Engage or Avoid?
Foreign Aid around Religious Spaces after International Crises

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Abstract

How does hostility change where donors place aid near socially important institutions? I develop a spatial model that unifies engagement and avoidance: hostility initially raises the value of visible outreach, but greater resistance and risk eventually make proximity less attractive. The model identifies the value of foreign public support and implementer exposure as the conditions that move donors between these responses. I test its donor-specific predictions with geocoded Chinese and U.S. development projects near mosques in Muslim-majority countries. After the July 2009 Urumqi riot, Chinese projects in recipients near Xinjiang show relative declines of 10.6–13.3 percentage points across the 1, 5, and 10 km mosque buffers and become farther from the nearest mosque. After September 11, U.S. projects in counterterrorism targets show a negative differential only within 1 km and no stable relocation across the broader spatial outcomes. The difference follows the crisis-specific mechanisms: Urumqi joined a domestic identity conflict, transnational Muslim networks, and risks to Chinese personnel, whereas U.S. policy combined outreach and security objectives across a multiagency aid portfolio. By treating project geography as a strategic choice, the paper explains why the same local institution can attract aid under one configuration of hostility and exposure but repel it under another.

Keywords: foreign aid, public diplomacy, security, China, United States, mosques, geocoded aid

1 Introduction

Foreign aid is meant to solve material problems, but donors rarely treat it as politically mute. They brand clinics, stage road openings, publicize deliveries, and ask whether recipients know who paid. Making the source clear can change how recipients evaluate both the donor and their own government (Dietrich et al., 2018, Goldsmith et al., 2014, Blair and Roessler, 2021). In a United Kingdom experiment, overseas credit claiming also affected source attribution, with greater support for aid concentrated among conservative respondents (Dietrich et al., 2019). These practices may build goodwill. They can also make a project a visible extension of an unpopular donor.

That tension becomes sharper after an international crisis. A terrorist attack, communal riot, military campaign, or diplomatic rupture can change what a donor represents to a foreign public. The donor then faces a spatial choice as well as a budgetary one. It may place visible projects near institutions where an important community gathers. It may instead keep projects farther away when proximity raises the risk of protest, sabotage, delay, or harm to local partners. Research on attacks against aid projects and the risks of military-linked relief shows why donors may anticipate such costs (Crost et al., 2014, Williamson, 2011). The question is not only whether aid wins hearts and minds. It is whether donors choose to be visible where skeptical communities gather.

This paper asks when hostility leads to engagement and when it leads to avoidance. I argue that the relationship is nonlinear. At very low hostility, a donor has little reason to spend heavily on persuasion. At moderate hostility, foreign public support becomes valuable while engagement remains feasible. Visible aid near a socially important institution can then carry information, create opportunities for interaction, and demonstrate commitment. At severe hostility, the diplomatic return falls while security and implementation costs rise. The donor reduces visible effort near the institution or abandons that strategy.

The argument joins research on soft power with work on conflict and backlash. Aid can improve opinions of donors and shape local political attitudes (Goldsmith et al., 2014, Dietrich et al., 2018, Blair and Roessler, 2021, Blair et al., 2022). Counterinsurgency programs can reduce violence in some settings, while projects can also become targets for insurgents (Berman et al., 2011, Crost et al., 2014). Aid may disappoint beneficiaries or strengthen local identities (Lyall et al., 2020, Isaksson, 2020). Chinese foreign direct investment can provoke anti-China protest, although the same study finds no corresponding effect for Chinese aid (Gong et al., 2024). Citizens may also discount an aid program when they distrust the donor's motives (Singh and Williamson, 2022). These patterns can coexist because donors choose where visibility is worth its costs.

I study that choice around mosques. Mosques are places of worship and neighborhood institutions. Religious networks can support welfare provision and political participation (Cammett and Issar, 2010, Livny, 2020). Repeated mosque participation can sustain political ties (Grewal, 2025). New mosque construction is also associated with political attitudes in nearby communities, with suggestive evidence that information plays a role (Sumaktoyo, 2025). Proximity does not reveal a project's religious content or measure actual contact. It shows whether a donor placed a project near a socially dense and publicly recognizable space.

The empirical analysis examines two crises. After September 11, 2001, U.S. strategy combined outreach to Muslim publics with wars and a wider counterterrorism campaign. I compare U.S. official development projects in counterterrorism targets with projects in lower-exposure recipients. The July 5, 2009 Urumqi riot linked China's domestic conflict in Xinjiang to transnational Uyghur networks and the safety of Chinese personnel abroad. I compare Chinese projects in eight recipients near Xinjiang with projects in the remaining Muslim-majority recipients. Because September 11 raised both outreach incentives and delivery risks, I do not expect the pooled U.S. portfolio to relocate in a common direction. Because Urumqi more directly joined Muslim identity, geography, and implementer risk, I expect Chinese projects near Xinjiang to become less mosque-proximate.

I estimate both cases with the same country-constrained distances, proximity outcomes, fixed effects, and uncertainty framework. I compare alternative exposure groups, samples, timing rules, and measures of geographic precision. I also compare mosque proximity with a broader shift in urban accessibility. Full regression tables and the complete specification curve appear in the appendix.

The Chinese results are broad. The estimated post-minus-pre changes for near-Xinjiang projects are 10.6, 13.3, and 11.2 percentage points lower within 1, 5, and 10 km than the changes for projects farther from Xinjiang. Logged distance also rises. The signs persist across many specifications and country exclusions. The event study shows, however, that the difference was already developing before 2009. Lower-exposure projects also became more mosque-proximate, and near-Xinjiang projects became relatively less urban-accessible. The evidence supports a negative exposure gradient after 2009 more clearly than a sudden change caused by the riot.

The U.S. results are narrower. Counterterrorism targets show a 43.7 percentage point relative decline within 1 km, but the 5 and 10 km estimates are small and imprecise. The 1 km contrast weakens when the sample is restricted by geographic precision and changes sign under a narrower direct-theater coding. The analysis treats aid funded across U.S. civilian and defense agencies as one donor portfolio. Official records document U.S. work with mosques, community centers, and religious partners, while the available project locations show no stable mosque-specific shift after September 11.

The paper contributes in three ways. First, it brings persuasion and withdrawal into one conditional model. Work on soft power shows why visible benefits may matter, while the model explains when exposure costs can offset that value (Nye, 2004, Goldsmith et al., 2014, Blair et al., 2022). Second, it treats project location as a strategic outcome. Research on subnational allocation shows that political incentives and delivery institutions shape where aid goes (Jablonski, 2014, Dietrich, 2021, Dreher et al., 2022). Third, it estimates the same relative exposure gradient for both donors and checks whether a mosque pattern is part of a wider change in portfolio geography. The argument applies most directly to visible projects whose locations or delivery arrangements can be changed.

The remainder of the paper develops the spatial logic of visible aid, derives the model and its empirical implications, explains the two comparisons, and describes the project and mosque data. I then present the estimates and discuss their implications.

2 The Spatial Politics of Visible Aid

2.1 Allocation, visibility, and political value

Research on foreign aid has long shown that donors balance recipient need against security, commercial, and diplomatic interests (Alesina and Dollar, 2000, Bueno de Mesquita and Smith, 2009). After the Cold War, deeper economic and social connections gave donors new reasons to direct development resources toward countries whose instability could spill across borders (Bermeo, 2017). Aid can reward allies, secure cooperation, and support a donor's broader foreign-policy coalition.

National totals are only the first stage of allocation. Recipient governments and donors also decide which districts, sectors, organizations, and communities receive projects. Recipient leaders steer resources toward electorally useful places (Jablonski, 2014, Briggs, 2014). Donors change delivery channels when they distrust the state, sometimes bypassing public institutions through firms or nongovernmental organizations (Dietrich, 2013, 2021). Geocoded studies of Chinese finance similarly show that subnational allocation responds to political relationships, recipient demand, and organizational incentives (Dreher et al., 2019, 2022). These findings make a simple point that matters here. A project location is an outcome of political bargaining, not just a coordinate attached after a budget is set.

The location of aid may move independently of its amount. A donor can maintain a country program while shifting new projects toward capitals, transport corridors, secure compounds, or favored constituencies. It can also move visible social projects toward communities it wants to reach without increasing the national envelope. A theory focused only on country-year aid totals misses this margin of adjustment.

Aid can produce soft power only when people connect a benefit to its source. Donors therefore invest in branding, ceremonies, and media coverage. Experimental work shows that revealing a foreign source changes how recipients evaluate both donors and local governments (Dietrich et al., 2018). Donor-side credit claiming can also affect domestic attribution and, for some respondents, support for aid (Dietrich et al., 2019). At the cross-national level, aid is associated with warmer views of donors in some settings (Goldsmith et al., 2014).

The effects differ across donors and programs. U.S. projects in Africa have been linked to greater support for the United States, while nearby Chinese projects do not always improve views of China and may reduce them (Blair et al., 2022). Aid exposure can leave recipient-state legitimacy intact or even strengthen it (Blair and Roessler, 2021). Chinese infrastructure aid has also been linked to more positive attitudes toward domestic government institutions through welfare gains and affinity with donor values (Bai et al., 2022). Aid therefore does not automatically buy affection: attribution, prior beliefs, delivery practices, and donor identity condition the response.

Most of this literature treats project exposure as an explanatory variable. The donor places the project first, after which researchers ask how nearby citizens respond. This paper moves one step backward. A donor that anticipates credit, resentment, or danger may choose a different location before the project reaches the public.

2.2 Backlash, exposure, and socially salient places

Counterinsurgency research supplies the clearest account of aid's double edge. Material assistance may generate information, improve support for the government, or raise the opportunity cost of joining an armed group (Berman et al., 2011). Community programs can also strengthen cooperation after war (Fearon et al., 2009). Yet aid may become a target when insurgents expect successful projects to weaken their influence (Crost et al., 2014). In Afghanistan, cash assistance produced a short-lived gain followed by disappointment, while pairing cash with vocational training produced a more durable response (Lyall et al., 2020). Humanitarian organizations have also warned that tying relief to military persuasion can compromise neutrality and endanger staff (Williamson, 2011).

The same ambiguity appears outside active war. Chinese aid projects can heighten local ethnic identification (Isaksson, 2020). Chinese foreign direct investment can also fuel anti-China protest, even though the same pattern does not appear for aid (Gong et al., 2024). Survey experiments in Egypt show that people evaluate otherwise identical aid programs differently when they learn which country funded them. Skepticism about a donor's motives lowers support for the program (Singh and Williamson, 2022). Chinese agencies sometimes direct public diplomacy resources toward places with anti-China protest (Yang et al., 2023). Hostility creates a reason to engage, but it can also reduce what engagement can accomplish.

This tension is usually framed as a disagreement about aid effectiveness. It is also an allocation problem. If donors expect that a visible project may attract violence, implicate a local partner, or deepen resentment, they can alter siting, branding, and delivery. Avoidance can reflect an expectation that visible diplomacy will be costly or ineffective even when diplomacy remains important.

Mosques matter to this siting problem because houses of worship are unusually dense social institutions. They bring people together repeatedly, support trusted networks, distribute services, and provide channels for information. In Lebanon, sectarian welfare institutions help organizations allocate benefits and build durable constituencies (Cammett and Issar, 2010). Across Muslim societies, religious networks can substitute for generalized trust and lower the cost of political participation (Livny, 2020). Repeated participation in mosque life can sustain electoral loyalty even when a party performs poorly (Grewal, 2025). Mosque construction is also associated with political attitudes in surrounding Indonesian communities, with suggestive evidence that information and communication carry part of the association (Sumaktoyo, 2025). In Western democracies, mosque participation can support mainstream civic and political activity rather than political withdrawal (Peucker, 2018).

A project near a recognizable institution may be easier to notice and attribute to its donor, which is necessary for credit claiming (Dietrich et al., 2018, Goldsmith et al., 2014). Religious leaders and service networks may also serve as partners and information conduits, while a nearby project creates opportunities for residents, implementers, and local partners to encounter one another. Contact is most likely to reduce prejudice when participants meet on equal terms, cooperate toward common goals, and receive institutional support; meta-analytic evidence shows that intergroup

contact generally reduces prejudice (Allport, 1954, Pettigrew, 1998, Pettigrew and Tropp, 2006). Geographic exposure to outgroups can also shape support for their protection (Pauselli, 2025).

Proximity does not establish that contact occurred, that a mosque administered a project, or that these favorable conditions were met. It measures a spatial opportunity. The same concentration of people, information, and symbolism that makes a mosque useful for engagement can make nearby delivery more conspicuous during identity-linked anger. Whether the opportunity or the risk dominates depends on the donor, the crisis, and local implementation.

Existing studies estimate how citizens respond to projects; they rarely examine whether donors anticipate those responses when choosing locations. The model formalizes that choice without assuming that mosques are uniformly political or that proximity is always beneficial.

3 A Model of Engagement and Avoidance

3.1 The donor's problem

Consider a donor deciding how much strategic effort to devote to making projects visible near a socially salient institution. Let $a \geq 0$ denote that effort. It can include branding, local partnership, or a preference for project density near the institution. It is not identical to total aid or to the placement of every observed project. Let $r > 0$ denote hostility toward the donor. Let $\omega > 0$ measure the political weight the donor places on foreign public support, and let $V > 0$ be the value of obtaining that support. Write $\Omega = \omega V$.

Visible effort can persuade, but its return is subject to resistance. The probability that effort produces the desired political return is $a/(a+r)$. The donor also pays a unit implementation cost. Hostility raises that cost when proximity exposes personnel, contractors, or partners to danger. The exposure parameter $\kappa \geq 0$ captures how quickly that additional cost rises. The donor's payoff is

$$U(a, r, \Omega, \kappa) = \Omega \frac{a}{a+r} - (1 + \kappa r)a. \tag{3.1}$$

The first term is the expected diplomatic return. The second is the combined resource and exposure cost. The model asks how strongly the donor favors visible projects nearby when persuasion is difficult and delivery is risky; it does not specify aid's effect on material welfare.

The optimal interior choice is

$$a^*(r, \Omega, \kappa) = \left[\sqrt{\frac{\Omega r}{1 + \kappa r}} - r \right]_+, \tag{3.2}$$

where $[x]_+ = \max\{x, 0\}$. The donor chooses positive visible effort only if

$$\Omega > r(1 + \kappa r). \tag{3.3}$$

Equation 3.3 is an exit condition. When hostility and exposure costs are too high relative to the value of foreign public support, strategic visibility falls to zero. Need, engineering constraints,

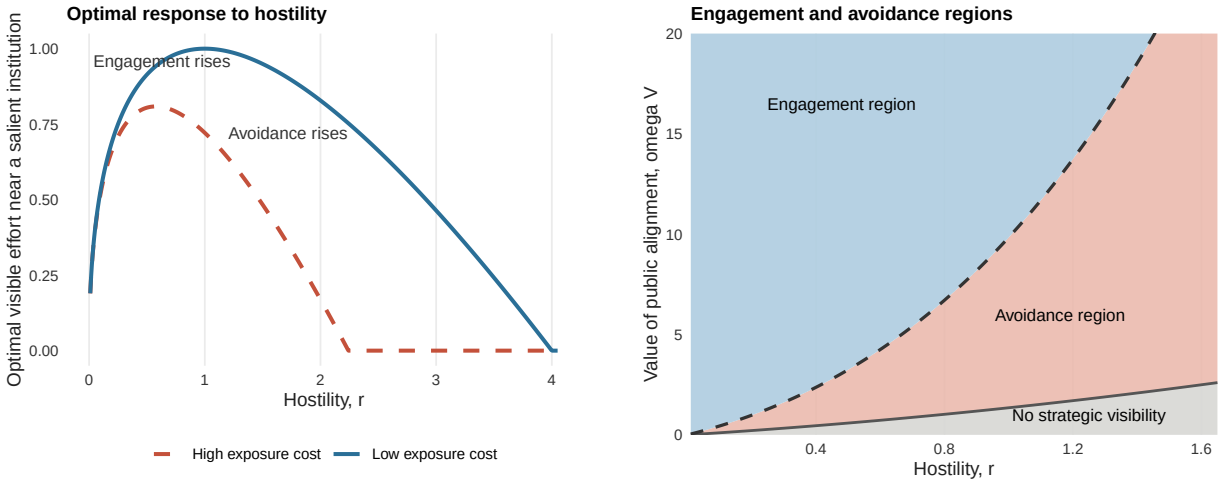
or recipient bargaining may still put projects near the institution, but the donor no longer places additional weight on proximity for diplomatic reasons.

Within the positive-effort region, visible effort rises with hostility when

$$\Omega > 4r(1 + \kappa r)^3. \tag{3.4}$$

Below that boundary, but above the exit boundary, additional hostility reduces visible effort. The response is therefore hump shaped. Low resistance requires little persuasion. Moderate resistance makes outreach valuable. Severe resistance makes it costly and ineffective.

Figure 1 illustrates these regions. Panel A holds the value of foreign public support fixed and traces optimal effort over hostility. A larger exposure cost lowers the curve, moves its peak left, and causes an earlier exit. Panel B places hostility on the horizontal axis and the value of support on the vertical axis. The lower solid boundary separates positive strategic visibility from exit. The upper dashed boundary separates avoidance from engagement. Appendix A provides the derivations and proofs.



(a) Optimal effort over hostility

(b) Regions in parameter space

Figure 1: Engagement and avoidance in the model. Panel A sets $\Omega = 4$ and compares $\kappa = 0$ with $\kappa = 0.35$. Panel B sets $\kappa = 0.35$. The numerical values illustrate the comparative statics rather than calibrating either donor.

3.2 From effort to project location

The data record the placement of observed projects, not branding, total effort, project entry, or the latent strategic component in Equation 3.2. The empirical analysis is therefore conditional on a project appearing in the source data. To connect effort to location, let m_i be the baseline site suitability of project i, including need, land, engineering, transport access, and recipient preferences. Let ϵ_i be a project-specific siting shock with a logistic distribution. An observed project is located near the salient institution when

$$N_i = \mathbf{1}\{m_i + \lambda a^*(r, \Omega, \kappa) - \epsilon_i \geq 0\}, \tag{3.5}$$

with $\lambda > 0$. It follows that

$$\Pr(N_i = 1 \mid r, \Omega, \kappa, m_i) = \Lambda\left(m_i + \lambda a^*(r, \Omega, \kappa)\right). \tag{3.6}$$

Because the logistic function is increasing, the probability of proximity moves in the same direction as optimal visible effort. Ordinary projects can remain near a mosque when strategic effort is zero, and some projects can remain far away when strategic effort is high. The model predicts changes in probability, not deterministic placement.

3.3 Observable expectations

Expectation 1: engagement after moderate hostility. If a crisis raises hostility while the donor remains above the engagement boundary in Equation 3.4, aid should become more likely to appear near the socially salient institution.

Expectation 2: avoidance after severe hostility. If the donor lies between the turning and exit boundaries, a further increase in hostility should reduce proximity. A sufficiently large increase can eliminate diplomacy-motivated visibility altogether.

Expectation 3: exposure shifts the response. Higher κ lowers visible effort at every interior point. It also moves the engagement peak and exit threshold toward lower hostility. Donors and implementers facing greater personnel, asset, or partner risk should begin withdrawing earlier.

Expectation 4: the value of foreign public support expands engagement. Higher Ω raises effort and extends the range of hostility over which engagement remains attractive. Political accountability may affect ω, but regime type is not the only source of variation. Strategic importance, bureaucratic mission, public-diplomacy doctrine, and the credibility of local partners can all change Ω.

These expectations concern changes after a crisis, not fixed donor personalities. The same donor can engage in one country and withdraw in another. A comparison of two crises cannot identify a general U.S. or Chinese type.

Observable implication: a relative exposure gradient. Suppose a crisis raises effective hostility or exposure cost more in high-exposure recipients than in otherwise comparable low-exposure recipients, so that the high-exposure response is more negative. This can occur because the high-exposure group lies on the downward branch while the low-exposure group lies on the rising branch, remains locally flat, or simply declines less. For a buffer outcome N, the design should

recover

$$\Delta \Pr(N = 1 \mid \text{high exposure}) - \Delta \Pr(N = 1 \mid \text{low exposure}) < 0, \tag{3.7}$$

with the opposite sign for logged distance. Because the estimand is relative, it does not distinguish outward movement by the high-exposure group from inward movement by the comparison group, nor does it identify the rising side of the hump. A coefficient near zero can reflect similar group changes, a locally flat region, or offsetting agency responses.

3.4 From the model to donor-specific predictions

Official records and prior research indicate how each crisis changed r, Ω, and κ. They motivate the donor-specific predictions below but do not identify causal effects.

3.4.1 The United States after September 11

The September 11 attacks raised countervailing forces in the model. They created an acute security threat, but the threat was an attack on the United States by al-Qaeda rather than a conflict with a particular foreign Muslim community or with mosques as institutions. Nine days later, President George W. Bush publicly distinguished a radical terrorist network from Muslim and Arab friends and described the campaign as using diplomacy, finance, intelligence, law enforcement, and military force (Bush, 2001). This distinction did not prevent the wars in Afghanistan and Iraq from increasing opposition to U.S. policy. By 2003, opinion of the United States had deteriorated sharply across many Muslim-majority countries (U.S. Government Accountability Office, 2004, National Commission on Terrorist Attacks Upon the United States, 2004). In the model's terms, resistance r increased, especially in countries most exposed to the ensuing counterterrorism campaign.

The same developments raised Ω. Post-9/11 strategy treated development and communication with Muslim publics as security instruments, not as peripheral humanitarian activities. The 2002 National Security Strategy made development and open institutions part of national security, while the 9/11 Commission later argued that the long struggle required educational and economic opportunity as well as force (The White House, 2002, National Commission on Terrorist Attacks Upon the United States, 2004). The State Department expanded public-diplomacy programs and resources in strategically important Muslim-majority countries (U.S. Government Accountability Office, 2004). Consistent with this doctrine, U.S. bilateral aid shifted toward strategically salient recipients during the War on Terror and responded especially strongly to terrorism that threatened U.S. interests (Fleck and Kilby, 2010, Boutton and Carter, 2014).

Post-9/11 programs did not fit within a single aid agency. A joint State–USAID strategic plan aligned diplomacy and development assistance, and the Government Accountability Office documented Muslim-world outreach by State, USAID, Defense, and other agencies (U.S. Department of State and U.S. Agency for International Development, 2003, U.S. Government Accountability Office, 2005). Provincial Reconstruction Teams combined Defense, State, USAID, and Agriculture personnel and linked service delivery, local governance, reconstruction, and counterinsurgency (U.S.

Government Accountability Office, 2008). These records establish institutional overlap in important programs, not joint planning of every U.S.-funded project.

Religious space was not categorically excluded from this portfolio. In Afghanistan, U.S. assistance supported cultural preservation at two Islamic sites, religious-leader exchanges, and the rehabilitation of approximately fifty mosques by the military (U.S. Department of State, 2006). In Iraq, an Inspector General audit documented four U.S.-funded mosque and community-center rehabilitation contracts in Fallujah. Officials expected them to support local employment, civic pride, acceptance of relief organizations, and reduced incentives for violence (USAID Office of Inspector General, 2009). Later State–USAID and development strategies identify religious leaders, faith communities, and local organizations as possible partners (U.S. Agency for International Development, 2011, U.S. Department of State and U.S. Agency for International Development, 2016). Pakistani communities exposed to foreign earthquake relief also retained more trust in Westerners years later, illustrating the potential political value of such engagement (Andrabi and Das, 2017).

The overlap between development and military stabilization also increased exposure costs. It could compromise the neutrality, credibility, and safety of personnel and local partners. Research in Afghanistan and Pakistan documents these tensions, while studies of Afghanistan, Iraq, and the Philippines show that local conditions can determine whether aid reduces violence or becomes a target (Wilder, 2010, Fishstein and Wilder, 2012, Williamson, 2011, Sexton, 2016, Berman et al., 2011, Crost et al., 2014). One implementation channel could manage the tradeoff without moving a project: USAID's marking policy permitted waivers for compelling political, safety, or security concerns. A 2009 audit records Lebanese school principals fearing attack or retaliation if U.S. support were visibly marked (U.S. Agency for International Development, Office of Inspector General, 2009, 21–22). Engagement could also proceed through a local partner, an unmarked service, a religious-leader exchange, or public diplomacy rather than a new project coordinate near a mosque.

I therefore analyze the agencies as one U.S. donor portfolio. Pooling is a donor-level analytic choice motivated by their documented overlap; it does not assume that every project was jointly planned or that the agencies had identical missions. Funding-agency fixed effects absorb changes in agency composition, while restricting the sample to one agency would answer a different question.

U.S. prediction: offsetting engagement and exposure. September 11 increased resistance, the political value of outreach, and delivery risk at the same time. I therefore expect the pooled U.S. response to be smaller and less uniform across outcomes than China's; a common directional shift is not expected. A weak spatial response would indicate that projects funded across U.S. agencies did not relocate in a common direction, not that other forms of engagement ceased.

The baseline high-exposure group contains Afghanistan, Pakistan, Iraq, Syria, Yemen, Somalia, Libya, Mali, Niger, Tunisia, and Iran. The lower-exposure group contains the other Muslim-majority recipients in the data. I also use a direct-theater definition limited to Afghanistan, Pakistan, and Iraq. The broad definition captures heterogeneous exposure to the wider post-9/11 security campaign. The narrow definition focuses on the clearest theaters but has very little pre-2001 support.

3.4.2 China after the July 2009 Urumqi riot

Chinese policy before the riot assigned diplomatic value to engagement with Muslim societies. The 2008 China–Arab States Cooperation Forum action plan promoted cultural exchange and dialogue between civilizations (Ministry of Foreign Affairs of the People's Republic of China, 2008). Rules for Chinese turnkey aid projects, effective January 2009, required implementers to respect recipient customs and to account for local conditions, architectural styles, and ordinary practice in project design (Ministry of Commerce of the People's Republic of China, 2008). These documents show that engagement was an official objective, consistent with $\Omega > 0$; they give no basis for treating Chinese aid as inherently averse to mosques. Two 2011 white papers provide later corroboration rather than evidence of pre-crisis intent: one described aid as a means of consolidating friendly relations and responding to recipient needs, while the other told overseas enterprises to respect local religions and customs (State Council Information Office of the People's Republic of China, 2011a,b).

The July crisis changed the identity and security environment much more specifically. On June 25–26, 2009, an unsubstantiated online allegation that Uyghur workers at a factory in Shaoguan had assaulted women preceded a Han attack in which two Uyghur workers were killed. A July 5 Uyghur demonstration in Urumqi demanded an investigation of that episode before violence spread. The sequence of police dispersal and crowd violence is contested, but Uyghur rioters attacked Han civilians and businesses, security forces used coercion, and armed Han crowds carried out reprisals over the following days (Clarke, 2010, Millward, 2009). The Chinese government's September white paper reported 197 deaths and more than 1,700 injuries; those figures and its attribution of the riot to externally organized separatist forces are the government's account, not uncontested facts (State Council Information Office of the People's Republic of China, 2009). Scholarly accounts place the episode within a longer deterioration in interethnic security (Millward, 2009, Tobin, 2020).

Unlike 9/11, this shock linked the donor's own territory, a predominantly Muslim minority, and transnational Uyghur networks. Beijing framed the violence through separatism and terrorism, while Uyghur organizations and human-rights advocates internationalized it. Muslim governments were generally restrained, but public pressure varied and Turkey was a prominent exception (Bianchi, 2016). Borders with Xinjiang, Turkic affinities, diaspora connections, and cross-border networks motivate coding Xinjiang-adjacent recipients as more exposed (Clarke, 2010). China's Foreign Ministry reported stones thrown at its embassy in the Netherlands and a Molotov attack on its consulate in Munich immediately after the riot (Ministry of Foreign Affairs of the People's Republic of China, 2009a). At a July 14 press conference, journalists asked whether the riot could prompt attacks on Chinese enterprises and citizens abroad. The ministry appealed to Islamic countries for support and promised stronger counterterrorism cooperation to protect Chinese institutions and citizens (Ministry of Foreign Affairs of the People's Republic of China, 2009b). The official exchange made the connection between the domestic riot and overseas personnel risk explicit.

Aid contractors worked on site and therefore faced location-specific security risks. A 2005 State Council directive required firms undertaking foreign-aid project construction to accept the leadership of Chinese embassies and consulates. It also instructed responsible agencies and missions

to monitor local political conditions, ethnic and religious tensions, public security, and terrorism (General Office of the State Council of the People's Republic of China, 2005). The January 2009 turnkey-aid rules assigned survey, design, and construction to state-selected firms, made them responsible for on-site delivery, and provided for Ministry of Commerce guidance against terrorist threats (Ministry of Commerce of the People's Republic of China, 2008). These rules apply to an important channel within the broader official-finance portfolio, not to every project in the data. Research on China's aid-contracting system shows that contractors can shape project proposals and implementation (Zhang, 2020). Evidence from Chinese construction projects in Ethiopia also documents the co-location of managers and workers in spatially identifiable work-and-residential compounds (Fei, 2020).

Later security documents illustrate a related logic rather than explain pre-2009 siting. Seven-agency regulations issued in 2010 required monitoring of ethnic and religious tensions, risk assessments in high-risk countries, and site protection (Ministry of Commerce of the People's Republic of China and six other agencies, 2010). A 2012 Ministry of Commerce manual advised that overseas office sites be kept away from possible targets or sensitive buildings, explicitly including mosques and other religious facilities (Ministry of Commerce of the People's Republic of China, 2012). Neither document is an aid-allocation rule, and neither establishes that the riot caused project relocation. They show, more narrowly, that Chinese authorities treated religious-site proximity as one possible security exposure for overseas implementers.

China prediction: relative avoidance. The Urumqi case therefore yields a clearer directional prediction than the U.S. case. The crisis increased resistance to China and the perceived exposure of Chinese personnel in a way directly connected to Uyghur, Muslim, and near-Xinjiang networks. Diplomatic outreach could continue at the interstate level even if implementers became more cautious around sites associated with the identity conflict. I therefore expect negative coefficients at all three mosque buffers and a positive coefficient for logged distance. This is a portfolio-level prediction, not a claim that every project moved or that the riot created a clean break.

The baseline high-exposure group contains Afghanistan, Iran, Kazakhstan, Kyrgyzstan, Pakistan, Tajikistan, Turkmenistan, and Uzbekistan. A diaspora-based definition replaces Iran with Turkey. In both cases, the empirical coefficient measures a relative exposure gradient. It does not locate either group at a precise point on the model's curve.

The comparison rests on crisis-specific mechanisms, not fixed national character. September 11 placed outreach and delivery risk inside the same multiagency U.S. strategy, whereas Urumqi linked a domestic identity conflict to transnational networks and the safety of Chinese implementers. The framework applies most directly to visible projects whose location or delivery can be altered; fixed-site infrastructure, budget support, debt relief, and disaster response offer less room for spatial adjustment.

4 Data and Measurement

4.1 U.S. and Chinese development projects

The U.S. data come from version 1.0 of the Geocoded Official Development Assistance Dataset, or GODAD (Bomprezzi et al., 2025, Geocoded Official Development Assistance Dataset (GODAD) Project, 2025). GODAD combines records derived from the OECD Creditor Reporting System with recipient-country aid-management sources and documents their provenance in its codebook. The working file covers 17,492 project locations in Muslim-majority countries from 1989 through 2020 and pools projects funded across U.S. civilian and defense agencies. I treat these observations as one portfolio of U.S. official development projects. The funding agency remains an implementation attribute and enters a fixed-effect robustness check; it does not redefine the donor or sample. The file records the year of a commitment or disbursement, so the timing tests allow for projects planned before a transaction appeared in the data.

The China file contains 4,238 project locations from AidData's Global Chinese Development Finance data for 2000 through 2021 (Custer et al., 2023, Goodman et al., 2024, Dreher et al., 2022). Timing is measured by commitment year. The common design defines the first full calendar year after each crisis as the start of the post period. The appendix also reports estimates based on exact dates and separates reported dates from estimated dates.

Some activities, including budget support and broad policy assistance, can inherit a capital or administrative coordinate even when they have no neighborhood-level site. The baseline keeps the full files. A common siteable-sector sample retains social infrastructure, economic infrastructure, and production sectors. A separate check keeps ODA-like flows, and another focuses on social infrastructure and services.

4.2 Mosques and spatial outcomes

Mosque locations come from 38 country-specific OpenStreetMap extracts. Point features are used directly, and polygon features are represented by an interior point. Each aid coordinate is matched only to mosques in the same recipient country. I calculate nearest distance on the sphere and define the 1, 5, and 10 km indicators from this single distance. The continuous outcome is $\log(1 + d)$.

Appendix Table 2 reports the validation checks. The distance measure is complete for both donors, all matches stay within the recipient country, and every buffer indicator is generated from its corresponding nearest distance.

The 1, 5, and 10 km buffers represent immediate-neighborhood, urban or peri-urban, and broader local proximity, respectively. A result confined to 1 km is substantively different from a shift that persists across all three buffers.

OpenStreetMap coverage varies across countries and over time. I treat the layer as a fixed measure of known mosque locations and include recipient fixed effects to absorb stable country differences in average coverage. Appendix C gives the coding and validation procedure.

4.3 Geographic precision

The source files use different precision codes. GODAD includes a Google Maps geocoder score and a separate indicator of whether a location is reliable at ADM1 or ADM2. AidData records whether each Chinese coordinate represents an exact feature, an administrative feature, or a broader location.

I harmonize these fields through a common resolution check. It retains U.S. observations coded as ADM2-or-better by GODAD and Chinese observations coded ADM2. This is the most comparable restriction available in the two files and shows whether fine-buffer results persist among more precisely located projects.

The 1 km result is evaluated against this precision check because immediate-neighborhood placement requires the most detailed coordinates.

4.4 Controls and an urban-access benchmark

The common covariate set contains logged organized-violence deaths, lagged liberal democracy, logged population, logged GDP per capita, and logged bilateral trade with the donor. These controls are measured at the country-year level. Recipient fixed effects absorb stable differences across countries, year fixed effects absorb donor-wide changes, and broad sector and flow fixed effects absorb portfolio composition. A shared robustness model adds funding-agency fixed effects to address changes in agency composition while retaining the full U.S. portfolio.

Complete covariates reduce the pre-crisis U.S. high-exposure cell from 18 projects to four. I report a complete-case model without controls next to the controlled model. The comparison separates movement caused by the selected sample from movement after covariate adjustment.

Mosques are concentrated in settled and accessible places, so a general move away from cities can resemble mosque avoidance. I measure $\log(1 + \text{minutes})$ to the nearest city with at least 50,000 residents using the 2000 global accessibility surface (Nelson, 2008). I first use accessibility as an alternative outcome under the complete model set. I then model mosque distance flexibly over accessibility and evaluate the high-exposure contrast at common travel times. This comparison shows whether the mosque result persists among projects with similar access to cities.

5 Research Design

For donor D, project i, recipient c, sector family s, flow class f, and year t, the baseline model is

$$Y_{icsft} = \beta_D \left(\text{Post}_t \times \text{Focal}_c\right) + \alpha_c + \delta_t + \lambda_s + \phi_f + \varepsilon_{icsft}. \tag{5.1}$$

For both donors, $\text{Focal}_c = 1$ denotes higher expected exposure. It identifies a counterterrorism target for the United States and a near-Xinjiang recipient for China. A negative β_D on a buffer outcome, or a positive coefficient on logged distance, means that high-exposure projects became relatively less mosque-proximate. In figures, I multiply the logged-distance coefficient and confidence

interval by -1 and label it "Mosque proximity ($-$ log distance)." This is only a display transformation: positive values then consistently mean greater proximity, matching the buffer outcomes. Regression tables retain the original distance sign.

The baseline uses linear probability models. Standard errors are clustered by recipient, which allows observations within a recipient to be related over time. A shared alternative clusters by recipient and year. The tables report both.

Only six U.S. and eight Chinese high-exposure recipients contribute projects in both periods. I therefore report restricted wild-cluster bootstrap-t tests alongside the recipient-clustered results. The tests use 999 six-point Webb draws at the recipient level. Appendix B gives the full procedure and results.

The coefficient is a difference in changes, not movement by one group alone. A negative buffer estimate can arise because high-exposure projects move outward, low-exposure projects move inward, or both. Appendix Table 3 reports the unadjusted means for all four cells. These means distinguish the two sources of the relative change.

The shared robustness set changes one feature at a time for both donors and all four outcomes. It varies clustering, covariate adjustment, trends, time windows, geographic precision, siteable sectors, flow types, weights, agency fixed effects, exposure definitions, and country inclusion. Appendix B reports the complete specification curve and regression tables.

The event studies use the same design for both donors and all four outcomes. The main figure covers event years -9 through +6, the longest common pre-period in both project files; it omits the crisis year and uses year -1 as the reference. Models include the baseline fixed effects and recipient-clustered uncertainty. Joint tests evaluate whether every estimable lead through year -2 is zero. The discussion begins with logged nearest distance because it uses all spatial information without selecting a buffer; the 1, 5, and 10 km results appear beside it. A matched -6 through +6 window is reported in the appendix.

Additional diagnostics include matched local contrasts, pre-crisis placebos, leave-one-country-out estimates, local-linear breaks, China-only exact-date models, and annual portfolio counts. Because the U.S. file lacks a monetary field comparable to Chinese commitments, project counts provide the common scale. The final checks use urban accessibility as an alternative outcome and compare mosque distance at common accessibility levels. Appendix B gives the full definitions and estimates.

6 Results

6.1 Baseline changes in proximity

Figure 2 plots the four baseline high-exposure differentials. For the distance outcome only, the plotted coefficient and interval equal the raw logged-distance estimates multiplied by -1. Values to the left therefore mean relatively less mosque-proximate placement for every outcome. The regression estimates in Appendix Table 4 retain the original distance sign.

For the United States, the 1 km coefficient is -0.437 (SE 0.210, $p = 0.045$), a 43.7-percentage-

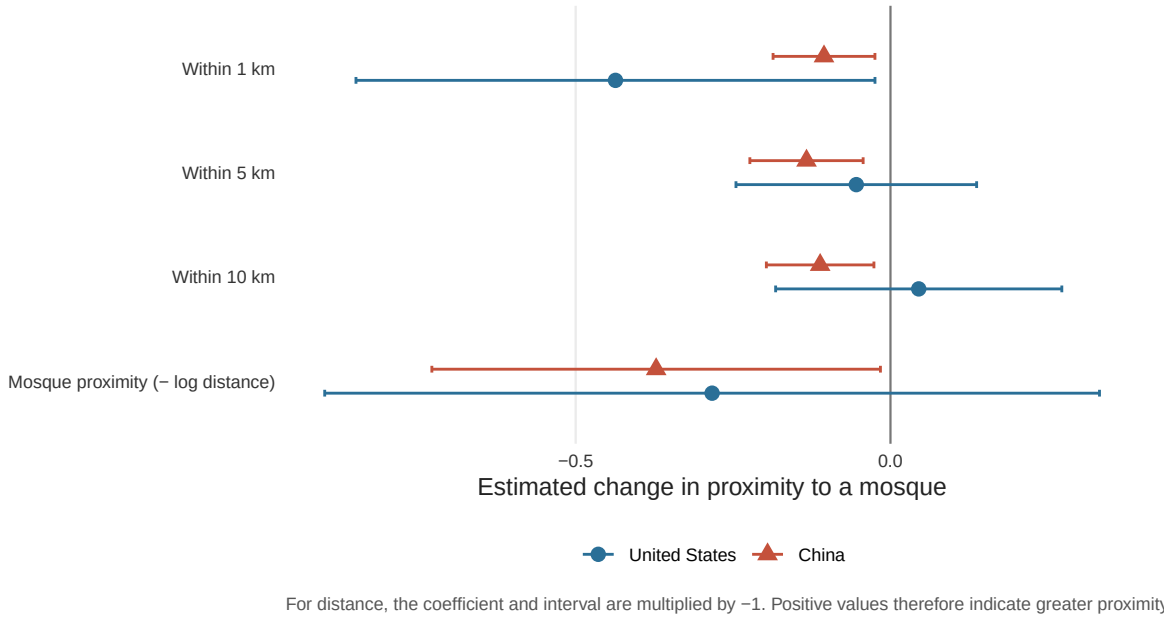


Figure 2: Baseline post-crisis differentials in aid proximity to mosques. The U.S. coefficient is counterterrorism targets minus other recipients after 2001. The China coefficient is near-Xinjiang minus other recipients after 2009. Both compare higher with lower exposure. Bars are 95% confidence intervals based on recipient-clustered standard errors. The plotted distance coefficient and interval are multiplied by −1, so positive values mean greater proximity for every outcome; tables retain the raw distance sign.

point relative decline in counterterrorism targets. The differential is −0.054 at 5 km (SE 0.098), 0.045 at 10 km (SE 0.116), and 0.283 for logged distance (SE 0.314). The wider measures do not show a common spatial response. The baseline comparison begins with 18 high-exposure projects before 2002.

The Chinese differential appears across the full set of outcomes. The buffer coefficients are −0.106 at 1 km, −0.133 at 5 km, and −0.112 at 10 km; the logged-distance coefficient is 0.372. Recipient-clustered p-values are 0.015, 0.006, 0.015, and 0.048. The agreement across radii and the continuous measure distinguishes this pattern from the U.S. result.

Restricted wild-cluster tests sharpen the contrast (Appendix Table 5). China's three buffer estimates remain statistically distinguishable from zero, with bootstrap p-values of 0.024, 0.024, and 0.040; the logged-distance p-value is 0.081. The U.S. 1 km bootstrap p-value is 0.138, and the other U.S. outcomes remain imprecise.

Raw means show how each relative estimate formed (Appendix Table 3). At 1 km, the U.S. high-exposure mean falls from 0.778 to 0.474 while the lower-exposure mean rises from 0.344 to 0.484. At 5 and 10 km, both U.S. groups decline. In China, near-Xinjiang means fall at every radius while the other group means rise. Both estimates therefore combine changes on the two sides of the comparison.

Appendix Figure 5 plots annual all-project and ODA-like record counts by exposure group. The series show sizable changes in portfolio composition. Comparable U.S. commitment amounts are

unavailable, so the common comparison uses project counts.

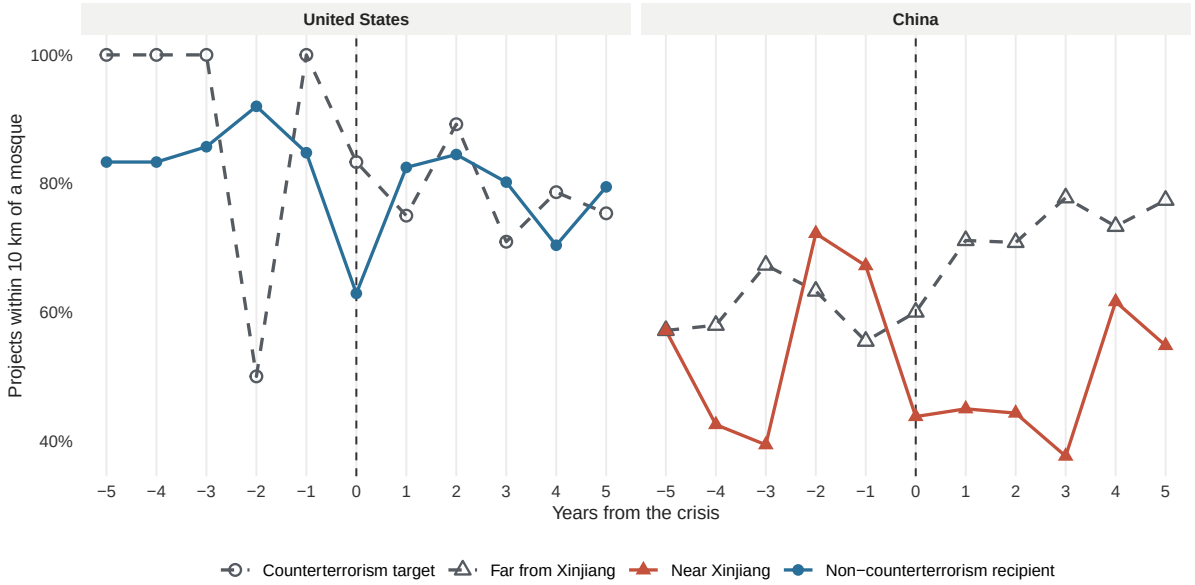


Figure 3: Group means around each crisis. Both panels show the annual share of projects within 10 km of a mosque over a symmetric five-year window. These descriptive means are not fixed-effect estimates and should be read alongside the cell counts in Appendix Table 1.

6.2 Checks across samples and measures

Appendix Figure 6 reports the complete specification curve and the corresponding estimates. For the United States, the negative 1 km differential recurs with recipient trends, shorter windows, siteable and ODA-like samples, equal country-year weights, social sectors, agency fixed effects, and the exclusion of Afghanistan and Pakistan. It contracts to -0.064 (SE 0.100) in the ADM2-or-better sample. Under the Afghanistan–Pakistan–Iraq definition it changes sign to 0.111 (SE 0.071). Wider-radius U.S. coefficients remain close to zero or imprecise across these models.

Dropping the mixed crisis year preserves the broad Chinese pattern and the isolated U.S. 1 km result. The U.S. estimate is -0.559 at 1 km but imprecise at wider buffers and for distance. The Chinese estimates remain -0.112, -0.147, and -0.113 across the three buffers, with a positive 0.409 distance differential.

The complete-case and controlled U.S. models separate sample selection from covariate adjustment. Complete covariates leave four pre-crisis high-exposure projects and make the 5 and 10 km estimates negative even before controls enter. Most of the change from the baseline therefore reflects complete-case selection rather than the controls themselves.

For China, the buffer coefficients remain negative with ADM2 locations, recipient trends, agency fixed effects, the diaspora-based group, and exclusion of Afghanistan and Pakistan. They are smaller in the ODA-like, equal-weight, and social-sector samples, especially at 10 km. The negative association is broader and more stable than the U.S. result.

Across these specifications, China retains a negative relative gradient, whereas the U.S. result remains confined to 1 km. Neither coefficient identifies the absolute change within either exposure group.

6.3 Dynamics and pre-crisis movement

Figure 4 shows the event studies for logged nearest distance and all three buffers. Each coefficient is the high-versus-low exposure difference in that event year relative to year -1. Negative values indicate relatively less mosque-proximate placement. The window begins nine years before each crisis, the longest common pre-period available in both project files.

The expanded window contains 11 pre-crisis U.S. high-exposure projects across four recipients. The joint lead test for continuous distance gives $p = 0.622$. The buffer tests give $p < 0.001$ at 1 km, $p = 0.004$ at 5 km, and $p < 0.001$ at 10 km. The longer pre-period therefore does not support a common parallel-trends claim for the U.S. buffer outcomes. The matched six-year window and its support counts are reported in Appendix Table 14.

The China series contains 464 pre-crisis high-exposure projects across all eight near-Xinjiang recipients. Its leads are jointly nonzero for continuous distance and every buffer. The p-values are below 0.001 for distance, 5 km, and 10 km, and 0.010 at 1 km. The graph shows a negative post-2009 exposure gradient, but the difference was already developing before the riot.

The placebo estimates likewise reveal pre-crisis divergence, especially for China. U.S. pre-crisis placebos are imprecise and use 128 observations. For China, the relative-year -3 placebo differs from zero at all three buffers and for logged distance. The post-period comparison should therefore be read as a change in the relative exposure gradient rather than a clean break caused only by either crisis.

6.4 Timing and country influence

Appendix Table 15 reports matched two-period contrasts. The U.S. one- and two-year comparisons have two pre-crisis high-exposure projects, and the three-year comparison has one. In the annual local-linear models, the U.S. 1 km break is the only estimate statistically distinguishable from zero. Wider buffers and logged distance are imprecise (Appendix Table 16). Chinese signs generally point away from mosques but change in size across windows and outcomes. The exact-date models separate reported from estimated dates and show the same dependence on bandwidth (Appendix Table 17).

Appendix Figure 9 examines whether one country drives the 1 km coefficients. The Chinese estimates stay negative when any one near-Xinjiang recipient is removed, ranging from -0.120 to -0.088. Every displayed interval remains below zero.

The U.S. point estimates remain negative when each counterterrorism target is removed, ranging from -0.549 to -0.147. Several intervals cross zero. Omitting Syria sharply reduces the estimate because Syria supplies seven of the 18 pre-crisis high-exposure observations.

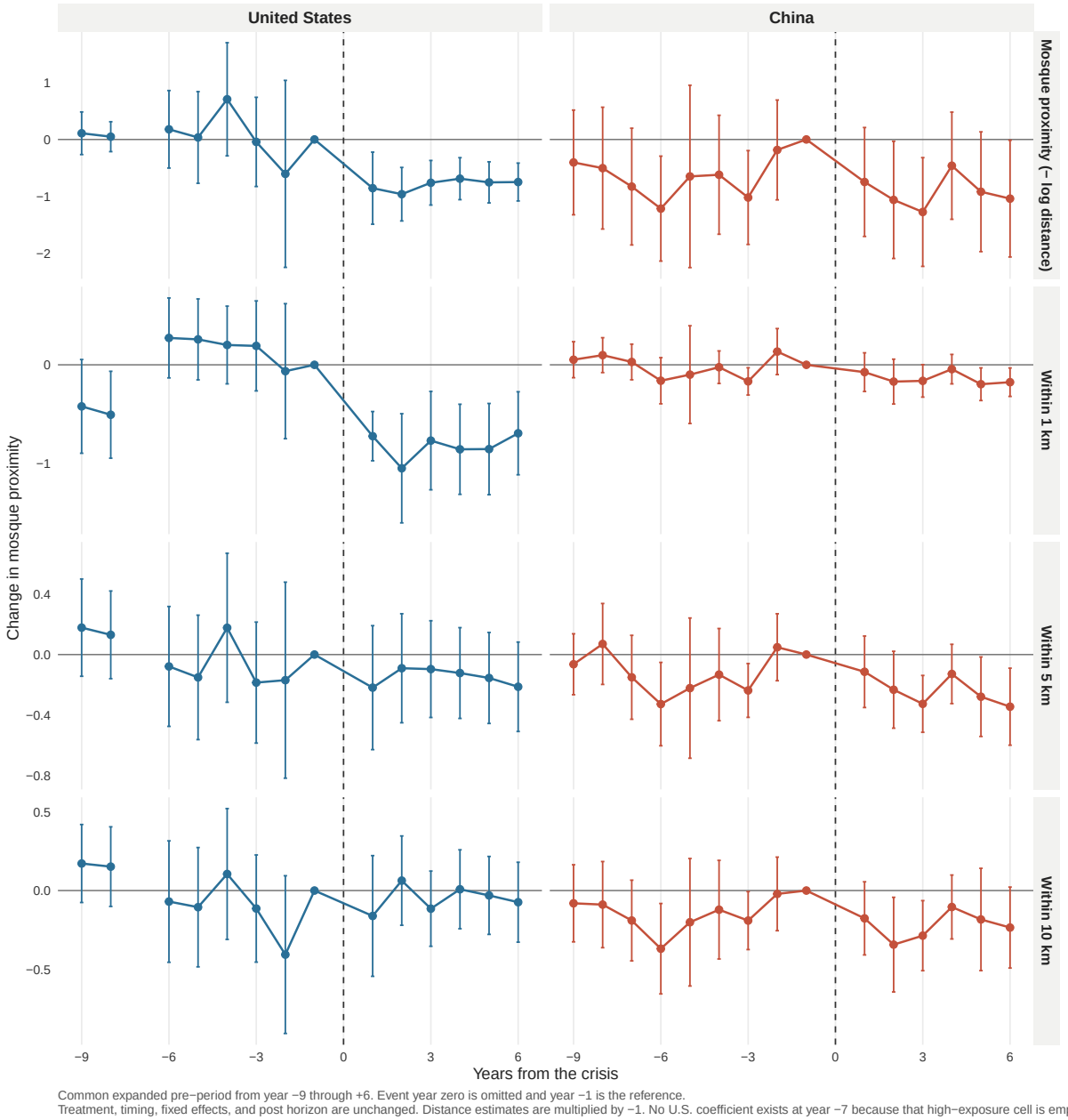


Figure 4: Event studies across all mosque-proximity outcomes. The panels use event years -9 through +6, omit the crisis year, use year -1 as the reference, and include the baseline fixed effects. The plotted distance coefficient and interval are multiplied by −1, so negative values mean relatively less mosque-proximate placement for every outcome. Bars are 95% confidence intervals based on recipient-clustered standard errors. The gap at U.S. event year -7 reflects the absence of a high-exposure project in that year.

6.5 Urban access and mosque specificity

The urban-access comparison asks whether a mosque pattern is part of a wider change in settlement geography. With logged travel time to a city of at least 50,000 as the outcome, the baseline high-exposure coefficient is 1.170 for U.S. projects (SE 0.582, $p = 0.052$) and 0.312 for Chinese projects (SE 0.149, $p = 0.043$). In both designs, high-exposure projects become relatively less accessible than lower-exposure projects (Appendix Table 10).

Appendix Figure 7 compares logged mosque distance at the same urban-access levels. For the United States, the post-crisis high-exposure contrast becomes negative at travel times from 10 to 60 minutes. Projects are closer to mosques once they are compared at the same accessibility level. For China, the distance contrast remains positive and is statistically distinguishable from zero at 30–50 minutes. The attenuation is more consistent with general remoteness in the U.S. case than in the Chinese case.

7 Conclusion

Foreign aid can be both an invitation and a liability. A donor may seek out skeptical communities because their opinions matter. The same donor may pull back when hostility makes persuasion unlikely and visibility dangerous. Treating project location as part of aid strategy makes these choices observable.

The formal model captures this tradeoff with a hump-shaped response. At low to moderate hostility, greater resistance can call forth more engagement. Beyond a turning point, additional hostility reduces visible effort. The value of foreign public support expands the engagement region, while exposure costs contract it. This framework joins research on soft power and attribution with work on strategic allocation and backlash (Dietrich et al., 2018, Goldsmith et al., 2014, Crost et al., 2014, Gong et al., 2024).

The empirical results identify one branch more clearly than the other. Chinese projects near Xinjiang show a broad negative post-2009 differential across the proximity measures. The difference was already developing before the riot, so the evidence supports a negative exposure gradient rather than a sudden riot effect. U.S. counterterrorism targets show only a narrow 1 km differential. The wider outcomes, precision check, alternative target coding, and urban-access comparison do not show a stable mosque-specific shift.

The donor-specific histories explain why the spatial predictions differ. Urumqi connected a domestic territorial conflict to transnational Muslim networks and risks borne by Chinese personnel, producing a clear avoidance prediction near Xinjiang. The pooled U.S. findings are consistent with offsetting outreach and security pressures after September 11, although sparse pre-crisis support limits the test. Official records document religious and community partnerships across multiple U.S. agencies, while the location data show no stable relative concentration of the pooled portfolio around mosques.

The comparison identifies project geography as a strategic margin distinct from national totals.

It also shows why a religious-space result should be compared with general settlement geography. These lessons complement evidence that aid effects depend on attribution, delivery, and local political context (Blair and Roessler, 2021, Blair et al., 2022, Dietrich, 2021, Dreher et al., 2022).

Location captures one margin of engagement. Donors can also work through local partners, communication, branding, and less visible delivery. Distinguishing these channels clarifies when hostility changes project geography and when it changes the way aid is implemented instead.

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A Formal Derivations and Proofs

This appendix establishes the claims summarized in the main text. Assume throughout that $r > 0$, $\Omega > 0$, and $\kappa \geq 0$. The donor solves

$$\max_{a \geq 0} \quad U(a, r, \Omega, \kappa) = \Omega \frac{a}{a + r} - (1 + \kappa r)a. \tag{A.1}$$

Proposition 1 (Optimal effort and exit)**.** *The donor's problem has a unique solution*

$$a^*(r, \Omega, \kappa) = \left[\sqrt{\frac{\Omega r}{1 + \kappa r}} - r \right]_+ . \tag{A.2}$$

The solution is positive if and only if $\Omega > r(1 + \kappa r)$*. The exit threshold is*

$$\bar{r} = \begin{cases} \Omega, & \kappa = 0, \\ \dfrac{\sqrt{1 + 4\kappa\Omega} - 1}{2\kappa}, & \kappa > 0. \end{cases} \tag{A.3}$$

Visible effort is positive for $0 < r < \bar{r}$ *and zero for* $r \geq \bar{r}$*.*

Proof. Differentiate utility with respect to effort:

$$U_a = \frac{\Omega r}{(a + r)^2} - (1 + \kappa r), \tag{A.4}$$

$$U_{aa} = -\frac{2\Omega r}{(a + r)^3} < 0. \tag{A.5}$$

Strict concavity gives at most one interior optimum. Setting Equation (A.4) to zero gives

$$a + r = \sqrt{\frac{\Omega r}{1 + \kappa r}}, \tag{A.6}$$

which yields the expression inside the positive-part operator in Equation (A.2).

At the boundary $a = 0$, the marginal payoff is

$$U_a(0) = \frac{\Omega}{r} - (1 + \kappa r). \tag{A.7}$$

It is positive exactly when $\Omega > r(1 + \kappa r)$. Strict concavity then implies an interior optimum. If it is weakly negative, utility falls from the boundary and $a^* = 0$.

The positive-effort condition becomes equality at the exit threshold. When $\kappa = 0$, $\Omega = \bar{r}$. When $\kappa > 0$, the positive root of $\kappa\bar{r}^2 + \bar{r} - \Omega = 0$ is Equation (A.3). The other root is negative and lies outside the parameter space. □

Proposition 2 (The engagement peak)**.** *Within the positive-effort region,*

$$\frac{\partial a^*}{\partial r} = \frac{\sqrt{\Omega}}{2\sqrt{r}(1+\kappa r)^{3/2}} - 1. \tag{A.8}$$

There is a unique turning point $r^\dagger \in (0, \bar{r})$ *defined by*

$$\Omega = 4r^\dagger(1+\kappa r^\dagger)^3. \tag{A.9}$$

Effort rises with hostility for $0 < r < r^\dagger$ *and falls for* $r^\dagger < r < \bar{r}$*. If* $\kappa = 0$*, then* $r^\dagger = \Omega/4$*.*

Proof. For an interior solution, rewrite Equation (A.2) as

$$a^* = \sqrt{\Omega r}(1+\kappa r)^{-1/2} - r. \tag{A.10}$$

Differentiation gives Equation (A.8). The derivative is zero when

$$\sqrt{\Omega} = 2\sqrt{r}(1+\kappa r)^{3/2}. \tag{A.11}$$

Both sides are nonnegative, so squaring preserves equivalence and gives Equation (A.9).

Define $g(r) = 4r(1+\kappa r)^3$. Its derivative is

$$g'(r) = 4(1+\kappa r)^2(1+4\kappa r) > 0. \tag{A.12}$$

Also, $g(0) = 0$ and $g(r)$ grows without bound. There is therefore a unique positive solution to $g(r) = \Omega$.

At the exit threshold, $\Omega = \bar{r}(1+\kappa\bar{r})$. The turning-point function evaluated there is

$$g(\bar{r}) = 4\bar{r}(1+\kappa\bar{r})^3 = 4(1+\kappa\bar{r})^2\Omega > \Omega. \tag{A.13}$$

Since g is strictly increasing, its crossing of Ω occurs before $\bar{r}$. Equation (A.8) is positive before that crossing and negative after it. Setting $\kappa = 0$ in Equation (A.9) yields $r^\dagger = \Omega/4$. □

Proposition 3 (Comparative statics)**.** *At every interior solution, effort rises with the value of foreign public support and falls with exposure cost:*

$$\frac{\partial a^*}{\partial \Omega} = \frac{1}{2}\sqrt{\frac{r}{\Omega(1+\kappa r)}} > 0, \tag{A.14}$$

$$\frac{\partial a^*}{\partial \kappa} = -\frac{r\sqrt{\Omega r}}{2(1+\kappa r)^{3/2}} < 0. \tag{A.15}$$

The exit threshold obeys

$$\frac{\partial \bar{r}}{\partial \Omega} = \frac{1}{1+2\kappa\bar{r}} > 0, \tag{A.16}$$

$$\frac{\partial \bar{r}}{\partial \kappa} = -\frac{\bar{r}^2}{1+2\kappa\bar{r}} < 0. \tag{A.17}$$

The turning point obeys

$$\frac{\partial r^\dagger}{\partial \Omega} = \frac{1}{4(1+\kappa r^\dagger)^2(1+4\kappa r^\dagger)} > 0, \tag{A.18}$$

$$\frac{\partial r^\dagger}{\partial \kappa} = -\frac{3(r^\dagger)^2}{1+4\kappa r^\dagger} < 0. \tag{A.19}$$

Proof. Equations (A.14) and (A.15) follow by differentiating Equation (A.10). For the exit threshold, define

$$F(\bar{r}, \Omega, \kappa) = \bar{r}(1+\kappa\bar{r}) - \Omega = 0. \tag{A.20}$$

Implicit differentiation gives

$$F_{\bar{r}} = 1 + 2\kappa\bar{r}, \qquad F_\Omega = -1, \qquad F_\kappa = \bar{r}^2. \tag{A.21}$$

Substituting these derivatives into the implicit-function formula gives Equations (A.16) and (A.17).

For the turning point, define

$$H(r^\dagger, \Omega, \kappa) = 4r^\dagger(1+\kappa r^\dagger)^3 - \Omega = 0. \tag{A.22}$$

The required partial derivatives are

$$H_{r^\dagger} = 4(1+\kappa r^\dagger)^2(1+4\kappa r^\dagger), \tag{A.23}$$

$$H_\Omega = -1, \tag{A.24}$$

$$H_\kappa = 12(r^\dagger)^2(1+\kappa r^\dagger)^2. \tag{A.25}$$

Implicit differentiation then gives Equations (A.18) and (A.19). □

Because $\Omega = \omega V$, Equation (A.14) also implies

$$\frac{\partial a^*}{\partial \omega} = V\frac{\partial a^*}{\partial \Omega} > 0, \qquad \frac{\partial a^*}{\partial V} = \omega\frac{\partial a^*}{\partial \Omega} > 0. \tag{A.26}$$

Accountability and the substantive value of foreign alignment enter through the same composite return, though they can have different political origins.

Corollary 1 (Spatial probability)**.** *Suppose project i is near the salient institution when*

$$N_i = \mathbf{1}\{m_i + \lambda a^* - \epsilon_i \geq 0\}, \tag{A.27}$$

where $\lambda > 0$ and ϵ_i has a standard logistic distribution. Then

$$\Pr(N_i = 1) = \Lambda(m_i + \lambda a^*), \tag{A.28}$$

and

$$\frac{\partial \Pr(N_i = 1)}{\partial r} = \lambda \Lambda(m_i + \lambda a^*) \left[1 - \Lambda(m_i + \lambda a^*)\right] \frac{\partial a^*}{\partial r}. \tag{A.29}$$

The probability of proximity therefore has the same engagement and avoidance regions as optimal effort.

Proof. The logistic cumulative distribution gives Equation (A.28). The chain rule gives Equation (A.29). Every term multiplying $\partial a^*/\partial r$ is positive, so both derivatives have the same sign. □

For a discrete crisis that changes hostility from r_0 to $r_1 > r_0$, the model predicts engagement when $a^*(r_1) > a^*(r_0)$ and avoidance when $a^*(r_1) < a^*(r_0)$. A shock that crosses $r^\dagger$ can move the donor from engagement to avoidance. A shock that crosses $\bar{r}$ removes diplomacy-motivated proximity while leaving baseline site suitability m_i in place.

B Robustness Results

Table 1: Project counts in the harmonized designs

Donor	Comparison group	Pre	Post
United States	Counterterrorism target	18	10,888
United States	Non-counterterrorism recipient	183	6,403
China	Far from Xinjiang	769	1,915
China	Near Xinjiang	544	1,010

Notes: The pre period includes the crisis year because only annual timing is available for the U.S. data. The main regressions define the first full year after each crisis as post.

Table 2: Construction and validation of mosque-proximity outcomes

Donor	Projects	Within 1 km	Within 5 km	Within 10 km	Distance observed	Buffers nested
United States	17,492	8,336	11,145	12,344	17,492	Yes
China	4,238	930	2,117	2,776	4,238	Yes

Notes: Every aid coordinate is matched only to mosque features in the same recipient country. Point features are used directly and polygon features are represented by an interior point. Distance is measured on the sphere, and all three buffers are derived from the same nearest distance.

Table 3: Descriptive pre- and post-crisis group means

Donor	Outcome	Group	Pre mean	Post mean	Change	Pre N	Post N
United States	Within 1 km	Counterterrorism target	0.778	0.474	-0.304	18	10,888
United States	Within 1 km	Non-counterterrorism recipient	0.344	0.484	0.139	183	6,403
United States	Within 5 km	Counterterrorism target	0.833	0.640	-0.194	18	10,888
United States	Within 5 km	Non-counterterrorism recipient	0.672	0.631	-0.041	183	6,403
United States	Within 10 km	Counterterrorism target	0.833	0.701	-0.132	18	10,888
United States	Within 10 km	Non-counterterrorism recipient	0.803	0.710	-0.093	183	6,403
United States	Log distance	Counterterrorism target	0.878	1.473	0.595	18	10,888
United States	Log distance	Non-counterterrorism recipient	1.427	1.462	0.034	183	6,403
China	Within 1 km	Far from Xinjiang	0.224	0.288	0.065	769	1,915
China	Within 1 km	Near Xinjiang	0.160	0.118	-0.042	544	1,010
China	Within 5 km	Far from Xinjiang	0.492	0.662	0.170	769	1,915
China	Within 5 km	Near Xinjiang	0.349	0.279	-0.070	544	1,010
China	Within 10 km	Far from Xinjiang	0.623	0.782	0.159	769	1,915
China	Within 10 km	Near Xinjiang	0.533	0.505	-0.028	544	1,010
China	Log distance	Far from Xinjiang	2.065	1.576	-0.489	769	1,915
China	Log distance	Near Xinjiang	2.319	2.396	0.076	544	1,010

Notes: These are unadjusted project-level means, not causal estimates. They decompose each relative coefficient into observed movement on the higher- and lower-exposure sides. Distance retains its original sign.

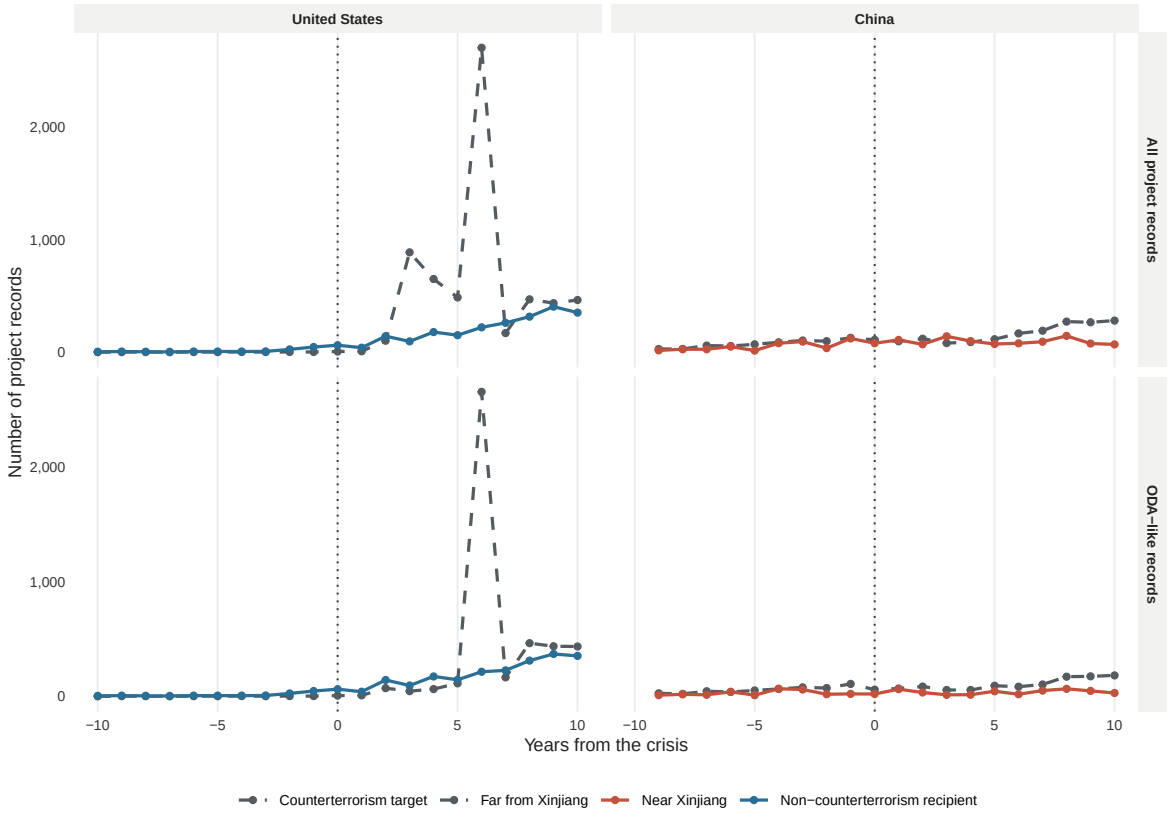


Figure 5: Annual project counts by exposure group. Panels distinguish all geocoded records from ODA-like records over a common ten-year event window. Counts do not measure commitment values, which are unavailable on a comparable basis for the pooled U.S. file.

Table 4: Baseline harmonized estimates of post-crisis aid placement

	Within 1 km	Within 5 km	Within 10 km	Log distance
United States	-0.437**	-0.054	0.045	0.283
	(0.210)	(0.098)	(0.116)	(0.314)
China	-0.106**	-0.133***	-0.112**	0.372**
	(0.041)	(0.046)	(0.044)	(0.182)
Recipient, year, sector-family, and flow fixed effects	Yes	Yes	Yes	Yes

Notes: Each cell is a separate project-level linear fixed-effects model. The U.S. coefficient is post-2001 times counterterrorism target. The China coefficient is post-2009 times near-Xinjiang recipient. Both focal groups represent higher expected exposure. Standard errors are clustered by recipient. Distance estimates retain their original sign in the table. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: Few-cluster inference for the baseline estimates

Donor	Outcome	Estimate	CRV1 SE	Conventional p	WCR p
United States	Within 1 km	-0.437	0.210	0.045	0.138
United States	Within 5 km	-0.054	0.098	0.581	0.571
United States	Within 10 km	0.045	0.116	0.701	0.731
United States	Log distance	0.283	0.314	0.373	0.423
China	Within 1 km	-0.106	0.041	0.015	0.024
China	Within 5 km	-0.133	0.046	0.006	0.024
China	Within 10 km	-0.112	0.044	0.015	0.040
China	Log distance	0.372	0.182	0.048	0.081

Notes: CRV1 standard errors and conventional p-values cluster by recipient. WCR p-values are two-sided restricted wild-cluster bootstrap-t tests with 999 six-point Webb draws at the recipient level, seed 20260824, the same draws for every outcome within donor, and the finite-draw +1 correction. The null is imposed using the fixed-effect-only restricted model. Only 6 U.S. and 8 Chinese high-exposure recipients contribute observations in both periods. This residual-based WCR check is not a leverage-adjusted WCR-S/CRV3 procedure and is not an exact randomization test. Monte Carlo error also remains with 999 draws.

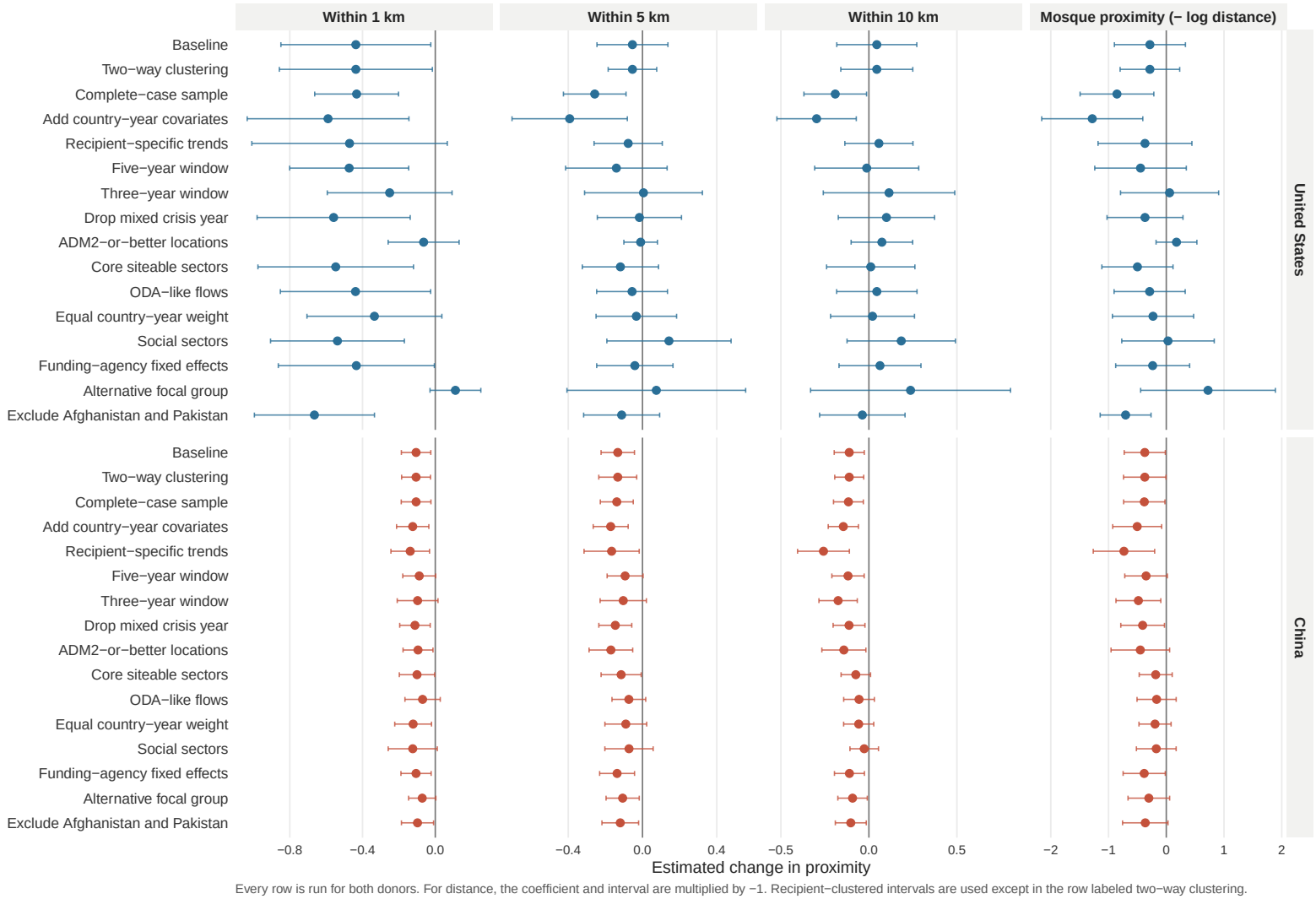


Figure 6: Harmonized specification curve. Every row is estimated for both donors and all four outcomes, with higher exposure coded as one in both cases. The plotted distance coefficients and intervals are multiplied by −1, so positive values mean greater proximity for every outcome. The complete-case row uses the same observations as the covariate row but omits the covariates.

Table 6: Alternative specifications: Within 1 km

Specification	U.S. estimate	U.S. N	China estimate	China N
Baseline	-0.437** (0.210)	17,492	-0.106** (0.041)	4,238
Two-way clustering	-0.437* (0.215)	17,492	-0.106** (0.041)	4,238
Complete-case sample	-0.433*** (0.118)	8,038	-0.106** (0.042)	4,179
Add country-year covariates	-0.590** (0.227)	8,038	-0.124*** (0.045)	4,179
Recipient-specific trends	-0.471* (0.274)	17,492	-0.138** (0.054)	4,238
Five-year window	-0.473*** (0.167)	2,910	-0.088* (0.046)	2,019
Three-year window	-0.251 (0.175)	1,427	-0.097* (0.057)	1,390
Drop mixed crisis year	-0.559** (0.215)	17,424	-0.112** (0.043)	4,048
ADM2-or-better locations	-0.064 (0.100)	3,989	-0.095** (0.042)	3,282
Core siteable sectors	-0.547** (0.218)	13,526	-0.100* (0.050)	3,695
ODA-like flows	-0.439** (0.211)	15,380	-0.070 (0.050)	2,456
Equal country-year weight	-0.335* (0.189)	17,492	-0.122** (0.052)	4,238
Social sectors	-0.538*** (0.188)	7,532	-0.124* (0.069)	1,632
Funding-agency fixed effects	-0.434* (0.219)	17,492	-0.106** (0.042)	4,238
Alternative focal group	0.111 (0.071)	17,492	-0.072* (0.038)	4,238
Exclude Afghanistan and Pakistan	-0.665*** (0.169)	14,349	-0.098** (0.045)	3,720

Notes: Every specification is run for both donors. The baseline uses recipient-clustered standard errors. The two-way row clusters by recipient and year. Complete-case and covariate rows use the same observations. The alternative U.S. group uses Afghanistan, Pakistan, and Iraq as direct theaters. The alternative China group uses recipients with a sizable Uyghur diaspora and includes Turkey. Stars follow the thresholds in Table 4.

Table 7: Alternative specifications: Within 5 km

Specification	U.S. estimate	U.S. N	China estimate	China N
Baseline	-0.054 (0.098)	17,492	-0.133*** (0.046)	4,238
Two-way clustering	-0.054 (0.067)	17,492	-0.133** (0.052)	4,238
Complete-case sample	-0.257*** (0.086)	8,038	-0.139*** (0.045)	4,179
Add country-year covariates	-0.392** (0.158)	8,038	-0.171*** (0.048)	4,179
Recipient-specific trends	-0.077 (0.094)	17,492	-0.166** (0.076)	4,238
Five-year window	-0.141 (0.140)	2,910	-0.094* (0.049)	2,019
Three-year window	0.005 (0.162)	1,427	-0.104 (0.064)	1,390
Drop mixed crisis year	-0.017 (0.116)	17,424	-0.147*** (0.045)	4,048
ADM2-or-better locations	-0.010 (0.046)	3,989	-0.170*** (0.060)	3,282
Core siteable sectors	-0.119 (0.105)	13,526	-0.115** (0.055)	3,695
ODA-like flows	-0.056 (0.097)	15,380	-0.074 (0.046)	2,456
Equal country-year weight	-0.033 (0.111)	17,492	-0.090 (0.057)	4,238
Social sectors	0.143 (0.171)	7,532	-0.073 (0.066)	1,632
Funding-agency fixed effects	-0.041 (0.105)	17,492	-0.137*** (0.048)	4,238
Alternative focal group	0.074 (0.246)	17,492	-0.107** (0.046)	4,238
Exclude Afghanistan and Pakistan	-0.112 (0.104)	14,349	-0.120** (0.050)	3,720

Notes: Every specification is run for both donors. The baseline uses recipient-clustered standard errors. The two-way row clusters by recipient and year. Complete-case and covariate rows use the same observations. The alternative U.S. group uses Afghanistan, Pakistan, and Iraq as direct theaters. The alternative China group uses recipients with a sizable Uyghur diaspora and includes Turkey. Stars follow the thresholds in Table 4.

Table 8: Alternative specifications: Within 10 km

Specification	U.S. estimate	U.S. N	China estimate	China N
Baseline	0.045 (0.116)	17,492	-0.112** (0.044)	4,238
Two-way clustering	0.045 (0.104)	17,492	-0.112** (0.042)	4,238
Complete-case sample	-0.191** (0.091)	8,038	-0.116** (0.043)	4,179
Add country-year covariates	-0.297** (0.115)	8,038	-0.145*** (0.044)	4,179
Recipient-specific trends	0.057 (0.099)	17,492	-0.258*** (0.075)	4,238
Five-year window	-0.012 (0.151)	2,910	-0.119** (0.047)	2,019
Three-year window	0.114 (0.191)	1,427	-0.175*** (0.056)	1,390
Drop mixed crisis year	0.100 (0.140)	17,424	-0.113** (0.046)	4,048
ADM2-or-better locations	0.074 (0.089)	3,989	-0.142** (0.064)	3,282
Core siteable sectors	0.011 (0.128)	13,526	-0.074* (0.043)	3,695
ODA-like flows	0.045 (0.116)	15,380	-0.056 (0.044)	2,456
Equal country-year weight	0.021 (0.122)	17,492	-0.058 (0.044)	4,238
Social sectors	0.184 (0.157)	7,532	-0.027 (0.041)	1,632
Funding-agency fixed effects	0.063 (0.119)	17,492	-0.111** (0.043)	4,238
Alternative focal group	0.237 (0.290)	17,492	-0.093** (0.043)	4,238
Exclude Afghanistan and Pakistan	-0.037 (0.124)	14,349	-0.103** (0.045)	3,720

Notes: Every specification is run for both donors. The baseline uses recipient-clustered standard errors. The two-way row clusters by recipient and year. Complete-case and covariate rows use the same observations. The alternative U.S. group uses Afghanistan, Pakistan, and Iraq as direct theaters. The alternative China group uses recipients with a sizable Uyghur diaspora and includes Turkey. Stars follow the thresholds in Table 4.

Table 9: Alternative specifications: Log distance

Specification	U.S. estimate	U.S. N	China estimate	China N
Baseline	0.283 (0.314)	17,492	0.372** (0.182)	4,238
Two-way clustering	0.283 (0.263)	17,492	0.372* (0.188)	4,238
Complete-case sample	0.854** (0.326)	8,038	0.380** (0.182)	4,179
Add country-year covariates	1.282*** (0.447)	8,038	0.503** (0.216)	4,179
Recipient-specific trends	0.369 (0.415)	17,492	0.733** (0.271)	4,238
Five-year window	0.446 (0.405)	2,910	0.350* (0.188)	2,019
Three-year window	-0.058 (0.434)	1,427	0.483** (0.198)	1,390
Drop mixed crisis year	0.368 (0.336)	17,424	0.409** (0.193)	4,048
ADM2-or-better locations	-0.177 (0.180)	3,989	0.449* (0.259)	3,282
Core siteable sectors	0.499 (0.314)	13,526	0.183 (0.146)	3,695
ODA-like flows	0.288 (0.314)	15,380	0.167 (0.173)	2,456
Equal country-year weight	0.229 (0.359)	17,492	0.195 (0.142)	4,238
Social sectors	-0.030 (0.409)	7,532	0.173 (0.176)	1,632
Funding-agency fixed effects	0.236 (0.327)	17,492	0.382** (0.186)	4,238
Alternative focal group	-0.723 (0.595)	17,492	0.302 (0.184)	4,238
Exclude Afghanistan and Pakistan	0.704*** (0.225)	14,349	0.364* (0.201)	3,720

Notes: Every specification is run for both donors. The baseline uses recipient-clustered standard errors. The two-way row clusters by recipient and year. Complete-case and covariate rows use the same observations. The alternative U.S. group uses Afghanistan, Pakistan, and Iraq as direct theaters. The alternative China group uses recipients with a sizable Uyghur diaspora and includes Turkey. Stars follow the thresholds in Table 4.

Table 10: Urban-access outcome across alternative specifications

Specification	U.S. estimate	U.S. N	China estimate	China N
Baseline	1.170* (0.582)	17,489	0.312** (0.149)	4,225
Two-way clustering	1.170** (0.561)	17,489	0.312* (0.157)	4,225
Complete-case sample	1.158 (0.868)	8,036	0.328** (0.149)	4,166
Add country-year covariates	1.526* (0.840)	8,036	0.441** (0.166)	4,166
Recipient-specific trends	1.269 (0.789)	17,489	0.503* (0.270)	4,225
Five-year window	0.927 (0.614)	2,909	0.344 (0.207)	2,012
Three-year window	-0.142 (0.623)	1,427	0.457** (0.221)	1,387
Drop mixed crisis year	1.479** (0.647)	17,421	0.370** (0.142)	4,036
ADM2-or-better locations	-1.093 (1.379)	3,989	0.347* (0.187)	3,272
Core siteable sectors	1.335** (0.601)	13,524	0.228 (0.162)	3,685
ODA-like flows	1.179* (0.586)	15,377	0.044 (0.197)	2,449
Equal country-year weight	0.966* (0.554)	17,489	0.253 (0.186)	4,225
Social sectors	0.665 (0.741)	7,530	-0.106 (0.155)	1,629
Funding-agency fixed effects	1.039 (0.631)	17,489	0.309* (0.153)	4,225
Alternative focal group	-0.609 (0.408)	17,489	0.223 (0.142)	4,225
Exclude Afghanistan and Pakistan	1.885*** (0.459)	14,346	0.317* (0.167)	3,707

Notes: The outcome is $\log(1 + \text{travel minutes})$ to the nearest city with at least 50,000 residents in 2000. Both coefficients compare the higher-exposure group with the lower-exposure group after the crisis. Positive values indicate relatively less accessible placement. Specifications otherwise match the mosque-outcome models.

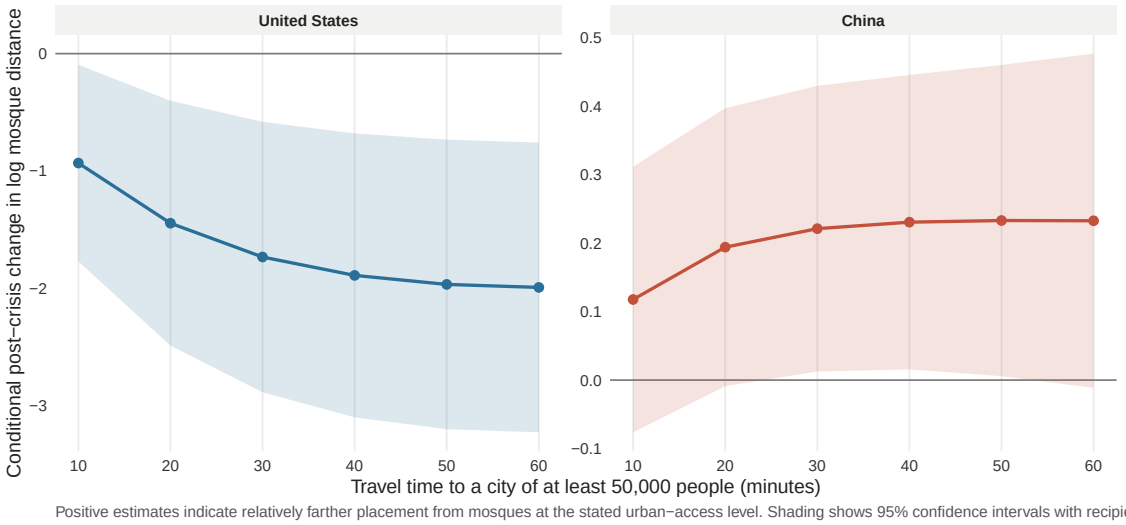


Figure 7: Post-crisis high-exposure contrast in logged mosque distance over urban accessibility. Positive values indicate relatively greater distance from mosques. Shading gives 95% confidence intervals from recipient-clustered models.

Table 11: Mosque-distance contrast conditional on urban access

Travel minutes	United States	China
10	-0.933**	0.118
	(0.414)	(0.096)
20	-1.446***	0.194*
	(0.515)	(0.100)
30	-1.735***	0.221**
	(0.569)	(0.103)
40	-1.892***	0.231**
	(0.598)	(0.106)
50	-1.969***	0.233**
	(0.610)	(0.112)
60	-1.994***	0.233*
	(0.610)	(0.120)

Notes: Each cell is the post-crisis higher-exposure contrast in log mosque distance evaluated at the stated urban-access level. Models interact the contrast with a three-degree-of-freedom natural spline, include the common fixed effects, and cluster by recipient. Positive values mean farther from mosques. The stored training basis is used for evaluation. Confidence intervals and p-values use the recipient-cluster t distribution.

Table 12: Harmonized event-study coefficients for placement within 10 km

Event year	United States	China
-6	-0.071	-0.348**
	(0.189)	(0.142)
-5	-0.107	-0.186
	(0.186)	(0.197)
-4	0.104	-0.110
	(0.204)	(0.152)
-3	-0.115	-0.181**
	(0.168)	(0.086)
-2	-0.408	-0.012
	(0.247)	(0.114)
-1	0.000	0.000
	(0.000)	(0.000)
1	-0.162	-0.161
	(0.189)	(0.112)
2	0.063	-0.325**
	(0.140)	(0.147)
3	-0.116	-0.272**
	(0.118)	(0.107)
4	0.008	-0.100
	(0.123)	(0.097)
5	-0.031	-0.167
	(0.122)	(0.156)
6	-0.074	-0.229*
	(0.125)	(0.123)

Notes: Year -1 is the reference. The crisis year is omitted. Both models use the same six-year window, fixed effects, outcome, and recipient-clustered standard errors.

Table 13: Joint tests of event-study leads

Donor	Test statistic	Degrees of freedom	P-value
United States	4.012	5	0.548
China	28.589	5	0.000

Notes: The null is that event years -6 through -2 are jointly zero. The test uses the covariance matrix from the recipient-clustered event-study model.

Table 14: Joint pretrend tests across all mosque-proximity outcomes

Design	Outcome	U.S. F	U.S. p	U.S. high-pre N	China F	China p	China high-pre N
Matched window (-6 to +6)	Mosque proximity (- log distance)	0.68	0.639	9	5.85	< 0.001	396
Matched window (-6 to +6)	Within 1 km	1.77	0.115	9	3.16	0.008	396
Matched window (-6 to +6)	Within 5 km	0.74	0.595	9	3.59	0.003	396
Matched window (-6 to +6)	Within 10 km	0.80	0.548	9	5.72	< 0.001	396
Expanded pre-period (-9 to +6)	Mosque proximity (- log distance)	0.76	0.622	11	8.53	< 0.001	464
Expanded pre-period (-9 to +6)	Within 1 km	10.29	< 0.001	11	2.50	0.010	464
Expanded pre-period (-9 to +6)	Within 5 km	2.96	0.004	11	4.81	< 0.001	464
Expanded pre-period (-9 to +6)	Within 10 km	7.62	< 0.001	11	4.72	< 0.001	464

Notes: Tests jointly set every estimable lead coefficient through event year -2 to zero and use recipient-clustered F reference distributions. Coefficient-specific degrees of freedom are stored in the accompanying CSV. Year -1 is the reference and the mixed crisis year is omitted. The expanded design uses the longest common pre-period in both files (-9 through -1) while retaining the six-year post horizon and all other estimand choices. It raises U.S. high-exposure pre-period support from 9 to 11 projects but adds no high-exposure recipient. Continuous log distance is listed first by measurement logic, not statistical significance, and every prespecified buffer is shown.

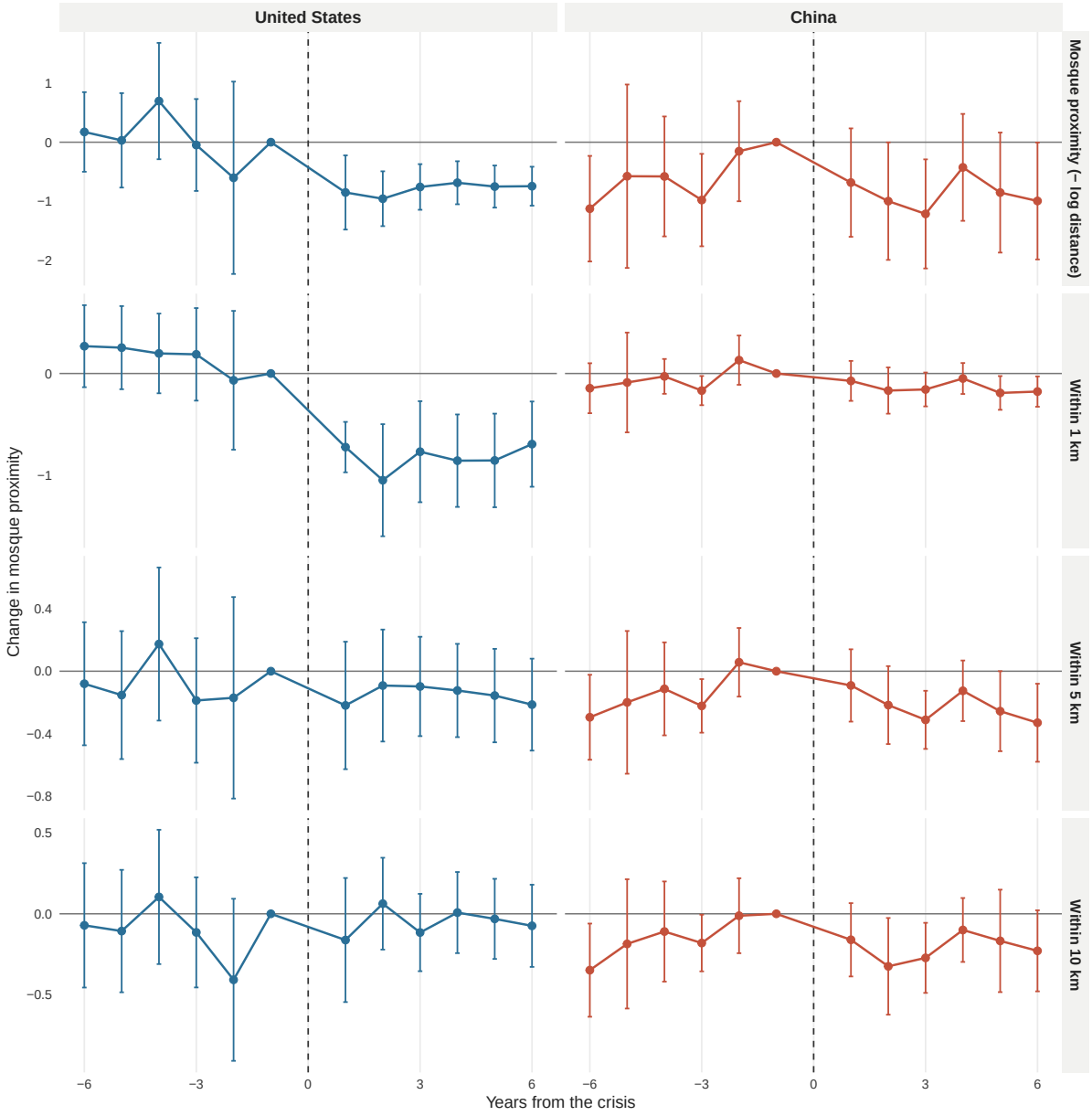


Figure 8: Event studies in the matched six-year window. The figure shows logged nearest distance and the 1, 5, and 10 km outcomes for both donors. Event year -1 is the reference and event year zero is omitted. The plotted distance coefficients and intervals are multiplied by −1, so negative values indicate relatively less mosque-proximate placement for every outcome.

Table 15: Matched two-period local contrasts

Outcome	Years compared	United States	China
Within 1 km	-1 and +1	-0.422**	-0.064
		(0.199)	(0.110)
Within 1 km	-2 and +2	-0.356*	-0.171
		(0.188)	(0.112)
Within 1 km	-3 and +3	–	-0.079
			(0.113)
Within 5 km	-1 and +1	-0.276	-0.096
		(0.224)	(0.094)
Within 5 km	-2 and +2	0.167	-0.218
		(0.194)	(0.156)
Within 5 km	-3 and +3	–	-0.057
			(0.123)
Within 10 km	-1 and +1	-0.218	-0.157
		(0.194)	(0.099)
Within 10 km	-2 and +2	0.632***	-0.337
		(0.045)	(0.201)
Within 10 km	-3 and +3	–	-0.152
			(0.117)
Log distance	-1 and +1	0.455	0.449
		(0.357)	(0.346)
Log distance	-2 and +2	-0.712**	0.738
		(0.301)	(0.452)
Log distance	-3 and +3	–	0.445
			(0.340)

Notes: Each row compares one pre-crisis year with the equally distant post-crisis year. The same event-year contrasts are applied to both donors.

Table 16: Common annual local-linear break diagnostics

Outcome	Symmetric window	United States	China
Within 1 km	±3 years	-0.851***	-0.164
		(0.236)	(0.165)
Within 1 km	±5 years	-0.692***	-0.113
		(0.236)	(0.143)
Within 1 km	±6 years	-0.941**	-0.121
		(0.347)	(0.123)
Within 5 km	±3 years	-0.350	-0.202
		(0.282)	(0.131)
Within 5 km	±5 years	-0.031	-0.178
		(0.224)	(0.129)
Within 5 km	±6 years	0.029	-0.162
		(0.216)	(0.110)
Within 10 km	±3 years	0.039	-0.199*
		(0.336)	(0.106)
Within 10 km	±5 years	0.142	-0.300**
		(0.248)	(0.120)
Within 10 km	±6 years	0.200	-0.293***
		(0.219)	(0.082)
Log distance	±3 years	1.050	0.795*
		(0.712)	(0.417)
Log distance	±5 years	0.533	0.889*
		(0.615)	(0.464)
Log distance	±6 years	0.572	0.872**
		(0.556)	(0.385)

Notes: The crisis year is omitted. Models allow the higher-exposure group's linear slope to differ before and after the crisis, absorb common annual shocks with year fixed effects, and cluster by recipient. The coefficient is the estimated differential break at event time zero. Annual U.S. timing does not support a regression-discontinuity interpretation.

Table 17: China exact-date local-linear diagnostics

Outcome	Date sample	Bandwidth	Estimate	N
Within 1 km	All dates	±0.5 years	-0.315 (0.295)	213
Within 1 km	Reported dates only	±0.5 years	-0.412 (0.332)	103
Within 1 km	All dates	±1 years	0.103 (0.250)	467
Within 1 km	Reported dates only	±1 years	0.185 (0.343)	262
Within 1 km	All dates	±1.5 years	-0.092 (0.103)	671
Within 1 km	Reported dates only	±1.5 years	-0.241 (0.213)	361
Within 1 km	All dates	±2 years	-0.153 (0.110)	824
Within 1 km	Reported dates only	±2 years	-0.170 (0.195)	430
Within 5 km	All dates	±0.5 years	-0.432** (0.196)	213
Within 5 km	Reported dates only	±0.5 years	0.195 (0.379)	103
Within 5 km	All dates	±1 years	-0.010 (0.231)	467
Within 5 km	Reported dates only	±1 years	0.041 (0.205)	262
Within 5 km	All dates	±1.5 years	-0.060 (0.130)	671
Within 5 km	Reported dates only	±1.5 years	-0.323 (0.194)	361
Within 5 km	All dates	±2 years	-0.202 (0.130)	824
Within 5 km	Reported dates only	±2 years	-0.289 (0.177)	430
Within 10 km	All dates	±0.5 years	-0.295 (0.407)	213
Within 10 km	Reported dates only	±0.5 years	-1.516*** (0.480)	103
Within 10 km	All dates	±1 years	-0.040 (0.310)	467
Within 10 km	Reported dates only	±1 years	-0.235 (0.312)	262
Within 10 km	All dates	±1.5 years	-0.217 (0.133)	671
Within 10 km	Reported dates only	±1.5 years	-0.385* (0.201)	361
Within 10 km	All dates	±2 years	-0.298*** (0.104)	824
Within 10 km	Reported dates only	±2 years	-0.363** (0.170)	430
Log distance	All dates	±0.5 years	1.770** (0.795)	213
Log distance	Reported dates only	±0.5 years	2.076** (0.891)	103
Log distance	All dates	±1 years	0.366 (0.946)	467
Log distance	Reported dates only	±1 years	0.604 (0.988)	262

Outcome	Date sample	Bandwidth	Estimate	N
Log distance	All dates	±1.5 years	1.037*** (0.367)	671
Log distance	Reported dates only	±1.5 years	1.689*** (0.591)	361
Log distance	All dates	±2 years	1.134*** (0.296)	824
Log distance	Reported dates only	±2 years	1.319** (0.523)	430

Notes: The running variable is years from July 5, 2009. Models include side- and group-specific local-linear slopes, recipient, year, sector-family, and flow fixed effects, and recipient-clustered standard errors. In the ±0.5-year raw window, 110 of 213 commitment dates are imputed and 97 of those heap on January 1. N is the fixed-effect estimation sample after singleton removal. The results are diagnostics, not a causal RD.

Table 18: Matched pre-crisis placebo estimates

Outcome	Pseudo-crisis year	United States	China
Within 1 km	-4	0.303	-0.024
		(0.281)	(0.064)
Within 1 km	-3	0.350	0.116**
		(0.295)	(0.054)
Within 1 km	-2	0.409	0.001
		(0.382)	(0.064)
Within 5 km	-4	0.159	0.060
		(0.187)	(0.081)
Within 5 km	-3	0.179	0.201***
		(0.200)	(0.072)
Within 5 km	-2	0.194	0.113
		(0.268)	(0.102)
Within 10 km	-4	0.224	0.074
		(0.273)	(0.075)
Within 10 km	-3	0.238	0.169**
		(0.297)	(0.064)
Within 10 km	-2	0.341	0.112
		(0.355)	(0.094)
Log distance	-4	-0.552	-0.136
		(0.583)	(0.254)
Log distance	-3	-0.613	-0.642***
		(0.621)	(0.221)
Log distance	-2	-0.706	-0.476
		(0.817)	(0.334)

Notes: Each placebo uses only pre-crisis observations. The pseudo-crisis year is expressed relative to the true crisis. Specifications match the baseline fixed effects and recipient clustering.

Table 19: Leave-one-country-out estimates for placement within 1 km

Donor	Country omitted	Estimate	N
United States	Afghanistan	-0.516** (0.206)	15,675
United States	Iran	-0.437** (0.210)	17,489
United States	Iraq	-0.437** (0.211)	11,903
United States	Libya	-0.437** (0.210)	17,456
United States	Mali	-0.432* (0.239)	16,682
United States	Niger	-0.462* (0.238)	17,162
United States	Pakistan	-0.549** (0.206)	16,166
United States	Somalia	-0.435** (0.210)	16,807
United States	Syria	-0.147 (0.110)	17,319
United States	Tunisia	-0.452** (0.219)	17,355
China	Afghanistan	-0.096** (0.041)	4,133
China	Iran	-0.088** (0.037)	4,105
China	Kazakhstan	-0.113** (0.043)	4,061
China	Kyrgyzstan	-0.103** (0.042)	4,088
China	Pakistan	-0.113** (0.047)	3,825
China	Tajikistan	-0.117** (0.044)	3,957
China	Turkmenistan	-0.105** (0.041)	4,207
China	Uzbekistan	-0.120** (0.045)	3,974

Notes: The U.S. models remove one counterterrorism target at a time. The China models remove one near-Xinjiang recipient at a time. All other choices match the baseline model.

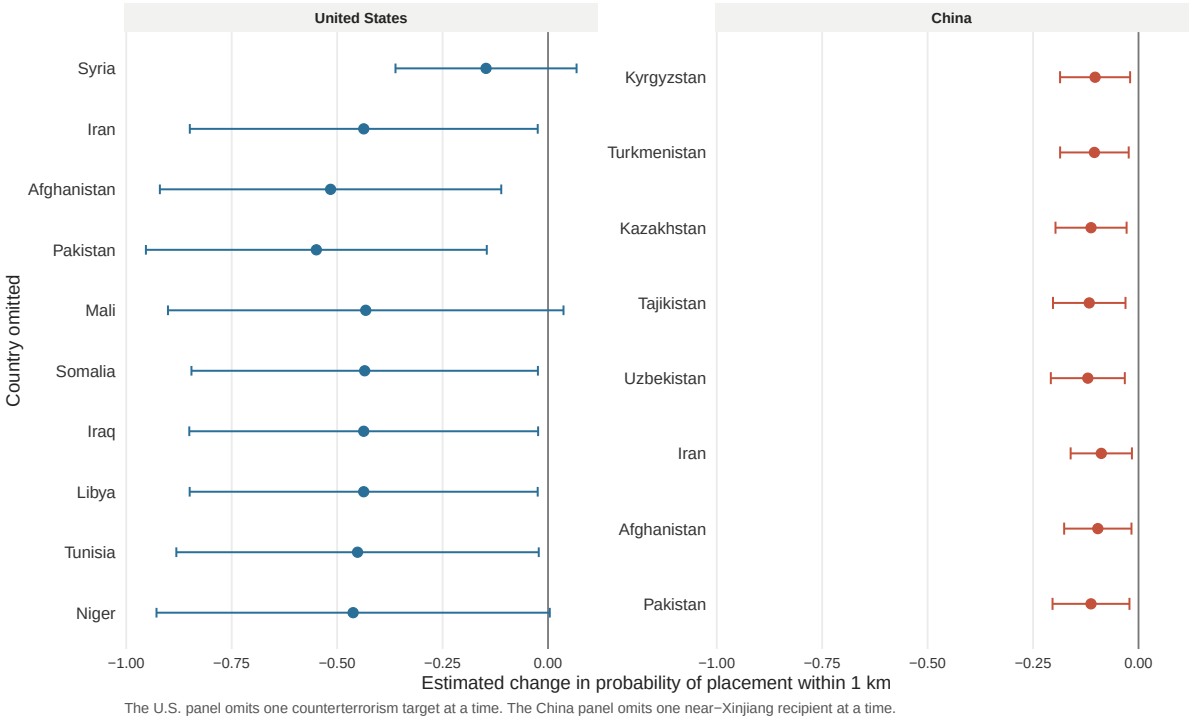


Figure 9: Leave-one-country-out estimates for placement within 1 km. The U.S. panel removes one counterterrorism target at a time. The China panel removes one near-Xinjiang recipient at a time. Bars are 95% confidence intervals.

C Data and Coding Details

C.1 Country groups

The baseline U.S. high-exposure group contains Afghanistan, Iran, Iraq, Libya, Mali, Niger, Pakistan, Somalia, Syria, Tunisia, and Yemen. All other Muslim-majority recipients in the working file form the lower-exposure group. The alternative direct-theater group contains Afghanistan, Pakistan, and Iraq.

The baseline China high-exposure group contains Afghanistan, Iran, Kazakhstan, Kyrgyzstan, Pakistan, Tajikistan, Turkmenistan, and Uzbekistan. The alternative diaspora group contains Afghanistan, Kazakhstan, Kyrgyzstan, Pakistan, Tajikistan, Turkey, Turkmenistan, and Uzbekistan. It replaces Iran with Turkey.

Using binary exposure groups gives both donor coefficients the same interpretation. The alternative definitions show how the estimates change when U.S. exposure is limited to direct theaters and Chinese exposure is based on diaspora ties.

C.2 Time coding

The U.S. post indicator equals one when transaction year is greater than 2001. Transaction year records a commitment or disbursement in GODAD. The China post indicator equals one when commitment year is greater than 2009. The crisis years remain in the pre category for the baseline annual models. The event studies omit event year zero because it mixes dates before and after each shock.

The matched design uses annual timing because the U.S. file does not supply a common day-level date. Appendix Table 17 reports Chinese local-linear estimates using July 5, 2009. It shows all recorded dates and then the subset reported as exact by the source.

C.3 Sector families and flows

Both donors are mapped into social infrastructure and services, economic infrastructure and services, production sectors, and other activities. Baseline models include family fixed effects. The siteable sample retains the first three categories and drops other activities. This removes many emergency, commodity, budget-support, and unspecified records.

The ODA-like sample uses the common flow label supplied by each source. It does not make U.S. and Chinese projects institutionally identical. It excludes nonconcessional and other-official-flow records.

C.4 Precision crosswalk

The U.S. file contains 11,402 observations coded by GODAD as reliable at least at ADM1, 3,989 coded as reliable at least at ADM2, and 2,101 AIMS-sourced observations without `precision_crs`.

The raw Google score is 12 for 12,018 observations. Since 12 is the least precise category on that scale, it must not be interpreted as the best precision code.

The China file contains 956 ADM1 and 3,282 ADM2 locations. The shared ADM2-or-better model keeps U.S. `precision_crs`=4 and Chinese `admin_level`=`ADM2`. The two fields come from different geocoding systems. The restriction is a practical common denominator, not a claim of exact equivalence.

C.5 Spatial distance construction

The analysis reads every country GeoJSON with `sf`, transforms mixed OpenStreetMap point and polygon features to one interior point per mosque, and calculates spherical nearest-neighbor distance within the recipient country. All three buffer indicators are derived from that single continuous distance. Appendix Table 2 reports the number of projects within each buffer, verifies that the buffers are nested, and confirms complete distance coverage.

C.6 Specification definitions

The baseline includes recipient, year, sector-family, and flow fixed effects with recipient-clustered standard errors. The two-way model adds year clustering. The complete-case model drops observations missing any of the five common covariates. The covariate model uses that same sample and adds logged organized-violence deaths, liberal democracy, logged population, logged GDP per capita, and logged bilateral trade.

The trends model adds one linear time slope per recipient. Window models keep event years -5 through +5 or -3 through +3. Equal country-year weighting assigns every project the inverse of the number of projects in its recipient-year cell. The social-sector model keeps social infrastructure and services. Agency fixed effects absorb funding-family composition. The alternative-group model changes only the high-exposure definition, and the final shared row excludes Afghanistan and Pakistan.

C.7 Reproducibility

The combined folder stores snapshots of both project files and the urban-access benchmark in `data/raw`. The analysis reads country mosque layers from the sibling `mosque-data` directory. Running

```
sh build.sh
```

from the combined project root generates the harmonized data, model results, figures, appendix tables, and PDF. Machine-readable estimates are written to `data/derived`. The primary script is `code/01_harmonized_analysis.R`.

The code records all sixteen specifications for both donors and all four mosque outcomes, repeats them for urban access, and does not select models based on statistical significance.