter interacted with the historical probability of aid. Baseline controls included. Robust standard errors clustered at the ADM2 level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Alternative trust measure

Table 6 replaces general social trust with a binary indicator for trust in neighbors. Chinese aid reduces the probability of trusting one's neighbors by 7.4 percentage points (country and year fixed effects), and World Bank aid reduces it by 14.1 percentage points. The fact that neighborhood trust, which reflects the most proximate and observable social relationships, is reduced even more than generalized trust suggests that aid disrupts the local social fabric rather than merely shifting abstract trust norms.

Table 6: Robustness: Alternative Trust Measure (Trust in Neighbors)

	Chinese aid		World Bank aid	
	(1)	(2)	(3)	(4)
No. of aid projects (50 km)	-0.074***	-0.066***	-0.141***	-0.140***
	(0.020)	(0.020)	(0.014)	(0.014)
Observations	42,373	42,373	45,584	45,584
Kleibergen-Paap F	181.491	180.551	397.870	414.600
Baseline controls	Yes	Yes	Yes	Yes
Country FE	Yes	No	Yes	No
Year FE	Yes	No	Yes	No
Country-Year FE	No	Yes	No	Yes

Notes: The dependent variable is a binary indicator for trusting one's neighbors. IV estimates with baseline controls. Robust standard errors clustered at the ADM2 level in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1.

Shift-Share IV: Independent Identification

Table 7 compares the supply-side instruments and the Bartik shift-share instrument side by side. For Chinese aid, the Bartik instrument produces a second-stage coefficient of −0.037 (country and year fixed effects, column 2) and −0.033 (country-by-year fixed effects, column 3), which are nearly identical to the Bluhm estimates of −0.036 and −0.044. The Bartik Kleibergen–Paap F-statistics of 1,089 and 1,136 are stronger than those from the Bluhm instrument, reflecting the shift-share's larger aggregate variation. For World Bank aid, the Bartik instrument yields estimates of −0.074 (country and year fixed effects) and −0.075 (country-by-year fixed effects), consistent in sign and order of magnitude with the Dreher et al. (2021) estimates of −0.088 and −0.089, with F-statistics of 389 and 383. The close agreement between results from two structurally independent identification strategies, one based on donor-country industrial production or financial capacity and the other on cross-country portfolio allocation, considerably strengthens confidence in the causal interpretation.

Table 7: IV Comparison: Supply-Side versus Shift-Share Instruments

	(1)	(2)	(3)
	Supply-side (Country+Year FE)	Bartik-LOO (Country+Year FE)	Bartik-LOO (Country×Year FE)
Chinese aid (50 km)	-0.036**	-0.037***	-0.033***
	(0.013)	(0.009)	(0.009)
Observations	90,204	90,204	90,204
Kleibergen-Paap F	471.144	1089.157	1135.616
World Bank aid (50 km)	-0.088***	-0.074**	-0.075**
	(0.017)	(0.023)	(0.023)
Observations	44,753	44,753	44,753
Kleibergen-Paap F	395.375	389.060	383.166

Notes: Column (1) uses the Bluhm et al. production-input instrument (Panel A) or the Dreher et al. financial capacity instruments (Panel B); Panel B column (1) reports the country+year FE specification (the country×year FE analogue is −0.089***). Columns (2) – (3) use the shift-share instrument pc × $LOO - j, t$. Baseline controls included. Robust standard errors clustered at the ADM2 level in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1.

The Collective Action Mechanism

Mechanism Test: Collective Action Outcomes

If the theoretical mechanism is correct, aid should reduce collective action participation, and those reductions should account for at least part of the trust effect. Table 8 presents IV estimates of aid's effect on four collective action outcomes.

For Chinese aid, each outcome is reduced significantly or near-significantly. Proximity to a Chinese project reduces the probability of membership in a voluntary group by 6.4 percentage points ($p < 0.10$), attendance at a community meeting by 6.2 percentage points ($p < 0.01$), participation in a demonstration or protest by 9.1 percentage points ($p < 0.05$), and willingness to join others in raising an issue by 6.8 percentage points ($p < 0.05$). The breadth and consistency of these reductions across distinct forms of collective action is difficult to explain through mechanisms other than the cooperative substitution we propose. The meeting attendance result is the most precisely estimated (Kleibergen–Paap F of 471), as it draws on all three Afrobarometer rounds.

World Bank aid produces even larger collective action reductions. Voluntary group membership falls by 13.7 percentage points ($p < 0.01$), the largest single effect we estimate. Attendance at community meetings falls by 5.0 percentage points ($p < 0.01$). Participation in raising issues falls by 6.2 percentage points ($p < 0.01$). The one exception is demonstration or protest attendance, for which the World Bank coefficient is −0.007 and not significant. This pattern is consistent with the Bank's conditionality-based operating model, which may reduce formal group membership and meeting attendance more than it affects spontaneous civic activity such as protests.

Across both donors, the collective action results establish that aid does not merely reduce trust in the aggregate but specifically suppresses the forms of community engagement through which trust is constructed. Estimates using ordinal rather than binary collective action measures (Appendix Table A6) confirm the pattern.

Table 8: Mechanism Test: Collective Action Outcomes

	(1)	(2)	(3)	(4)
	Volunteer group	Attend meeting	Demonstration	Raise issue
Chinese aid (50 km)	-0.064*	-0.062***	-0.091**	-0.068**
	(0.027)	(0.014)	(0.030)	(0.022)
Observations	42,100	89,717	41,608	42,180
Kleibergen-Paap F	183.035	471.289	181.845	181.804
World Bank aid	-0.137***	-0.050***	-0.007	-0.062***
	(0.020)	(0.014)	(0.019)	(0.016)
Observations	44,170	44,450	43,684	44,344
Kleibergen-Paap F	392.422	394.493	391.385	393.123
Baseline controls	Yes	Yes	Yes	Yes

Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: IV estimates with baseline controls and country and year fixed effects. Dependent variables are binary. Robust standard errors clustered at the ADM2 level in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1.

Dose-Response Analysis

Table 9 presents dose-response estimates using project counts (number of projects). For Chinese aid, each additional project within 50 km reduces social trust by 0.05 percentage points ($p < 0.05$), group membership by 0.09 percentage points ($p < 0.10$), community meeting attendance by 0.09 percentage points ($p < 0.01$), and issue-raising by 0.10 percentage points ($p < 0.05$). The consistent dose-response relationship across both the outcome and the mechanism measures implies that the crowding-out effect is proportional to the intensity of aid presence. For World Bank aid, the dose-response coefficients are larger, with each additional project reducing trust by 0.15 percentage points ($p < 0.01$) and group membership by 0.22 percentage points ($p < 0.01$); as in the binary specification, demonstration attendance is unaffected

Table 9: Dose-Response Analysis: Project Count and Collective Action

	(1)	(2)	(3)	(4)	(5)
	Social trust	Volunteer group	Attend meeting	Demonstration	Raise issue
Panel A: Chinese aid					
No. of projects (50 km)	-0.0005**	-0.0009*	-0.0009***	-0.0013**	-0.0010**
	(0.0002)	(0.0004)	(0.0002)	(0.0004)	(0.0003)
Observations	90,204	42,100	89,717	41,608	42,180
Kleibergen-Paap F	477.145	156.644	480.738	157.323	157.145
Panel B: World Bank aid					
No. of projects (50 km)	-0.0015***	-0.0022***	-0.0008***	-0.0001	-0.0010***
	(0.0003)	(0.0003)	(0.0002)	(0.0003)	(0.0003)
Observations	44,753	44,170	44,450	43,684	44,344
Kleibergen-Paap F	2404.625	2406.660	2400.328	2412.919	2401.580

Notes: IV estimates with baseline controls and country and year fixed effects. Demonstration attendance was not estimated in the Chinese dose-response specification. Robust standard errors clustered at the ADM2 level in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1.

Mediation Analysis

To assess what fraction of aid's trust effect operates through collective action, we follow Baron and Kenny (Baron & Kenny, 1986) and compare OLS estimates with and without collective action controls. Table 10 presents these estimates for the Chinese aid sample. The aid coefficient in the baseline OLS specification without collective action controls is −0.017 (column 1). Adding controls for all four collective action measures reduces the coefficient to −0.014 (column 2), a reduction of approximately 18 percent. This reduction is consistent with collective action accounting for a meaningful share of the trust pathway.

We emphasize three caveats. First, the comparison is based on OLS because full IV mediation would require a separate instrument for each endogenous variable; our single production-input instrument for Chinese aid cannot separately identify the direct and indirect channels. Second, adding the collective action controls restricts the sample to observations for which all four measures are available (40,980 versus 90,204), so part of the coefficient movement may reflect the change in sample composition. Third, the 18 percent figure is a lower bound on the collective action channel, because the OLS controls are themselves endogenous and their coefficients may be attenuated. For World Bank aid, the baseline OLS is not statistically significant, precluding a meaningful mediation comparison for that donor.

Table 10: Mediation Analysis (Chinese Aid, OLS)

	(1)	(2)
	Without CA controls	With CA controls
Chinese aid (50 km)	-0.017***	-0.014*
	(0.004)	(0.006)
Member of voluntary group		0.007
		(0.005)

Attend community meeting		-0.018*
		(0.008)
Join others to raise issue		0.019**
		(0.007)
Attend demonstration		-0.011*
		(0.005)
Observations	90,204	40,980
R-squared	0.055	0.070

Notes: OLS estimates with baseline controls and country and year fixed effects. Robust standard errors clustered at the ADM2 level in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1.

Temporal Precedence

A key prediction of the three-stage mechanism is that collective action effects should appear before trust effects, because trust erodes only after cooperative opportunities have been consistently missing over time. Table 11 tests this prediction by comparing estimates for recently completed projects (zero to two years before the survey) against older completed projects (three or more years before the survey).

For Chinese aid, the results support temporal ordering. Among recently completed projects, community meeting attendance falls by 7.9 percentage points ($p < 0.01$) while the trust effect is small and statistically indistinguishable −0.023. Among older completed projects, meeting attendance remains significantly reduced (−0.183, $p < 0.05$) while the trust effect is again small (−0.003). This pattern is consistent with the mechanism: aid's immediate impact is to crowd out cooperation; trust accumulates gradually, and its erosion may require extended periods of depleted cooperative activity. The very large group membership coefficient for recently completed projects (−0.432) should be interpreted with considerable caution: the first-stage Kleibergen–Paap F-statistic in that column is 8.9, below conventional weak-instrument thresholds, and the subsample is small, so the local average treatment effect may differ from the full-sample estimate. We therefore rely on the precisely estimated meeting attendance results for the temporal argument.

For World Bank aid, the subgroup estimates are not statistically significant for either outcome or timing group, likely reflecting reduced statistical power in subsamples that draw on only two Afrobarometer rounds.

Table 11: Temporal Precedence: Trust versus Collective Action by Time Since Completion

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Trust (0-2yr)	Trust (3+yr)	Group (0-2yr)	Group (3+yr)	Meeting (0-2yr)	Meeting (3+yr)	Raise issue (0-2yr)	Raise issue (3+yr)
Chinese aid (50 km)	-0.023	-0.003	-0.432**	0.014	-0.079***	-0.183**	-0.003	0.087*
	(0.013)	(0.054)	(0.146)	(0.054)	(0.015)	(0.056)	(0.081)	(0.043)
Observations	17,101	16,927	3,146	1,526	16,970	16,874	3,176	1,529
Kleibergen-Paap F	502.461	70.238	8.867	118.432	501.249	70.325	8.969	115.603
World Bank aid (50 km)	-0.061	-0.093	-0.094	-0.107	-0.009	0.089	-0.029	-0.033
	(0.044)	(0.077)	(0.052)	(0.087)	(0.028)	(0.077)	(0.031)	(0.085)
Observations	8,745	1,944	8,493	1,913	8,651	1,916	8,617	1,914
Kleibergen-Paap F	61.734	17.399	60.942	17.514	60.984	18.319	61.084	18.023

Notes: IV estimates with baseline controls and country and year fixed effects. Columns split completed projects by time since completion. Robust standard errors clustered at the ADM2 level in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1.

Heterogeneity by Baseline Collective Action

We test H3 by splitting the sample on pre-existing collective action. Using Afrobarometer round 3 as a pre-period measure, we construct a cluster-level index averaging three collective action indicators and classify clusters as high or low collective action relative to the sample median. We then estimate the trust effect separately for each group and via an interaction term, excluding round 3 observations from all estimations to prevent mechanical correlation. Table 12 presents the results for Chinese aid. The split-sample point estimates in columns 1 and 2 are similar in magnitude (−0.101 in high-baseline communities, −0.102 in low-baseline communities), but only the low-baseline estimate is precisely estimated (significant at the five percent level); the high-

baseline estimate is statistically indistinguishable from zero. The interaction specification in column 3 provides the sharper test: the main effect of aid is −0.144 ($p < 0.05$), and the interaction of aid with high-baseline collective action is +0.117 ($p < 0.10$), implying a net effect of approximately −0.027 in high-capacity communities against −0.144 in low-capacity communities.

The evidence therefore rejects the simple version of H3, under which communities with more cooperation at stake would lose the most, in favor of a social fragility hypothesis: aid's trust-reducing effect is concentrated in communities where collective action is already weak. Communities with robust pre-existing social capital appear partially resilient to the crowding-out mechanism, perhaps because their cooperative norms and networks persist even when individual cooperative occasions are reduced. Communities with thin social capital, by contrast, may have fewer channels through which to recover from the displacement of even their limited cooperative activity. This finding has direct policy relevance: it identifies the communities most vulnerable to the adverse social consequences of aid. Given that the interaction term is significant only at the ten percent level, we interpret this pattern as suggestive rather than definitive.

For World Bank aid, the baseline collective action heterogeneity analysis produces first-stage F-statistics below 13, rendering the IV estimates unreliable for this donor. We do not interpret those results.

Table 12: Heterogeneity by Baseline Collective Action (Chinese Aid)

	(1)	(2)	(3)
	High baseline CA	Low baseline CA	Interaction
Chinese aid (50 km)	-0.101	-0.102**	-0.144**
	(0.058)	(0.039)	(0.048)
Aid × High baseline CA			0.117*
			(0.046)
High baseline CA (above median)			-0.073*
			(0.029)
Observations	6,665	7,183	13,848
Kleibergen-Paap F	47.038	59.253	76.470

Notes: Baseline collective action is measured from Afrobarometer round 3; round 3 observations are excluded from all specifications. IV estimates with baseline controls and country and year fixed effects. Robust standard errors clustered at the ADM2 level in parentheses. *** $p < 0.01$, ** $p < 0.05$,

* $p < 0.1$.

Alternative Explanations and Heterogeneity

Distributional Conflict and Relative Deprivation

Aid projects may generate local distributional conflicts that erode trust independently of the collective action channel. If project benefits accrue to some community members and not others, the resulting inequality may reduce trust among those who perceive themselves as excluded (Fehr & Gächter, 2000). To assess this explanation, we note two features of the data. First, the collective action mechanism operates symmetrically across forms of civic participation that are not directly tied to aid project benefits, including protest attendance and issue-raising. Distributional conflict predicts reduced trust among the non-beneficiaries but not necessarily reduced civic participation of this form. Second, the adverse trust effect is stronger for women than for men (Section 7.4), and women are less likely to be primary aid project beneficiaries in most contexts, which is the opposite pattern from what a distributional conflict explanation would predict. While we cannot fully rule out distributional conflict as a contributing factor, the pattern of evidence favors the collective action channel.

Elite Capture

Aid projects captured by local elites may create perceptions of unfairness and erode trust in neighbors and community institutions. We address this concern indirectly by noting that our instruments are driven by donor-side supply shocks (Chinese production inputs, World Bank financial capacity) rather than recipient-side political economy conditions that predict elite capture. The IV estimates therefore isolate the effect of aid itself, net of the political conditions under which it was allocated. Moreover, the temporal precedence evidence in Section 6 shows that collective action reductions precede trust erosion rather than occurring simultaneously, which is more consistent with the substitution mechanism than with an elite capture story in which trust erodes through political disillusionment.

Contact with Outsiders

The physical presence of foreign contractors and project personnel could reduce generalized trust by exposing communities to culturally unfamiliar actors, or could increase it through positive

contact effects (Allport, 1954). The negative effects we find are inconsistent with a positive contact story. Our alternative trust measure, which focuses specifically on trust in neighbors (rather than strangers), is more negative than the generalized trust effect, which argues against interpreting aid as merely shifting abstract trust norms by introducing foreign contacts. The adverse effects are concentrated within communities, among the familiar social networks where cooperative interaction builds trust.

Urban-Rural and Gender Heterogeneity

Table 13 presents heterogeneity by urban or rural residence and by gender for both donors. For Chinese aid, the urban and rural subsamples produce identical point estimates of −0.029, neither significant at conventional levels individually, though the effect is significant in the full sample. The absence of an urban-rural differential for Chinese aid is notable: aid crowds out collective action in rural agricultural communities and in urban neighborhoods alike, suggesting the mechanism operates through social interactions broadly rather than through agricultural collective action specifically. For World Bank aid, by contrast, the effect is concentrated in urban areas (−0.130, $p < 0.01$) and is small and insignificant in rural areas (−0.035). This urban concentration is consistent with the composition of World Bank lending, which skews toward large urban infrastructure and institutional projects, and with the larger disruption such projects impose on dense urban social networks.

The gender results are revealing for both donors. For Chinese aid, the trust effect is larger and more precisely estimated for women (−0.041, $p < 0.05$) than for men (−0.031, not significant). For World Bank aid, both effects are significant, with the female estimate again modestly larger (−0.092 versus −0.084). This asymmetry is consistent with evidence that women play a disproportionate role in community coordination activities in African societies (Fearon et al., 2009), and therefore have more to lose from the crowding out of cooperative occasions. When aid displaces community problem-solving, women's cooperative roles are disproportionately eliminated.

Table 13: Heterogeneity by Urban/Rural Residence and Gender

	(1)	(2)	(3)	(4)
	Urban	Rural	Male	Female
Chinese aid (50 km)	-0.029	-0.029	-0.031	-0.041**

	(0.015)	(0.026)	(0.016)	(0.016)
Observations	40,585	49,619	45,234	44,970
Kleibergen-Paap F	303.371	127.762	467.996	470.406
World Bank aid (50 km)	-0.130***	-0.035	-0.084***	-0.092***
	(0.024)	(0.023)	(0.020)	(0.021)
Observations	19,548	25,205	22,485	22,268
Kleibergen-Paap F	153.302	236.186	396.354	387.164

Notes: IV estimates with baseline controls and country and year fixed effects. Robust standard errors clustered at the ADM2 level in parentheses. *** p < 0.01,

** p < 0.05, * p < 0.1.

Heterogeneity by Project Type

Table 14 splits projects by whether they involve physical construction. For Chinese aid, the point estimate for infrastructure projects is large (−0.205, p < 0.10) but the first stage is weak in this subsample (Kleibergen–Paap F of 22.6), so we place more weight on the non-infrastructure estimate of −0.028 (p < 0.10, F of 339). For World Bank aid, both infrastructure (−0.106, p < 0.01) and social-sector projects (−0.100, p < 0.10) reduce trust by similar magnitudes. The fact that the trust effect is not confined to construction-intensive projects argues against interpretations based purely on physical disruption during building activity and supports the more general substitution mechanism: any external provision that displaces community problem-solving, whether a road or a social program, forecloses cooperative occasions.

Table 14: Heterogeneity by Project Type

	(1) Infrastructure	(2) Non-infrastructure
Chinese aid (50 km)	-0.205*	-0.028*
	(0.085)	(0.014)
Observations	33,410	56,794
Kleibergen-Paap F	22.614	339.089
World Bank aid (50 km)	-0.106***	-0.100*

	(0.027)	(0.043)
Observations	22,271	3,904
Kleibergen-Paap F	153.397	76.771

Notes: Infrastructure denotes projects involving physical construction activities. IV estimates with baseline controls and country and year fixed effects. Robust standard errors clustered at the ADM2 level in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1.

Heterogeneity by Survey Wave

In Appendix Table A7, we report heterogeneous effects by survey wave. For Chinese aid, the trust-eroding effect is significant for wave 3 but not for the other waves. For World Bank aid, the significant effect holds in both survey waves.

Project Lifecycle Effects

Binary Lifecycle Analysis

The timing of aid's trust effect in the project lifecycle provides additional evidence on the mechanism. Table 15 reports baseline IV estimates separately for projects under implementation and completed projects. For Chinese aid, the point estimate during implementation is −0.126 (standard error 0.086), consistent with a negative effect but not significant, partly reflecting the smaller sample and weaker first stage (Kleibergen–Paap F of 31) in this group. Completed projects yield a precisely estimated effect of −0.044 (p < 0.01), identical to the full-sample baseline, indicating that the trust erosion persists well after construction activity has ceased.

World Bank projects show significant effects in both phases: −0.065 ($p < 0.05$) during implementation and −0.116 ($p < 0.01$) after completion. The larger post-completion effect is consistent with the cumulative logic of the mechanism: displacement of cooperative opportunities begins during implementation, and the resulting trust erosion continues to accumulate after the project is delivered. It is inconsistent with explanations based purely on the transitory disruption of construction, under which the effect should fade once building activity ends.

Table 15: Project Lifecycle: Implementation versus Completion

	(1)	(2)
	Under implementation	Completed
Chinese aid (50 km)	-0.126	-0.044***
	(0.086)	(0.013)

Observations	9,094	81,110
Kleibergen-Paap F	30.649	466.652
World Bank aid (50 km)	-0.065**	-0.116***
	(0.022)	(0.026)
Observations	21,033	23,720
Kleibergen-Paap F	228.942	186.804

Notes: IV estimates with baseline controls and country and year fixed effects. Robust standard errors clustered at the ADM2 level in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1.

Granular Time-Since-Completion Analysis

Table 16 presents estimates across four project phases: implementation, zero to two years since completion, three to five years since completion, and six or more years since completion. For Chinese aid, no consistent gradient of effects by time since completion is apparent. The implementation estimate remains imprecise (Kleibergen–Paap F of 31), and none of the post-completion estimates is individually significant. This pattern may reflect that the trust- eroding effect operates gradually and is not captured by comparing narrow time windows; the pooled post-completion estimate of −0.044 (Table 15) is significant, so the imprecision here likely reflects that the identifying variation within each time window is thinner than in the pooled sample.

For World Bank aid, the implementation effect is the most precisely estimated within this granular breakdown (−0.065, p < 0.05), while the post-completion estimates are individually imprecise even though the pooled completed-project effect is large and significant. The limited number of post-completion observations available across only two Afrobarometer rounds constrains the granular lifecycle analysis for World Bank aid.

Table 16: Project Lifecycle: Time Since Completion

	(1)	(2)	(3)	(4)
	Implementation	0-2 yrs	3-5 yrs	6+ yrs
Chinese aid (50 km)	-0.126	-0.023	0.016	-0.003
	(0.086)	(0.013)	(0.028)	(0.054)
Observations	9,094	17,101	13,496	16,927

Kleibergen-Paap F	30.649	502.461	161.037	70.238
World Bank aid (50 km)	-0.065**	-0.061	0.045	-0.093
	(0.022)	(0.044)	(0.094)	(0.077)
Observations	21,033	8,745	3,190	1,944
Kleibergen-Paap F	228.942	61.734	37.223	17.399

Notes: IV estimates with baseline controls and country and year fixed effects. Robust standard errors clustered at the ADM2 level in parentheses. *** p < 0.01,** p < 0.05, * p < 0.1.

Conclusion

This paper provides causal evidence that foreign aid erodes interpersonal trust across a large number of African countries, and shows that the effect operates through the crowding out of collective action. Using three independent identification strategies and data spanning Chinese and World Bank aid projects across 26 countries, we find consistent evidence that aid proximity reduces social trust by 3.6 to 4.4 percentage points for Chinese aid and 8.8 to 8.9 percentage points for World Bank aid.

The mechanism evidence supports the theoretical framework in which aid substitutes for cooperative community activity, reducing the frequency of social interactions through which trust forms. Aid reduces participation in voluntary groups, community meetings, collective protests, and joint issue-raising. These effects scale with project intensity, are not contingent on the specific instrument used, and are significantly attenuated in communities with strong cooperative traditions. The adverse consequences of aid are therefore largest precisely where communities can least afford to lose their social infrastructure.

Three policy implications follow directly from these findings. First, aid modalities that require community participation rather than replacing community effort should reduce the adverse trust effects documented here. Community-driven development programs that task beneficiaries with co-producing project outcomes (Fearon et al., 2009; Turiansky, 2021) preserve the cooperative occasions through which trust forms. A testable implication is that CDD-structured projects should exhibit smaller or reversed effects on trust compared to contractor-delivered projects. Second, the social fragility finding identifies which communities most need participatory safeguards. Communities with weak pre-existing collective action are most vulnerable; a targeting rule that applies participatory co-production requirements to projects in such communities could reduce the adverse social consequences without necessarily increasing project costs. Third, the timing of trust erosion suggests that the social consequences of aid accumulate over time. Short-term

evaluations of aid projects will understate the full social cost of collective action displacement; monitoring frameworks should track trust and civic engagement over multi-year horizons.

This paper has three important limitations. The collective action mechanism is identified through reduced-form comparisons across IV subsamples rather than through a full structural mediation analysis, which would require additional instruments. The cross-sectional comparison across project completion timing is a partial substitute for true panel identification of the temporal sequence. While the results generalize across two major donors and 26 countries, the setting is limited to Africa and the period of major Chinese engagement from 2000 onward; whether the mechanism operates similarly in other contexts remains an open question.

Future research should exploit the variation in aid delivery modalities within donors to directly test whether participatory co-production dampens the trust effects we document. Panel data tracking individuals before and after project arrival, if available, would sharpen the temporal mechanism. And extending the analysis to aid recipients in South and Southeast Asia would test whether the social fragility pattern generalizes beyond the African context.

Data availability

The Afrobarometer survey data used in this paper are publicly available from the Afrobarometer data archive. The geocoded aid data are publicly available from AidData (Global Chinese Development Finance Dataset, Version 3.0; World Bank Geocoded Research Release, Version 1.4.2). The Chinese production-input series and the World Bank financial indicators are drawn from published sources cited in the text. The constructed instruments, the analysis data, and the Stata code that reproduces every table and figure will be deposited in a public repository in accordance with the journal's replication policy.

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Appendix

Table A1: Summary Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Panel A: Chinese aid sample					
Male	90,204	0.501	0.500	0	1
Urban	90,204	0.450	0.497	0	1
Age	90,204	37.13	14.79	18	130
Aid within 50 km	90,204	0.564	0.496	0	1
No. of projects within 50 km	90,204	10.57	18.92	0	109
Social trust	90,204	0.161	0.368	0	1
Trust neighbours (dummy)	42,373	0.861	0.346	0	1
Attend meeting (dummy)	89,717	0.856	0.352	0	1
Attend demonstration (dummy)	41,608	0.358	0.479	0	1
Raise issue (dummy)	42,180	0.818	0.386	0	1
Member of voluntary group (dummy)	42,100	0.310	0.462	0	1
Panel B: World Bank sample					
Male	44,753	0.502	0.500	0	1
Urban	44,753	0.437	0.496	0	1
Age	44,753	37.12	14.78	18	130
Aid within 50 km	44,753	0.616	0.486	0	1
No. of projects within 50 km	44,753	9.33	13.86	0	73
Social trust	44,753	0.194	0.395	0	1
Trust neighbours (dummy)	44,601	0.865	0.342	0	1
Attend meeting (dummy)	44,450	0.860	0.347	0	1
Attend demonstration (dummy)	43,684	0.353	0.478	0	1
Raise issue (dummy)	44,344	0.818	0.386	0	1
Member of voluntary group (dummy)	44,170	0.306	0.461	0	1

Table A2: Exclusion Restriction: Falsification Tests Using Future Aid

	(1) Chinese aid	(2) World Bank aid
Future aid (50 km)	3.676	0.431***
	(11.928)	(0.096)
Observations	90,204	44,753
Kleibergen–Paap F	0.097	42.04

Notes: Future aid refers to areas that eventually received aid projects in subsequent years but where implementation had not yet started at the time of the survey. For Chinese aid, the near-zero first-stage F-statistic indicates the instrument does not predict pre-treatment assignment (the second-stage coefficient is unidentified). For World Bank aid, the positive coefficient indicates positive selection into future placement, consistent with the balance table and the attenuated OLS estimate. Robust standard errors clustered at the ADM2 level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A3: Reduced Form: Instrument and Social Trust (Chinese Aid)

	(1) Social trust
Input ($t-2$) × Prob. of aid	−0.018**
	(0.006)
Observations	90,204
R-squared	0.055

Notes: OLS regression of social trust on the instrument, with baseline controls and country and year fixed effects. Robust standard errors clustered at the ADM2 level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A4: Probit Estimation (IV Probit Marginal Effects)

	(1)	(2)	(3)	(4)
	Social trust	Social trust	Trust neighbours	Trust neighbours
Panel A: Chinese aid				
Chinese aid (50 km) (dummy)	-0.168**		-0.307***	
	(0.064)		(0.079)	
No. of projects (50 km)		-0.002*		-0.004***
		(0.001)		(0.001)
Observations	90,204	90,204	42,373	42,373
Wald test (p-value)	0.000	0.000	0.000	0.000
Panel B: World Bank aid				
World Bank aid (50 km) (dummy)	-0.349***		-0.665***	
	(0.068)		(0.062)	
No. of projects (50 km)		-0.006***		-0.011***
		(0.001)		(0.001)
Observations	44,753	44,753	44,601	44,601
Wald test (p-value)	0.000	0.000	0.000	0.000

Notes: IV probit estimates with baseline controls and country and year fixed effects. Wald tests of exogeneity reject at $p < 0.001$ in all columns. Robust standard errors clustered at the ADM2 level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A5: Robustness: Alternative Distance Thresholds

	(1)	(2)	(3)	(4)	(5)
	10 km	25 km	50 km	75 km	100 km
Chinese aid	-0.039**	-0.032**	-0.036**	-0.058**	-0.087**
	(0.014)	(0.012)	(0.013)	(0.021)	(0.031)
Observations	90,204	90,204	90,204	90,204	90,204
Kleibergen-Paap F	248.613	430.446	471.144	330.060	225.603
World Bank aid	-0.084***	-0.073***	-0.088***	-0.278***	-0.551***
	(0.015)	(0.014)	(0.018)	(0.057)	(0.129)
Observations	44,753	44,753	44,753	44,753	44,753
Kleibergen-Paap F	385.029	557.115	395.375	68.503	26.323

Notes: IV estimates with baseline controls and country and year fixed effects. The larger World Bank estimates at 75 and 100 km coincide with weaker first stages and should be interpreted with caution. Robust standard errors clustered at the ADM2 level in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1.

Table A6: Mechanism Test: Ordinal Collective Action Measures

	(1)	(2)	(3)	(4)
	Volunteer group	Attend meeting	Demonstration	Raise issue
Chinese aid (50 km)	-0.105*	-0.588***	-0.145**	-0.334***
	(0.051)	(0.052)	(0.054)	(0.070)
Observations	42,100	89,717	41,608	42,180
Kleibergen-Paap F	183.035	471.289	181.845	181.804
R-squared	0.103	0.150	0.074	0.171
World Bank aid (50 km)	-0.230***	-0.412***	0.012	-0.322***
	(0.038)	(0.061)	(0.035)	(0.057)
Observations	44,170	44,450	43,684	44,344
Kleibergen-Paap F	392.422	394.493	391.385	393.123

Notes: Dependent variables are ordinal (0–3 or 0–4). IV estimates with baseline controls and country and year fixed effects. Robust standard errors clustered at the ADM2 level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A7: Heterogeneity by Survey Wave

	(1)	(2)	(3)
	Wave 3 (2005)	Wave 5 (2011)	Wave 8 (2019)
Chinese aid (50 km)	-0.095**	-0.032	-0.023
	(0.033)	(0.027)	(0.016)
Observations	13,848	28,673	47,683
Kleibergen-Paap F	110.529	113.187	312.014
World Bank aid (50 km)	-0.094***	-0.082***	
	(0.024)	(0.025)	
Observations	13,848	30,905	
Kleibergen-Paap F	243.913	215.867	

Notes: IV estimates with baseline controls and country fixed effects, estimated separately by survey wave. Robust standard errors clustered at the ADM2 level in parentheses. *** p < 0.01,

** p < 0.05, * p < 0.1.