Aid Cuts and the Politics of Refugee Hosting

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Abstract

In 2025, foreign aid contracted at the fastest rate on record, driven by U.S. cuts to USAID. Does exposure to aid cuts reduce host citizens' support for hosting refugees? I develop a formal model in which aid cuts push exposed hosts in two directions: the contraction of the refugee-aid economy creates material losses that can generate backlash, while proximity to refugee suffering can sustain support through humanitarian concern. Testing the theory against the 2025 cuts in Kenya—using two nationwide surveys, a high-frequency panel in a major hosting region during the height of the cuts, a vignette experiment, and 52 refugee-leader interviews—I find that exposure reduces support. Citizens in the hosting region withdrew support for hosting refugees in their area as aid was cut, even though support nationwide did not fall. This backlash is concentrated among households who personally lost benefits such as access to refugee-aid services or employment, driven by egocentric loss rather than sociotropic concern. Yet humanitarian concern is widespread, and those with high empathy for refugees do not backlash even in the face of severe cuts. These findings suggest that continued cuts could weaken support for hosting across the low- and middle-income countries that provide asylum to most of the world's refugees.

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1 Introduction

In 2025, foreign aid contracted at the fastest rate on record: aid from OECD countries fell by 23.1 percent, largely driven by U.S. cuts to USAID spending (OECD, 2026). These cuts are expected to have severe consequences for aid-recipient populations, including an estimated 1.2 to 2.6 million excess deaths (Kenny and Sandefur, 2025). The world's 42.5 million refugees are exposed through two channels. First, cuts to major refugee organizations reduce access to food, medical care, schooling, and employment, worsening welfare and increasing preventable mortality.[1] Second, aid cuts may alter the politics of refugee hosting. 71% of refugees live in low- and middle-income countries (LMICs), where governments often rely on donor financing to sustain hosting (Betts, Loescher and Milner, 2012; Dempster and Ginn, 2026). When that funding is reduced, citizens and governments in asylum countries may become less willing to host refugees, increasing pressure for them to return to unsafe origin countries or move onward to third countries.

Motivated by these political consequences, this paper asks whether exposure to refugee aid cuts reduces host citizens' support for refugee hosting. Existing research pulls in two directions. Unlike in many high-income countries, exposure to refugees in LMICs can increase support for hosting because refugees often bring aid, jobs, and infrastructure (Zhou et al., 2025; Baseler et al., 2023; Demir, Mayda and Maystadt, 2025). On this account, withdrawing aid may lead to backlash where the aid economy is most concentrated, as some citizens lose jobs and services and others worry about the broader consequences of hosting with less international aid (Hainmueller and Hopkins, 2014; Weber et al., 2024). A second literature suggests the opposite: refugee hosting is often supported because of humanitarian concerns, particularly in LMICs (Newman et al., 2015; Pettigrew and Tropp, 2006; Ghosn, Braithwaite and Chu, 2019). Aid cuts impose severe harms on refugees, particularly acute hunger. Citizens most exposed face losses from the cuts, but they also have the most contact with refugees and see their suffering firsthand, which may sustain or deepen support.

I build a formal model to show that, given these opposing dynamics, the net effect of aid cuts on attitudes is not obvious *ex ante*. The model predicts that backlash concentrates where exposure to the aid economy is highest, that it is driven by households that personally lose benefits or absorb new costs rather than by their unaffected neighbors, and that humanitarian concern pulls in the opposite direction, limiting backlash and, among the most empathetic, potentially increasing support. I test the theory against the 2025 U.S. aid cuts in Kenya, a major hosting country and one of the largest pre-cut U.S. aid recipients. I draw on two nationally representative phone surveys bracketing the cuts, a high-frequency face-to-face panel in the hosting region of Kakuma refugee camp during the height of the cuts, a pre-registered vignette experiment, and 52 refugee-leader interviews.

The analysis yields three findings. First, support for refugees declined in the regions most exposed to aid cuts. The literature's predictions about the economic losses hosts face are borne out: those closest to refugees show declines in support across the nationwide, panel, experimental, and qualitative data. Yet there was no nationwide backlash; if anything, support rose modestly between 2023 and 2026. The net result is convergence, with hosting regions starting higher and moving toward the national average. Second, this regional backlash is driven by personal loss and concerns about insecurity, not sociotropic economic concerns. It is concentrated among households who personally lose aid-related benefits; their no-loss neighbors do not backlash.

Third, empathy can limit backlash, helping explain why nationwide support did not decline. Citizens often discuss their support for refugees in terms of humanitarian concerns, both at the nationwide and

[1] The United States funded roughly 40% of UNHCR's budget; funding fell 73% between 2024 and 2025 (UNHCR, 2025*b*,*c*). WFP funding fell 34%, affecting 16.7 million people (World Food Programme, 2025). Recent studies document the harms that cuts to WFP rations, predating the 2025 cuts, impose on refugees (Grossman et al., 2025; Sterck and Bruni, 2025).

local level. The main determinant of support for hosting is whether conflict is ongoing in the country of origin, rather than aid conditions, and high-empathy respondents do not change their support for hosting even when aid is entirely cut. The key takeaway of the findings is that aid cuts can weaken support for refugee hosting where LMIC hosts depend on the aid economy. In this case, the decline remains modest and localized, with empathy limiting the spread of backlash nationwide.

This study makes three contributions. First, the paper speaks to a debate in the immigration-attitudes literature over whether sociotropic, egocentric, or humanitarian concerns drive attitudes. The prevailing view holds that sociotropic concern about national effects predicts attitudes more than personal economic loss (Hainmueller and Hopkins, 2014; Weber et al., 2024). Aid cuts offer leverage on this question, since a single shock raises egocentric and sociotropic concerns while also heightening humanitarian concern. In this setting, I find that egocentric loss and humanitarian concern drive attitudes, and that insecurity matters more than economic cost. This is consistent with work showing that empathy shapes attitudes toward vulnerable groups more than rational-economic models predict (Newman et al., 2015), particularly in LMICs (Alrababa'h et al., 2021; MacDonald and Lichtenheld, 2026; Hartman, Morse and Weber, 2021).

Second, the paper contributes to long-running debates over the effects of foreign aid, and to pressing policy questions raised by the U.S.' large-scale withdrawal of assistance. While existing research has focused on the provision of aid and its effects on recipients and donors (e.g., Bermeo, 2017; Bearce and Tirone, 2010), less is known about the political consequences of sudden retrenchment in aid-recipient societies. I provide systematic evidence on this question, as well as qualitative evidence on the severe, distressing consequences of aid cuts for refugees as they unfolded. The findings raise broader questions about the long-term options available to refugees when aid contracts, particularly the possibility of local integration and policies that provide freedom to move and work rather than remain dependent on aid (MacDonald and Lichtenheld, 2026; Hovil and Maple, 2022).

Third, the paper provides real-time evidence on a major international shock. I designed the study as the cuts were announced, before their trajectory was known, and fielded a high-frequency panel that ultimately captured both the sharpest contraction and its partial recovery. This was a setting with high uncertainty, potential insecurity, and restrictions on access, making data collection difficult. I therefore use a range of quantitative and qualitative evidence to triangulate: no single design identifies the effect of the cuts on its own, but the panel, two nationally representative surveys, a vignette experiment, and refugee-leader interviews converge on the same pattern, with the interviews and open-ended responses interpreting the quantitative results and tracing the wave-to-wave changes the surveys alone would miss. The design illustrates how fast-moving policy shocks can be studied as they unfold, and the value of triangulating across a mixed-method design.

The paper proceeds as follows. I first develop the theory and hypotheses, then describe the Kenyan setting, the sequence of aid cuts, and the research design. I next present results across the national and regional levels before discussing robustness checks and concluding.

2 Theory

Two literatures generate opposite predictions about how aid cuts might affect host attitudes toward refugees. The first anticipates backlash: aid cuts impose economic costs on hosts, both personally and on their communities, and where refugee support relies on the material returns of hosting, withdrawing aid may reduce it. By contrast, a second literature suggests that empathy may dampen backlash: the same cuts that impose costs on hosts inflict acute suffering on refugees, which may instead sustain or increase support.

2.1 Economic costs and backlash

In LMICs, refugee hosting can bring material benefits to host communities. Camps often bring international organizations and aid to remote border regions, generating jobs, business opportunities, and access to services for local residents (Alix-Garcia et al., 2018; International Finance Corporation, 2018; World Bank and UNHCR, 2021; Zhou, Grossman and Ge, 2023). Zhou et al. (2025) finds that refugee settlements in Uganda improve local infrastructure, including schools, roads, and health clinics, while Alix-Garcia et al. (2018) show that refugee inflows in Kenya increased local economic activity, measured using nighttime lights. While most present in camp settings, there can be broader economic gains to hosting. Refugee aid can also benefit refugees and hosts in urban areas, support national public services and infrastructure, and become linked to wider development financing (Betts, Loescher and Milner, 2012; Dempster and Ginn, 2026). Consistent with this logic, greater exposure to the material returns of hosting is associated with higher support for refugees and incumbent politicians (Baseler et al., 2023; Zhou et al., 2025; MacDonald and Lichtenheld, 2026; Demir, Mayda and Maystadt, 2025).

Aid cuts threaten these benefits through two channels, both of which increase with exposure. The first is egocentric: locals employed by NGOs or refugee households may lose work, those using camp-area services may lose access, businesses selling to refugees may lose customers, and rising refugee deprivation may generate theft or resource conflict. Egocentric concerns should therefore produce backlash among those who personally lose employment, services, customers, or security.

The second is sociotropic: citizens who are not personally harmed may nonetheless worry about the consequences for their community or country. They may fear that the state will assume more of the fiscal cost of hosting, that refugees will compete for public services, or that camp closures will displace people into urban areas. Because evidence on immigration suggests that sociotropic concerns often outweigh personal economic vulnerability in shaping attitudes, aid cuts may also produce more diffuse backlash among those not directly harmed (Hainmueller and Hopkins, 2014; Bansak, Hainmueller and Hangartner, 2023; Weber et al., 2024; Aviña et al., 2026). Since both personal and community losses are highest near camps and settlements, where the aid economy is densest, support should decline most in exposed refugee-hosting regions, with the largest declines among those personally exposed and possible spillovers to their unaffected neighbors.

2.2 Humanitarian concern and support

A second literature predicts the opposite response, because attitudes toward refugees are shaped by empathy and humanitarian concern as well as by economic considerations. Citizens are consistently more welcoming toward refugees who have suffered severe physical or mental distress than toward those fleeing economic hardship (Bansak, Hainmueller and Hangartner, 2016, 2023), and humanitarian concern itself reduces support for restrictive immigration policy, an effect that is stronger among more empathetic individuals (Newman et al., 2015). People who have themselves endured violence host more refugees and respond more strongly to refugee distress, including across group boundaries (Hartman and Morse, 2020; Hartman, Morse and Weber, 2021), and perspective-taking and empathy-priming exercises raise willingness to support refugees and immigrants (Adida, Lo and Platas, 2018; Williamson et al., 2021).

Aid cuts likely activate humanitarian concerns toward refugees. Reductions in food, healthcare, education, and cash assistance produce immediate welfare losses for refugees, including hunger, service closures, and harmful coping strategies. These losses are especially severe where refugees lack the legal right to work, move freely, or access state services. Aid cuts therefore differ from many policy shocks studied in the immigration literature: they impose costs on hosts while also worsening conditions for refugees. Hosts may lose income, services, or security, but refugees often lose access to basic needs. Because the same shock harms both groups, aid cuts can generate economic resentment and

humanitarian concern at the same time, creating a countervailing channel through which support for hosting may persist.

Humanitarian concerns may be largest among citizens most exposed. A large literature on intergroup contact finds that proximity and sustained interaction foster empathy and more favorable outgroup attitudes (Pettigrew and Tropp, 2006; Ghosn, Braithwaite and Chu, 2019). Hosts in high-exposure regions are thus more likely to witness the deprivation that cuts produce, and more likely to be aware of it through their social ties. The exposure that makes hosts vulnerable to aid cuts also makes refugee suffering more visible and concerning, which may preserve or increase support where empathy is strong.

2.3 Formalization

I formalize this theory as follows. Each host i's support for refugee hosting, H_i, combines three channels—egocentric, sociotropic, and empathy—capturing how much the host's own household, the community, and the refugees each gain or lose from hosting, weighted by how much i values them:

$$H_i = \underbrace{\alpha_i \theta_i}_{\text{egocentric (household)}} + \underbrace{\beta_i E_i \big[\sigma A - C(A)\big]}_{\text{sociotropic (community)}} + \underbrace{\gamma_i \big[E_i S(A) + \tfrac{1}{2} S(A)\big]}_{\text{empathy (refugees)}} \tag{1}$$

The egocentric channel captures the host's household welfare: α_i is how much i values it, and $\theta_i \in \{-1, 0, 1\}$ indicates whether i's household holds an aid-related benefit or bears an aid-related cost. Aid cuts enter through discrete transitions ($\Delta\theta_i = -1$) as a household loses a benefit or absorbs a new cost. Personal loss and community exposure are not independent: I denote by $p(E_i)$, increasing in E_i, the probability a household undergoes $\Delta\theta_i = -1$ given a cut, so residents of high-E_i areas are more likely to lose out personally.

The sociotropic channel captures community welfare. β_i is how much i values it, and E_i is community exposure to the refugee aid economy, which is highest near camps and lower (but non-zero) elsewhere. The community-level payoff is $\sigma A - C(A)$, where $\sigma \in [0, 1]$ is the share of aid reaching host communities and $C(A) = \bar{c} - A$ is the cost of hosting ($\bar{c}$ the maximum, at $A = 0$). The community receives net benefits as long as $\sigma A > C(A)$, with crossover at $A^* = \bar{c}/(\sigma + 1)$: above A^* it still gains from a contracting aid economy, but below it incurs net costs.

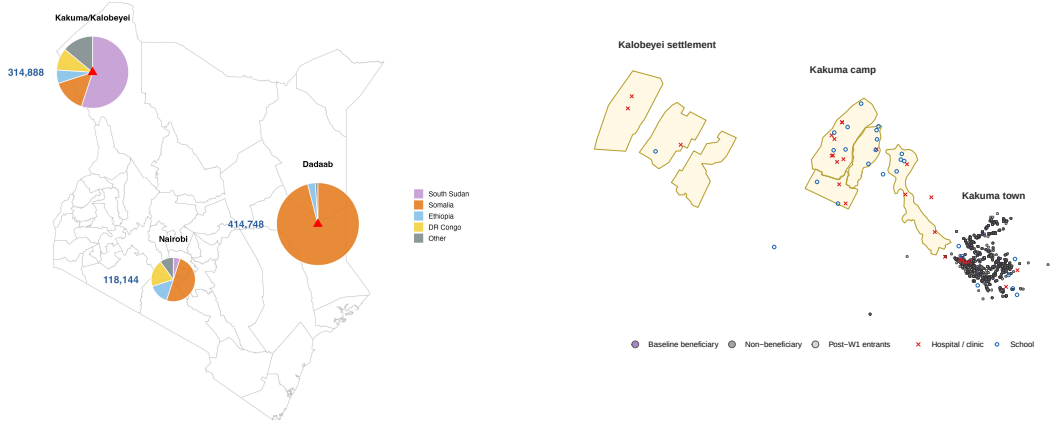
The empathy channel captures the host's response to refugee suffering. γ_i is the weight i places on refugee welfare: $\gamma_i > 0$ indicates empathy, $\gamma_i < 0$ discrimination or hostility. E_i enters again, capturing contact with refugees and awareness of their conditions, and $S(A) = \bar{s} - A$ is the level of refugee suffering ($\bar{s}$ the maximum, at $A = 0$). The additive $\frac{1}{2} S(A)$ term lets even unexposed hosts respond through media coverage and indirect social ties, though less strongly than the exposed.

The formalization's payoff is that community exposure, E_i, enters the sociotropic and empathy channels with opposite signs of response to aid cuts. In the sociotropic channel, an aid cut pulls support down: as A falls, host benefits (σA) shrink and host costs ($C(A)$) rise. In the empathy channel, the same cut pulls support up: as A falls, refugee suffering rises, and empathic concern rises with it. Community exposure amplifies both pulls simultaneously, so the net direction of the response depends on whether $\beta_i(\sigma + 1)$ or γ_i is larger, and on whether the host faces personal losses (θ_i).

2.3.1 Empirical implications

Three sources of variation under declining A generate the testable predictions; full derivations appear in the Appendix. The first compares populations with high vs. low exposure; the second, personal

Figure 1: Refugee-hosting areas and survey sample



(a) Refugee populations in Kenya

(b) Sample around Kakuma, by exposure status

Notes: Panel (a) shows refugee populations by hosting location and origin country. Panel (b) plots survey respondent GPS locations by exposure status (purple = baseline aid recipient or NGO/aid employee; grey = not exposed; light grey = post-W1 entrants to the survey in RCS). Yellow polygons show camp and settlement boundaries.

household losses within a community; the third, hosts who differ in empathy.

Under an aid cut, the expected response in a high-exposure population combines three terms: an egocentric-channel decrement ($-\alpha_i\, p(E_i)$) from the higher rate of personal losses, and a sociotropic decrement and an empathy increment that are both amplified by E_i. The egocentric and sociotropic channels jointly push high-exposure populations toward backlash relative to low-exposure ones; the empathy channel offsets this, and can reverse it where empathy is strong enough (see H3), but in general we expect backlash.

H1 (Community exposure): *Aid cuts reduce hosting support more in high-exposure than in low-exposure populations.*

Within a high-exposure community, two otherwise comparable hosts who differ only in personal loss, one of whom loses a benefit or incurs a cost ($\theta_i : 0 \to -1$), differ in attitude change by exactly $-\alpha_i$.

H2 (Personal loss): *Within an exposed community, hosts who personally lose aid-related benefits or absorb new costs show a steeper decline in hosting support than otherwise-similar neighbors.*

Holding E_i and θ_i fixed, the response to an aid cut moves linearly in γ_i. Two hosts with identical community exposure and personal loss but empathy of opposite sign respond in opposite directions.

H3 (Empathy): *Holding community exposure and personal loss constant, aid cuts move high-empathy hosts ($\gamma_i > 0$) toward more pro-hosting attitudes.*

3 Context

3.1 Refugee hosting in Kenya

Kenya hosts approximately 800,000 refugees, the vast majority from South Sudan and Somalia living in Kakuma and Dadaab camps (see Figure 1a; UNHCR, 2025*a*). Camp-based refugees rely heavily on international assistance due to restrictions on movement outside the camps and limited opportunities for employment (MacDonald and Lichtenheld, 2026).[2]

This study focuses on Kakuma camp and Kalobeyei settlement ("Kakuma") in Turkana, a semi-arid border county that is among Kenya's poorest and most politically marginalized regions. 72% of Turkana residents live in poverty, higher than 68% among refugees and 37% nationwide (World Bank and UNHCR, 2021). Before the camp was established in 1992, the surrounding area was sparsely populated. Refugees, camp infrastructure, and international aid transformed it into an urban center, making the area substantially more developed than the surrounding county (International Finance Corporation, 2018; Alix-Garcia et al., 2018; Sanghi, Onder and Vemuru, 2016; World Bank and UNHCR, 2021).

Hosts in Kakuma benefit from the camp economy through three channels, which I describe using evidence from the panel survey (see Section 4) and statistics from the World Bank (World Bank and UNHCR, 2021). First, hosts access refugee services, particularly health facilities and schools. 69% of Turkana hosts have no formal education (compared to 47% of refugees), and only 40% can read and write (78% refugees). Hosts account for an estimated 10–15% of beneficiaries of camp healthcare services. As one respondent put it, "Since the refugees came, there has been help. Schools and hospitals were built" (Woman, 21).[3] Second, hosts find employment with NGOs and with refugee households, particularly as casual domestic laborers. The Turkana hosts are "asked to fetch water, wash the dishes, do the laundry, and after doing the work [we] get something to go and eat at home with [our] kids" (Woman, 48).[4] Third, hosts run businesses that depend on refugee demand, including shops and the sale of local resources. One respondent described: "Most of [us] take firewood to the camps and sell it to the refugees, and some take charcoal. After that, they are given food [by the refugees] and they go feed their kids" (Woman, 23).[5] In May 2025, a large majority of panel respondents (71%) agreed that locals benefit from the camp.

Kenya is a strong case to test the theory: exposure to refugee assistance is geographically concentrated, and the cuts arrived in identifiable waves with clear timing, allowing us to test variation in community exposure and personal loss. I use "exposure" throughout for the geographic contrast between high- and low-exposure populations; within-Kakuma differences between households that did and did not lose aid-related benefits I call personal or household loss.

3.2 Aid cuts

This subsection describes how aid cuts affected the camp economy, drawing on interviews with refugee leaders and the panel survey (see Section 4). Refugee assistance in Kenya contracted in three stages (see details in Table 1). First, WFP reduced food assistance from 80 percent to 40 percent of minimum

[2] 13 percent of refugees live in urban areas, particularly Nairobi. This group is more self-sufficient and less dependent on aid.

[3] Additional quotes: refugees enabled hosts to "access basic needs like health from the UN" (Man, 31); "[we] get free treatments from the camp hospitals and free schooling for [our] children" (Man, 32).

[4] Additional quote: refugees "boosted the economy by introducing NGOs, creating job opportunities" (Man, 27).

[5] At the level of the broader local economy, hosts describe Kakuma as more developed than the surrounding region: "Kakuma's revenue has increased over time as compared to Lodwar due to a lot of businesses going on about and a lot of people duly paying their taxes" (Woman, 34).

Table 1: Aid shocks and data collection

Date	Event	Description	Captured by	Trajectory
2023–2025	WFP reductions	General food basket cut in stages from 80% to 40% of minimum daily needs: 80→60% Jul 2023; 60→50% Feb 2024; 50→45% May 2024; 45→40% Feb 2025.	Natrep 2023 → 2026 (bundled)	Decline
Jan–Feb 2025	USAID cuts	USAID-funded NGOs reduced services and laid off staff across the camp.	Natrep 2023 → 2026 (bundled)	Decline
Jun 2025	Cash assistance suspended	Cash grants fully suspended; 40→30% food basket for all residents.	W1 → W2; Natrep	Decline
Jul 2025	DA announced	Vulnerability-tiered Differentiated Assistance model announced; sparked protests and delayed food distribution.	W1 → W2; Natrep	Trough
Aug 2025	DA takes effect	Tiered rations begin: Cat 1 at 40%, Cat 2 at 20%, Cat 3–4 receive nothing; cash still suspended; increased police presence.	W2 → W3; Natrep	Partial recovery
Sep–Oct 2025	Cash resumption announced	Resumption of cash for Oct–Nov announced.	W2 → W3; Natrep	Partial recovery
Oct–Nov 2025	Rations increased, cash resumes	Rations raised (Cat 1 55%, Cat 2 35%, Cat 3 20%); cash resumes for Categories 1–3.	W3 → W4; Natrep	Partial recovery

daily needs between July 2023 and February 2025 (Anyadike, 2024).[6] Second, after the U.S. cut foreign assistance worldwide in 2025, rations fell again and organizations reduced staff and services. These cuts affected the main NGOs serving Kakuma, including UNHCR, which funds many other camp organizations, IRC, which delivers healthcare, and LWF, which delivers education.[7]

The third stage, captured by the panel, marked the trough of the cuts. In June, WFP suspended cash transfers, eliminating the $3.09–$3.98 per refugee that had helped cover fresh food, hygiene products, firewood, school fees, and other costs. In July, WFP announced a Differentiated Assistance (DA) model that classified refugees into four vulnerability tiers.[8] When the model was rolled out in August, almost half of registered refugees, those in Categories 3 and 4, received no rations or cash. Refugee leaders reported that many vulnerable households were misclassified as self-sufficient, producing acute hunger and crisis. The announcement triggered protests and delayed food distribution by two weeks, leading to increased police presence. Assistance partially recovered in late 2025, when WFP announced the resumption of cash transfers for most groups and modest ration increases. The recovery was incomplete, however. Cash assistance and rations resumed at lower levels than before, Category 4 refugees still received no assistance, and NGO staffing did not return to prior levels.

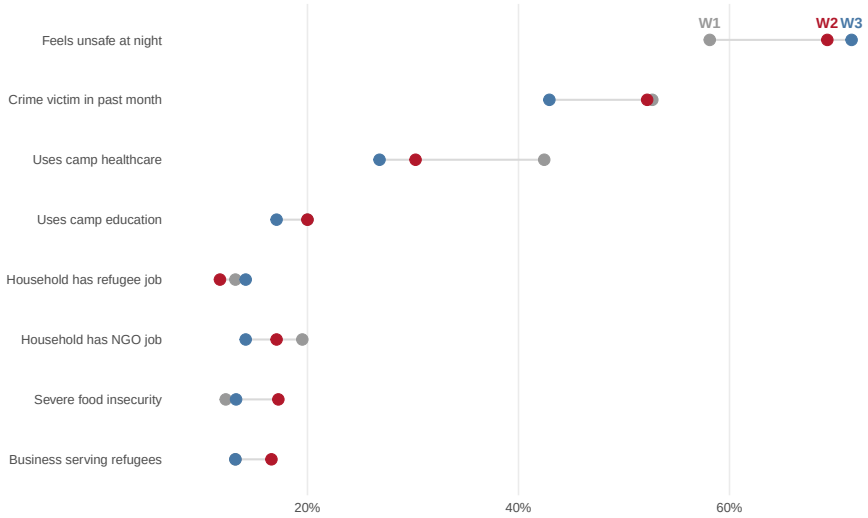
Interviews with refugee leaders provide qualitative data on the harm these cuts caused, particularly in July 2025. The central problem was widespread hunger, with many refugees eating "once per day" (R14, W1). To cope, they skipped meals, took on debt, resorted to crime, and sold their assets. One mother was forced to "sell the roof of the house . . . so that she can get food for her kids" (R19, W3); adolescent girls "sleep with a man to get [sanitary] pads" (R20, W1); and "people have been cut with panga [knives] just because of a smart phone" (R8, W2). Schools and clinic access became more

[6] The United States funded a large share of these flows: in fiscal year 2024, the U.S. provided 47.0 percent of WFP's 2023–2027 contributions in Kenya and 45.8 percent of UNHCR's contributions (WFP, 2025; UNHCR, 2024). Sterck and Bruni (2025) show that a 20 percent cash reduction during this period harmed refugees, though they do not estimate effects on hosts.

[7] In fiscal year 2024, the U.S. disbursed $116.7 million to WFP, $36.6 million to UNHCR, $4.8 million to IRC, $0.7 million to LWF, and $6.0 million to other organizations serving refugees in Kakuma (U.S. Department of State, 2025; OCHA, 2025). Author calculated these statistics using U.S. government data.

[8] For evidence on the impact on refugees in Uganda, see Grossman et al. (2025).

Figure 2: Host livelihood outcomes across the Kakuma panel



Notes: Percentage of Kakuma panel households reporting each outcome at each wave. Services, employment, and crime are asked for the previous month. Severe food insecurity: missed meals on 5–7 days in the past week. Feels unsafe at night: disagrees or strongly disagrees with feeling safe. Rows ordered by W3 prevalence.

strained, the main issue being a lack of medication: "Now we have no drugs in the hospital. They're just giving you painkillers . . . Malaria is the biggest killer" (R5, W1). Refugees in Kakuma cannot move elsewhere in Kenya for work without formal permission, nor grow their own food, because "it's arid, dry, [and] there's no water." Some returned to South Sudan despite the risk of conflict, reasoning that it is "better to die in your country with a bullet than hunger" (R2, W2). By Wave 3, leaders noted, the situation had improved, though not fully: "Comparing August and now, things are much better. August was really really bad. People had no food, there was a lot of tension and theft also. [Now] feel like things are not good all time, but at least, you're not hearing stories of someone starving and rushed to the hospital" (R7, W3).

Host citizens were also affected, through lost employment, services, and the food previously exchanged between refugees and hosts. As one refugee leader put it, "The situation is becoming worse on both sides. Refugees used to buy charcoal from [the Turkana]. Now that you don't have money, how will you buy that charcoal" (R4, W2); a host echoed, "Life in the host community has started to become difficult, there is no food or work" (Male, 72). Figure 2 summarizes the household-level trajectory across waves.[9]

The clearest shift was a sharp rise in feeling unsafe at night ($p < 0.001$), reflecting insecurity from the protests, rising crime and theft, and conflict as refugees collect charcoal and firewood they previously bought from the Turkana. There was also a sustained decline in camp healthcare access ($p = 0.002$), with smaller, statistically insignificant decreases in education access. On economic outcomes, hosts reported lost income, increased food insecurity, lost NGO employment, and lost businesses serving refugees, though these shifts are not statistically significant. The W2 to W3 recovery was muted at the household level: of the panel households that lost access to camp-area education, healthcare, refugee employment, or NGO employment between W1 and W2, only 8% had regained access in that domain by W3. The main improvements were instead in reduced crime victimization ($p = 0.049$) and food insecurity. Hosts were nonetheless still accessing services and employment at W3, indicating that the

[9] Tests are linear probability models of each outcome on wave indicators, with standard errors clustered by respondent; see Appendix. With roughly 390 households per wave, the panel is underpowered to detect modest changes in low-prevalence outcomes, so null results should be read as imprecise.

aid cut was partial.

4 Research Design

4.1 Data sources

I draw on three data sources to test the hypotheses derived from the theory. First, I fielded two original nationally representative phone surveys. The baseline was conducted in September–October 2023, with 3,326 respondents: a nationally representative sample of 2,432 and oversamples near Dadaab and Kakuma camps (441 in Garissa and 453 in Turkana) (MacDonald and Lichtenheld, 2026).[10] The follow-up was fielded in February 2026, after the major 2025 cuts, with a nationally representative sample of 2,042 and a Turkana oversample of 289.[11] In both surveys, I construct post-stratification weights using entropy balancing, matching each sample to the 2023 Afrobarometer distribution of age, gender, education, and region (Hainmueller, 2012). The two waves include 2025 aid cuts as well as earlier WFP reductions, beginning in 2023 (Table 1).

Second, I fielded three waves of a face-to-face panel survey with citizens living next to Kakuma camp.[12] Although designed before the trajectory of aid was known, the panel tracks citizens through the main aid cut shock, from pre-trough conditions in May to the trough in July and early partial recovery in September. The main analysis uses 205 respondents present in all three waves; the appendix reports parallel results using the repeated cross-section, refreshed to at least 300 citizens per wave. A fourth phone wave, fielded alongside the 2026 nationwide survey, captures longer-term change but has higher attrition and is reported in the appendix. Figure 1b maps the camp, Kakuma town, panel respondents, and schools and health facilities.

Third, I conducted 52 semi-structured interviews with a panel of refugee leaders alongside the survey waves. The interviews provide qualitative evidence on refugees' suffering, host-community spillovers, and the evolution of the cuts between waves. Seventeen leaders remained in the panel across all three waves (18 in Wave 1 and 17 in Waves 2 and 3).[13] Interviews lasted 30–60 minutes and covered the cuts, coping strategies, relations with the local Turkana community, and blame attribution. This evidence is informed by previous in-person interviews in Kakuma in 2022 and multiple years of fieldwork in Kenya.

4.2 Outcome measures

I analyze three measures of hosting attitudes. *Nation Hosting* and *Local Hosting* are five-point scales measuring support for hosting refugees in Kenya and in the respondent's district, respectively. *Not Repat.* captures whether the respondent prefers a policy option other than the harshest option of sending refugees back to their origin countries. In the nationwide survey, it equals one if the re-

[10] Because the 2023 survey experimentally varied whether respondents were asked about "refugees" in general or specific refugee groups (for a separate paper), over-time comparisons with the 2026 survey use only the generic-"refugees" arm for the hosting items ($N = 810$).

[11] A May 2026 follow-up corrected a technical issue in the nationwide hosting items only ($N = 1{,}776$ nationally; no oversample).

[12] I fielded the panel in Kakuma rather than Dadaab because insecurity in Garissa prevented in-person survey work. TIFA Research recruited 300 Kakuma residents using a multi-stage design: sub-locations were selected with probability proportional to household counts, households were randomly selected within sub-location, and an eligible adult was randomly selected within household. Protests in July/August disrupted survey administration and increased attrition. See Appendix for details.

[13] Leaders were purposively sampled to vary by nationality, gender, and leadership role, including healthcare workers, local news broadcasters, leaders of refugee-led organizations, and employees of international organizations. Respondents were recruited with the help of a local journalist and research assistant with strong networks in Kakuma. Interviews were conducted by phone because UNHCR restrictions prevented in-person research in Kakuma during the cuts. Details are in the appendix.

spondent does not select repatriation from five refugee-policy options: full rights, settlements, camps, repatriation, or resettlement. In the panel, the equivalent is *Oppose Camp Closure*, which is support for "The Kenyan government closing Kakuma camp and moving the refugees in Kakuma back to their countries," measured on a five-point scale and reverse-coded so that higher values indicate opposition to closure.

For the nationwide analysis, the main outcome is the *Hosting Index*, an inverse-covariance-weighted index (Anderson, 2008) combining the three items above. The index is standardized to the 2023 national baseline (mean 0, SD 1), so coefficients represent standard-deviation changes relative to pre-cut attitudes. For the panel analyses, I focus on the two local measures rather than the index (*Local Hosting* and *Oppose Camp Closure*).

I estimate all regressions by OLS, using a linear probability model for binary outcomes. Standard errors are HC1 for cross-sectional specifications and clustered by respondent for panel specifications. Demographic controls adjust for compositional differences between samples where relevant.

4.3 Empirical strategy

This section lays out the estimation strategy for each hypothesis. I give the regression equations for the main specifications and describe variants and supporting designs in text. Full equations appear in the Appendix.

4.3.1 H1: Community exposure

Nationwide difference-in-differences. I estimate a pooled difference-in-differences comparing pre-cut (2023) and post-cut (2026) attitudes in the main refugee-hosting counties, Turkana and Garissa, with the rest of the country:

$$Y_i = \alpha + \beta\,\mathrm{Post}_i + \rho\,\mathrm{Host}_i + \tau\,(\mathrm{Post}_i \times \mathrm{Host}_i) + X_i'\Gamma + \varepsilon_i, \tag{2}$$

where Host_i indicates a refugee-hosting county and X_i controls for age, gender, and education. The coefficient β captures the adjusted change outside hosting counties, where community exposure is lower, while τ captures the differential change in hosting counties. H1 predicts $\tau < 0$ on the *Hosting Index* unless empathy offsets the economic effects of exposure. I interpret the estimates descriptively because the two waves span multiple years, meaning that we cannot isolate the aid cuts, and selection into hosting counties is not random.

Panel over-time change. The Kakuma panel traces within-person attitudes as aid contracts (W1→W2) and partially recovers (W2→W3). I estimate an event-study specification on the balanced panel,

$$Y_{it} = \mu + \nu_i + \delta_2 D_{2t} + \delta_3 D_{3t} + \varepsilon_{it}, \tag{3}$$

with respondent fixed effects ν_i, wave indicators D_{2t} and D_{3t} (W1 omitted), and standard errors clustered on the respondent. The coefficient δ_2 is the cumulative change from baseline to the trough of the contraction; the linear contrast $\delta_3 - \delta_2$ measures the change from the trough to the partial-recovery period. H1 predicts $\delta_2 < 0$ for *Local Hosting* and *Oppose Camp Closure*. Although the over-time design does not permit a formal causal test, the high-frequency nature of the panel gives confidence that attitudinal shifts most likely reflect the specific month-on-month aid changes.

Vignette experiment. A factorial vignette experiment embedded in W3 manipulates specific aid conditions. Each respondent rated six hypothetical hosting scenarios on *Nation Hosting*, drawn from a $3 \times 2 \times 2 \times 2$ design. Three attributes capture aid conditions: the aggregate level of international aid (*Aid*: 100, 40, or 0 percent of pre-cut levels), whether NGOs hire local residents (*Jobs*), and whether

camp services are shared with the host community or restricted to refugees (*Access*). The fourth attribute, *Conflict*, tests H3 and is described below. I estimate average marginal component effects with respondent fixed effects and standard errors clustered by respondent. H1 predicts that reducing each community benefit—aid levels, local hiring, and shared services—will reduce hosting support. These effects are causal under random assignment, which holds by design.

Hypothetical full-cut scenario. Embedded in W1 and W2, a stated-preference item asked respondents how strongly they would support Kenya continuing to host refugees if donors completely withdrew financial support, a more severe cut than the one observed. I use this item in three ways. First, the level of support gauges attitudes toward hosting under a hypothetical full withdrawal. Second, I compare each respondent's support under the full cut against their support for hosting refugees in Kenya generally, stacking the two items and adding a scenario-by-wave interaction with respondent fixed effects. Third, comparing responses across W1 and W2, with standard errors clustered by respondent, assesses whether support fell as the cuts worsened and it became clearer what the reality of life without aid would be like. These estimates are descriptive.

4.3.2 H2: Personal loss

H1 establishes whether and where backlash occurs; H2 asks who within the hosting region drives it and why. H2 predicts that hosts who personally lose aid-related benefits or absorb new aid-related costs ($\theta_i : 0 \to -1$) show an additional decline in hosting support. I exploit panel variation in household-level loss across four domains—education, healthcare, refugee employment, and NGO employment—treating a household as having lost access in a domain at wave t if it reported access at an earlier wave and none at t ($\Delta\theta_i = -1$). The outcome is *Oppose Camp Closure*. I report three specifications that trade identification against power, all pointing the same way. Selection into loss is non-random in every case, so I read all three as quasi-causal evidence on the egocentric channel rather than as definitive estimates of α_i. To complement this quasi-causal evidence, in W3, I ask respondents an open-ended question about why they support or oppose closing the camp.

Baseline beneficiaries (ITT). I interact a fixed Wave 1 beneficiary indicator, whether the household received aid or had an NGO/aid employee at baseline, with wave dummies in the balanced panel with respondent fixed effects:

$$Y_{it} = \mu + \nu_i + \delta_2 D_{2t} + \delta_3 D_{3t} + \tau_2, (B_i \times D_{2t}) + \tau_3, (B_i \times D_{3t}) + \varepsilon_{it}, \tag{4}$$

where B_i is baseline beneficiary and τ_2 and τ_3 capture differential change among exposed households at the trough and partial-recovery waves. Because beneficiary is measured before the severe cuts, it cannot be caused by later attitudes; the specification therefore estimates an intent-to-treat effect of pre-shock structural vulnerability to loss in an aid cut. The estimate is likely attenuated because not all baseline-beneficiary households lost benefits. It is descriptive rather than causal: baseline-beneficiary households were more pro-refugee before the cuts, and exposure is not randomly assigned.

Cohort difference-in-differences. I compare households that first lost access between W2 and W3 ($A_i = 1$) against households that never lost access across the three waves ($A_i = 0$):

$$Y_{it} = \mu + \nu_i + \delta_2 D_{2t} + \delta_3 D_{3t} + \tau_2 \left(A_i \times D_{2t}\right) + \tau_3 \left(A_i \times D_{3t}\right) + \varepsilon_{it}, \tag{5}$$

estimated on the no-loss ($N = 75$) and new-loss ($N = 51$) households with respondent fixed effects and standard errors clustered on the respondent. I examine the W3 transition rather than the W2 one because it makes W1–W2 available as a pre-trend test: τ_2 is a placebo coefficient that should be near zero if the two groups were on parallel trajectories before the W3 shock, while τ_3 is the test of H2, the additional within-person change among new-loss households relative to controls.

Switching difference-in-differences. To gain power and estimate effects by domain, I pool all loss and gain events across waves, regressing the camp-closure item on an indicator that switches to one once a household has lost access in a domain, with respondent and wave fixed effects and standard errors clustered on the respondent. The coefficient is the average within-person change in support upon losing access, in SD units. It uses all loss events and yields domain-specific estimates, at the cost of assuming loss effects are constant across cohorts of losers.

4.3.3 H3: Empathy

H3 predicts that hosts who differ in empathy respond differently to the same aid cut. The W3 vignette experiment tests two patterns this implies: hosts should respond to refugee suffering itself (a precondition for empathy to matter on average), and their response to aid should vary with baseline empathy.

Vignette experiment. The vignette experiment described under H2 also includes a *Conflict* attribute, which varies whether conflict in refugees' home countries is ongoing or has ended. Ending the conflict removes the source of refugee suffering independent of aid, so the *Conflict* effect isolates the empathy channel. If respondents are empathetic, their support for refugee hosting should be greater when conflict is ongoing in the refugees' home country. Comparing its magnitude to the *Aid* effect shows whether hosts respond more to refugee suffering or to the aid environment. Both effects are causal under random assignment.

Empathy heterogeneity. I measure each host's empathy with an index averaging two five-point items (concern about the refugees and their suffering, and worry that refugees lack enough food because of the cuts), standardized to mean zero and unit variance in the W3 sample. I add an *Aid* $\times$ *empathy* interaction to a regression of the vignette rating on *Aid* and *Conflict*, with respondent fixed effects and standard errors clustered on the respondent. H3 predicts a positive interaction: high-empathy respondents have a less negative aid slope than low-empathy ones.[14] The interaction is a heterogeneous treatment effect: aid is randomized but empathy is observed rather than assigned, so it recovers the slope of the aid response in empathy, not the causal effect of changing empathy. A further limitation is that empathy is measured at W3, after the cuts, so exposure may itself have moved it; I therefore treat the index as a proxy for baseline empathy and read the interaction as suggestive rather than definitive evidence on the empathy channel.

Open-ended responses. Two open-ended instruments gauge the salience of humanitarian concern. In the Kakuma panel, respondents were asked what changes they had seen from the cuts. Nationwide, the 2023 survey asked respondents why they support or oppose hosting. Both are coded into categories distinguishing humanitarian, economic, security, and reciprocity-based reasons; see appendix for details.

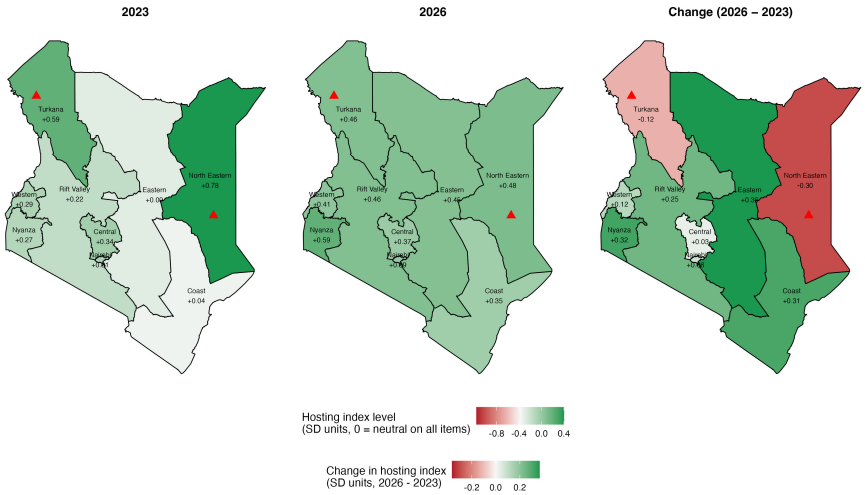
5 Results

5.1 H1: Community exposure

H1 predicts that under aid cuts, hosts in high-exposure populations move toward less pro-hosting attitudes than hosts in low-exposure populations, unless empathy offsets the egocentric and sociotropic losses.

[14] The linear-in-Aid coding parameterizes the pre-registered AMCE specification to deliver a single interaction coefficient testing H3's monotonic prediction; the appendix reports the indicator-form AMCE interaction as the pre-specified robustness check, along with the same specification estimated separately on each empathy item.

Figure 3: Hosting Index across Kenya, 2023–2026



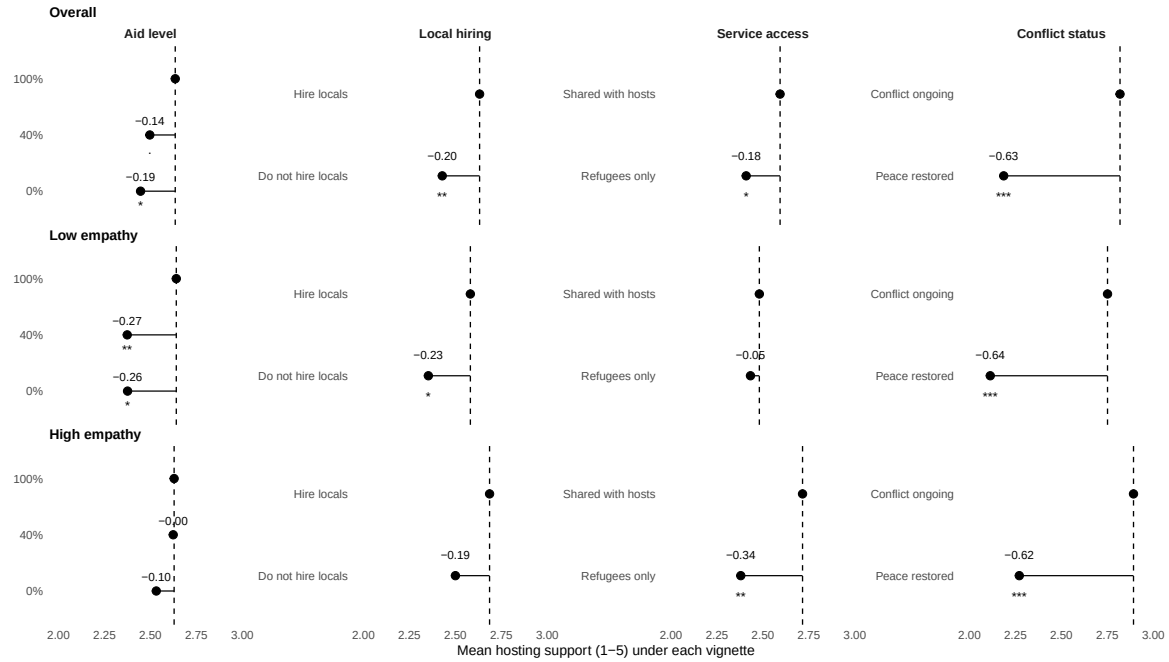
Notes: Weighted mean of the Hosting Index by region. The index is an Anderson (2008) inverse-covariance-weighted combination of *Nation Hosting*, *Local Hosting*, and *Not Repat.*. The index is standardized on the 2023 nationally representative baseline (mean 0, SD 1); higher values indicate more pro-refugee attitudes. Provinces are shown except for Turkana and Garissa, the refugee-hosting counties, which are broken out separately. Both surveys use entropy-balancing post-stratification weights matched to the 2023 Afrobarometer distribution on age, gender, education, and region. The 2023 hosting items are restricted to the generic-refugees framing arm. Red triangles mark Kakuma and Dadaab refugee camps. Numbers show the weighted mean in each region.

Table 2: Pooled difference-in-differences: nationwide refugee attitudes, 2023–2026

	Hosting Index	Nation Hosting	Local Hosting	Not Repat.
Post	0.114**	-0.027	0.099	0.079***
	(0.040)	(0.066)	(0.072)	(0.019)
Hosting county	0.326***	0.562***	0.381**	0.109***
	(0.063)	(0.095)	(0.120)	(0.028)
Post × Hosting county	-0.365***	-0.418*	-0.659***	-0.099**
	(0.081)	(0.196)	(0.155)	(0.036)
Controls	Yes	Yes	Yes	Yes
Observations	3,401	2,841	3,381	3,357
R^2	0.040	0.023	0.034	0.033

Notes: OLS with HC1 standard errors. Post coefficient gives nationwide change outside hosting counties; Post × Hosting county gives differential change in Turkana and Garissa. All outcomes coded so higher = more pro-refugee. Demographic controls: age, gender, education. 2023 sample restricted to generic-refugees arm. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, † $p < 0.10$.

Figure 4: Vignette experiment: hosting support by aid conditions and baseline empathy



Notes: AMCEs from Wave 3 factorial vignette (Kakuma, $N = 301$ respondents, 1,885 profiles). Points show mean *Nation Hosting* at each attribute level; dashed line marks reference. Linear model with respondent fixed effects, SEs clustered by respondent. First panel full sample; second and third split at the sample median of the empathy index. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, † $p < 0.10$.

Figure 3 summarizes nationwide shifts in the hosting index between 2023 and 2026. In line with H1, most of the country did not backlash and moved modestly in a pro-refugee direction, while the two refugee-hosting regions moved against. Table 2 confirms the pattern in the DiD. The H1 test, $\hat{\tau}$, is -0.365 on the Hosting Index ($p < 0.001$): hosts in the refugee-hosting counties moved against refugees relative to the rest of the country by roughly a third of a standard deviation. The decline appears across all three components: it is largest for local hosting ($\hat{\tau} = -0.659$, $p < 0.001$) but also significant on nationwide hosting ($\hat{\tau} = -0.418$, $p < 0.05$) and not selecting repatriation as preferred policy ($\hat{\tau} = -0.099$, $p < 0.01$).

Because hosting regions began above the national average ($\hat{\rho} = 0.326$, $p < 0.001$), the shift represents convergence toward the national mean from a higher baseline rather than hosting regions becoming more hostile than the rest of the country. The coefficient for the non-hosting regions $\hat{\beta} = 0.114$ ($p < 0.01$) aligns with the prediction that where E_i is low and empathy is high, there is no backlash and even a slight rise in support, though the increase is substantively small and largely driven by the repatriation-choice measure.

The Kakuma panel traces within-person attitudes as aid contracted (W1 to W2) and partially recovered (W2 to W3). Table 3 (Panel A) shows a decline in opposition to camp closure from baseline to the trough ($\hat{\delta}_2 = -0.260$, $p < 0.10$), consistent with backlash. Support for local hosting also declines, but the estimate is small and not distinguishable from zero ($\hat{\delta}_2 = -0.010$, n.s.). Neither measure changes significantly between W2 and W3. Elevated support for closure therefore persisted despite the partial recovery, consistent with household losses largely remaining through the panel period.

Figure 4 reports the AMCEs for the vignette experiment (regression table in appendix). All three

reductions in aid for hosts move attitudes in the predicted direction: cutting aid to zero lowers hosting support by 0.19 points ($p < 0.05$), removing local hiring by 0.20 ($p < 0.01$), and restricting services to refugees by 0.18 ($p < 0.05$); reducing aid to 40 percent produces a smaller decline of 0.14 that does not reach conventional significance ($p < 0.10$).

Examining attitudes towards a full-withdrawal scenario (regression table in appendix), support was 0.47 points lower than support for hosting refugees in Kenya generally in W1 ($p < 0.01$), and this gap widened by a further 0.37 points by W2 ($p < 0.10$). Support for hosting under full withdrawal also fell sharply across waves, from 2.64 to 2.12 on the five-point scale ($\hat{\beta} = -0.545$, $p < 0.01$), while the share expressing support fell from 42.1 to 29.4 percent ($p < 0.01$). As cuts deepened and the prospect of hosting without aid became more concrete, opposition to this scenario increased. Yet support did not collapse: even under hypothetical full withdrawal, 29 percent still supported hosting, suggesting that attitudes do not rest on aid alone.

5.2 H2: Personal loss

H2 predicts that within the high-exposure community, hosts who personally lose aid-related benefits or absorb new costs (a discrete transition $\theta_i : 0 \to -1$) show an additional decline in hosting support of magnitude α_i relative to otherwise-similar neighbors who share the same community exposure but face no direct household losses. The within-respondent difference-in-differences shows that the muted panel-wide trend masks heterogeneity across households.

Panel B of Table 3 is the primary specification, and it shows that baseline-beneficiary households (who were aid recipients or employees in the camp at W1) moved toward more pro-closure attitudes relative to the non-beneficiaries, both at the trough ($\hat{\tau}_2 = -0.711$, $p < 0.05$) and at W3 ($\hat{\tau}_3 = -0.712$, $p < 0.05$) on the camp-closure outcome, with a parallel decline on local hosting by W3 ($\hat{\tau}_3 = -0.676$, $p < 0.10$). The cohort DiD in Panel C corroborates the result on camp closure, with new-loss households shifting by $\hat{\tau}_3 = -0.840$ ($p < 0.05$) relative to never-lost households. The switching DiD in Panel D, which pools all loss events and estimates effects by domain, gives an any-domain estimate of -0.606 ($p < 0.05$); the signs are consistent across all four domains, with significant declines for healthcare (-0.790, $p < 0.01$), refugee employment (-0.970, $p < 0.05$), and NGO employment (-0.629, $p < 0.05$), and a smaller, non-significant estimate for education (-0.311, n.s.).

Examining the open-ended questions on closing the camps in W3 (Figure 5), we see that the major reason to support the camps is the economic benefits they bring, consistent with their removal driving backlash. Among the majority who would keep the camps open, the modal rationale is economic benefit (63.8 percent of coded responses), followed by humanitarian reasons (22.7 percent). Among the minority who would close the camps, the modal rationale is not economic cost but insecurity (58.1 percent).

Two characteristics of the loss population are important to note. First, loss is broad rather than rare: 79 of the 205 panel households (38.5 percent) lost access in at least one of the four domains between W1 and W2, and 124 (60.5 percent) are in a lost state at W3 as losses accumulate. Second, loss and no-loss households differ at baseline: respondents whose households later absorbed losses begin at somewhat higher levels of pro-refugee support, consistent with the most connected residents having derived the largest pre-cut benefits from the camp economy. Respondent fixed effects absorb this baseline gap, so the estimates compare within-person change rather than baseline differences.

5.3 H3: Empathy

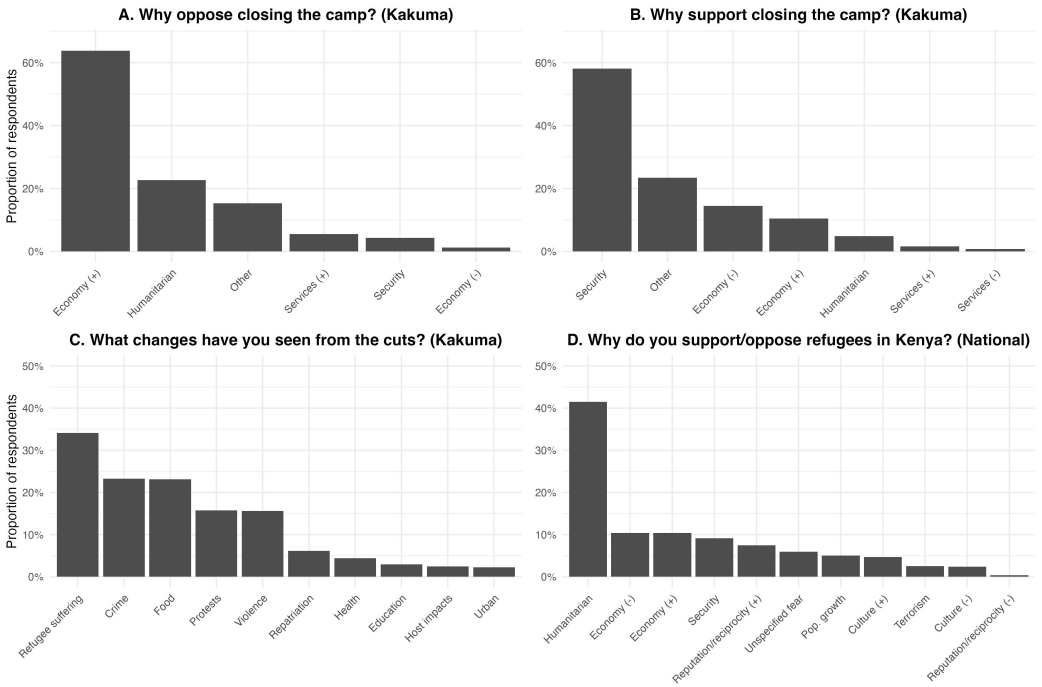
H3 predicts that when hosts have high empathy ($\gamma_i > 0$), they are less likely to backlash in response to aid cuts, and may even become more supportive.

Table 3: Within-Kakuma hosting attitudes over time and by household loss

	Oppose Camp Closure	Local Hosting	Nation Hosting
Panel A: Over-time change (event study)			
Wave 2 vs Wave 1	-0.260†	-0.010	-0.166
	(0.151)	(0.128)	(0.136)
Wave 3 vs Wave 2	-0.039	-0.059	0.441**
	(0.147)	(0.167)	(0.135)
Respondent-wave obs.	612	613	614
Respondents	204	205	205
Panel B: Intent-to-treat, baseline beneficiary status			
Baseline beneficiary × Wave 2	-0.711*	0.067	-0.102
	(0.311)	(0.269)	(0.289)
Baseline beneficiary × Wave 3	-0.712*	-0.676†	-0.603†
	(0.336)	(0.352)	(0.335)
Respondent-wave obs.	612	613	614
Respondents	204	205	205
Panel C: Cohort DiD, newly lost W2–W3 vs never lost			
Newly lost W2→W3 × Wave 2 (placebo)	-0.493	-0.165	0.171
	(0.389)	(0.342)	(0.357)
Newly lost W2→W3 × Wave 3	-0.840*	-0.577	-0.562
	(0.398)	(0.466)	(0.425)
Respondent-wave obs.	375	376	377
Respondents	125	126	126
Panel D: Switching DiD by loss domain			
Any domain	-0.606*	-0.325	-0.583**
	(0.240)	(0.238)	(0.222)
Education	-0.311	-0.030	-0.207
	(0.342)	(0.290)	(0.307)
Healthcare	-0.790**	-0.024	-0.357
	(0.272)	(0.257)	(0.224)
Refugee employment	-0.970*	-0.372	-0.655
	(0.440)	(0.383)	(0.399)
NGO employment	-0.629*	-0.494†	-0.321
	(0.262)	(0.293)	(0.292)
Respondent-wave obs.	612	613	614
Respondents	204	205	205

Notes: OLS with respondent fixed effects, standard errors clustered by respondent. Higher values indicate more pro-refugee attitudes. Panel A: event-study coefficients (W2 vs. W1; W3 vs. W2). Panel B: ITT by baseline beneficiary status ($N = 127$ beneficiary, $N = 78$ non-beneficiary). Panel C: cohort DiD comparing new-loss ($N = 51$) vs. never-loss ($N = 75$) households; τ_2 tests parallel pre-trends. Panel D: pooled loss-event analysis by domain. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, † $p < 0.10$.

Figure 5: Open-ended responses on camp closure, the cuts (Kakuma), and reasons for hosting refugees (national)



Notes: Share of respondents mentioning each theme (multiple mentions allowed). Panels A–B: W3 Kakuma respondents supporting ($N = 124$) and opposing ($N = 163$) camp closure. Panel C: pooled W1–W3 Kakuma ($N = 615$ respondent-waves). Panel D: 2023 national sample, control arm. See appendix for codebook.

The vignette experiment examines this channel in two ways. First, the *Conflict* contrast tests whether support is higher when refugees continue to need asylum and protection. Figure 4 shows that ongoing origin-country conflict raises hosting support by 0.63 points on the five-point scale relative to restored peace ($p < 0.001$). This is the largest effect in the experiment, exceeding each of the aid contrasts. The result suggests that respondents place weight on refugees' need for protection, independent of the aid-related benefits they receive.

Second, the heterogeneity test interacts each aid-related attribute with baseline empathy, estimating responses separately for low- and high-empathy respondents. Figure 4 shows the clearest evidence for H3 on the aid-level contrast: high-empathy respondents do not differ significantly across aid levels (the 40 percent contrast is essentially zero, −0.00, and the 0 percent contrast small and non-significant, −0.10), whereas low-empathy respondents reduce support under both the 40 percent (−0.27) and 0 percent (−0.26) scenarios. The split is similar but weaker for local hiring, where only low-empathy respondents reduce support when NGOs do not hire locals (−0.23 vs. a smaller, non-significant high-empathy estimate), and reverses for service access, where the high-empathy group reduces support when services are restricted to refugees (−0.34) but the low-empathy group does not (−0.05). The empathy moderation is therefore concentrated in the aid percentage dimension, rather than holding uniformly across the vignette.

Two open-ended items provide complementary descriptive evidence that γ_i is high on average, in both samples. In Kakuma, when respondents are asked what changes they have seen from the cuts, refugee suffering is the most commonly mentioned category, cited by 34.1 percent of respondent-waves, against 23.3 percent for crime, 23.1 percent for food, and just 2.4 percent for impacts on the host community

itself (Figure 5). Hosts describe the cuts primarily by reference to what is happening to refugees, not themselves. The same pattern appears nationwide. When respondents were asked why they support or oppose hosting in 2023, over 40 percent cited humanitarian reasons, more than any other category, including economic considerations on either side (Figure 5).

Finally, the empathy channel is also visible in how the cuts shift attitudes for different levels of hosting. Despite the decline in support for local hosting in Kakuma across the panel, support for Kenya hosting refugees more broadly does not fall: Table 3 shows that nationwide hosting support holds steady and in fact rises with the partial recovery, and the same pattern appears for several nationwide refugee-rights measures, which increase at Wave 3 (see appendix). The backlash that aid cuts produce is therefore specific to local hosting rather than to the principle of hosting refugees in Kenya at all, consistent with the open-ended evidence that respondents retain support for refugees' broader protection.

6 Robustness

I conduct a series of robustness checks to improve confidence in the results; full results and summary statistics are in the appendix. Because only the vignette experiment relies on random assignment, I read the other empirical components as observational. The empirical support for the theory instead rests on triangulation across designs fielded at different levels, with different weaknesses, that converge on the same pattern. The main results are robust to the checks below.

I conduct a series of robustness checks that apply across the paper. I examine sample balance across the experiment's treatment arms, compare the two nationwide samples to confirm they are nationally representative and similar to each other, add enumerator fixed effects, and assess attrition.[15] Social desirability is unlikely to drive the results, since the quantities of interest are over-time changes rather than levels, so desirability pressures present at every wave difference out. The appendix also reports integration outcomes—support for refugees' healthcare access, right to work, and freedom of movement—for both the nationwide 2026 cross-section and the panel.

For the nationwide difference-in-differences, a concern is that the change reflects something other than the aid cuts, since the waves span three years. The biggest concern is the Kenyan government's Shirika Plan, a new refugee policy announced between waves; I field a series of questions on the Plan and show it likely does not account for the shift, as nine in ten respondents in 2026 were unaware of it. Because only two counties are treated, conventional clustered standard errors are unreliable, so I use a wild cluster bootstrap (Cameron, Gelbach and Miller, 2008). Results also hold dropping the post-stratification weights, replacing the inverse-covariance-weighted index with a simple average, recoding the five-point items as binary, and estimating ordered logit in place of OLS. The appendix further reports nationwide change in other outcomes, including a measure of trust in refugees and an Afrobarometer item fielded in 2024 and 2026.

For the panel, given the small sample, I report minimum detectable effects. The panel also captures only short-run responses, spanning five months. Wave 4, fielded by phone alongside the 2026 nationwide survey, extends the window but carries higher attrition and a change in survey mode, so I read it as suggestive of longer-run trajectories. The appendix provides further detail on the three personal-loss measures. Finally, for the experiment, I repeat the analysis with and without a manipulation check.

[15] In the panel, Wave 2 fieldwork coincided with the protests, so departing households may have been those most affected. Attriters do not differ from panelists on age, gender, education, or baseline hosting support, and attrition does not vary by baseline beneficiary status; if anything, attrition of the most affected households biases the personal-loss estimates downwards.

7 Conclusion

In 2025, foreign aid contracted at the fastest rate on record. This paper asks whether host citizens exposed to refugee aid cuts withdrew support for hosting as a consequence. Drawing on national surveys, a high-frequency panel, a vignette experiment, and refugee-leader interviews, I find localized but not nationwide backlash. Support fell in refugee-hosting regions while holding steady elsewhere, producing convergence toward the national mean rather than a broad collapse. Within the hosting region, the decline was concentrated among households that personally lost aid-related benefits. Humanitarian concern limited the spread of backlash: high-empathy hosts did not withdraw support even under severe cuts in the experiment.

The findings point to a more general claim about exposure and refugee attitudes. Existing research often treats exposure either as a source of economic threat or benefit, or as a source of contact. This paper brings these channels into the same framework and shows how they condition responses to aid cuts. In aid-dependent hosting areas, exposure makes citizens vulnerable to the loss of jobs, services, and other benefits tied to the refugee-aid economy. But it also makes refugee suffering more visible, allowing citizens to observe the effects of ration cuts and acute hunger among people who often cannot legally work, move freely, or access state services. A single shock that harms hosts and refugees at once can therefore move attitudes in opposing directions. Personal, egocentric losses push some hosts toward opposition, while humanitarian concern pulls in the opposite direction. The net effect depends on exposure, personal loss, and empathy.

The empirical evidence in this study comes from a single context, raising a question of external validity. The theory, however, applies more broadly, and specifies where the dynamics documented here should most likely recur. First, backlash should be strongest where refugees are tied to a visible local aid economy. This is most likely in camp contexts, where assistance is geographically concentrated, locals receive benefits, and hosts and refugees interact. The dynamics should be weaker where refugees are dispersed, mostly urban, or not clearly associated with aid benefits. In such settings, aid cuts may still harm refugees, but they are less likely to impose visible losses on host citizens or alter the perceived returns to hosting, weakening the egocentric and sociotropic channels; empathy may also be weaker where refugee suffering is less visible. Second, the effect of aid cuts depends on prior politicization and hostility. Kenya was not a setting in which refugees were already the object of widespread negative sentiment. In the model, this means the empathy parameter remained positive for many citizens, allowing refugee suffering to offset some of the material costs of hosting. Where refugees are already politicized, aid cuts may produce broader backlash even among citizens who are not personally harmed.

There are several additional limitations with this study. The short time horizon of the panel means that I cannot assess the long-run effects of the aid cuts. The fourth wave extends the window but was fielded by phone and had higher attrition, so it is only suggestive. The panel sample is also small, limiting precision, especially for subgroup and personal-loss estimates. On identification, selection into personal loss is non-random: the households that lost benefits were more connected to the aid economy and more supportive of refugees at baseline. Respondent fixed effects absorb this baseline gap, but cannot rule out that these households were already on a different trajectory for reasons other than the loss itself. Apart from the vignette experiment, the evidence is therefore descriptive or quasi-experimental.

A concern about the broader implications of this study is that localized backlash may have limited political significance if refugee policy is made nationally and public opinion outside hosting regions remains stable. The evidence in this paper cannot establish how policymakers respond to aid cuts, making this a central question for future research. Yet local attitudes still matter. Refugee hosting is implemented in specific places, and opposition from local citizens can generate tension, conflict, and

domestic political pressure that ultimately shape refugee policy. There are antecedents in Kenya: in the 1990s, the government closed a set of coastal camps partly in response to conflict and violence between refugees and citizens (MacDonald, 2026). The absence of nationwide backlash therefore should not be read as evidence that aid cuts are politically inconsequential.

The central takeaway of this study is that aid cuts have political consequences beyond their direct welfare effects. They do not only reduce food, healthcare, education, and employment for refugees; they can also weaken the local support that makes refugee hosting politically sustainable in low- and middle-income countries, where most refugees live and where governments often rely on donor financing to maintain asylum. The backlash documented here remained modest and local, in part because some aid continued and material benefits remained, humanitarian concern was widespread, and the cuts did not become a highly politicized national issue. These conditions may not persist under sustained aid reductions. Continued cuts may further reduce the benefits hosts receive and their empathy for refugees, raising pressure for them to return to unsafe origin countries or move onward. Understanding how host populations respond as the aid economy contracts is increasingly important for explaining the future politics of refugee hosting.

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