

When Does Aid Reduce Conflict? The Role of Inclusive Refugee Policy in the Global South

Zhejun Qiu *

Abstract

Foreign aid is often used to stabilize refugee-hosting communities, yet its effects on civil conflict are mixed: sometimes reducing violence, sometimes amplifying it. I argue that this variation reflects host governments' refugee policies, especially whether refugees have economic rights to work, move, and own property. Where these rights are protected, aid generates spillovers that ease distributional tensions and raise the opportunity costs of violence. Under restrictive policies, aid heightens competition, grievances, and recruitment risks. I test this argument using subnational data and a shift-share instrumental-variable design with high-dimensional fixed effects to address aid-allocation endogeneity and selection concerns in policy adoption and refugee hosting. The conflict-mitigating effect of aid emerges only under economically inclusive refugee policies, consistent with the theory. Country-level evidence on refugee-related violence and individual-level survey data provide additional support. These findings show that refugees are not uniformly destabilizing and that economic inclusion and security can be mutually reinforcing.

Key Words: Foreign Aid, Refugee Policy, Civil Conflict, Displacement, Global South

Word Count: 10,973

*I am grateful to Burcu Savun, Jude Hays, William Spaniel, Alex Braithwaite, Rebecca Cordell, Siyao Li, Benjamin C. Krick, Christopher W. Blair, Yang-Yang Zhou, Michael Tierney, Katherine Sawyer, and all participants in the “Foreign Aid” panel at SSIP/ISA 2025, PSSI 2024, the “Refugees, Violence, and Integration” panel at APSA 2024, the “Domestic Conditions and Peace” panel at MPSA 2024, and the 2024 Pitt–Penn State IR Workshop for their helpful comments and suggestions on earlier versions of this manuscript.

Introduction

Why does foreign aid reduce violence in some contexts but fail—or even backfire—in others? This question is especially acute in refugee-hosting settings, where external resources are routinely deployed to stabilize local communities under demographic and fiscal strain. As of late 2025, more than 41.6 million people were living as refugees or asylum seekers, and over 68 percent were hosted in low- and middle-income countries with limited capacity to absorb large population shocks (UNHCR, 2026). Scholars have long debated whether the presence of refugees itself heightens the risk of civil conflict—through mechanisms such as rebel mobilization and cross-border violence—or instead generates positive spillovers such as economic growth and improved services (Zolberg et al., 1986; Salehyan, 2011; Choi and Salehyan, 2013; Zhou and Shaver, 2021; Lehmann and Masterson, 2020). Less is known about how foreign aid – the policy instrument donors and host governments actually deploy in response to displacement – itself shapes the risk of violence in these same settings.

Indeed, in many Global South states, international assistance plays a central role in responding to displacement, yet its effects on stability vary considerably. In some cases, aid stimulates local markets, enhances service delivery, and reduces grievances and violence (Zhou et al., 2023; Lehmann and Masterson, 2020). In others, it heightens tensions—by creating perceptions of favoritism, deepening intergroup inequality, or attracting insurgent violence (Grossman, 1991; Wood and Sullivan, 2015; Nunn and Qian, 2014; Choi and Salehyan, 2013). The same broad policy response—more aid—thus appears to produce sharply divergent security outcomes.

What explains this divergence? I argue that domestic refugee policy—especially policies governing refugees’ economic rights—is the key moderator of these effects. I define

inclusive refugee policy as legal access to economic participation—in particular the rights to work, move freely, and own or access property. Where such rights are protected, aid is more likely to generate broad local gains rather than narrow transfers. These gains can diffuse beyond camps, support host-community livelihoods, reduce distributive resentment, and raise the opportunity costs of violence. By contrast, restrictive refugee policies limit refugee integration, constrain local spillovers, and can increase local grievances and make refugees more vulnerable to recruitment by armed groups. Refugee policy thus shapes whether aid becomes a stabilizing investment with local spillovers or a contested resource that intensifies insecurity.

I test this argument using a panel dataset covering 2,405 first-level administrative units across 124 countries in the Global South from 1997 to 2016. The empirical strategy applies a shift-share instrumental-variable approach to address potential endogeneity in aid allocation. The results show that foreign aid reduces civil conflict, but only in provinces where national refugee policies are relatively inclusive. Where policies are restrictive, aid has no pacifying effect and may even be associated with increased violence. Similar patterns emerge in the country-level analysis using data on refugee-related violence and in individual-level survey evidence on perceived refugee rights and social integration. Additional analyses provide evidence for the proposed mechanism: inclusive policies enhance the developmental impact of aid, making it more likely to reduce conflict.

This study makes three core contributions to the literature on forced migration, foreign aid, and political violence. First, it provides an institutional explanation for heterogeneity in the security consequences of refugee-hosting by showing that refugee policy conditions whether foreign aid stabilizes or destabilizes local areas. Second, it contributes to the literature on foreign aid effectiveness by identifying a mechanism — shared local economic

gains under inclusive policy regimes — through which aid can foster peace (Savun and Tirone, 2011; Gehring et al., 2022). Third, it challenges the presumed tradeoff between refugee protection and political stability, showing that inclusive refugee policies — when paired with sustained aid — can yield mutually reinforcing benefits for both displaced and host populations (Weiner, 1998; Zhou and Shaver, 2021; Savun, 2022; Lehmann and Masterson, 2020).

Displacement and Civil Conflict Risk

Scholars have long debated how refugee inflows affect the risk of conflict in host states (Zolberg et al., 1986; Salehyan and Gleditsch, 2006), particularly in fragile settings where weak institutions limit governments’ ability to manage population shocks and address local grievances (Böhmelt et al., 2019; Fisk, 2018). Displaced populations can shape civil conflict dynamics in multiple ways, including by altering local demographics, straining state capacity, shifting political and ethnic balances, and inadvertently aiding armed groups.

One influential strand of research emphasizes the so-called “refugee warrior” dynamic, in which displaced populations are seen not merely as victims of violence but as direct contributors to armed conflict through militarization and rebel collaboration (Lischer, 2005; Salehyan, 2011; Haer and Hecker, 2019). Poor conditions in camps, lack of legal employment opportunities, and prolonged displacement can lower the opportunity costs of violence and generate collective grievances that make rebellion more attractive (Lebson, 2013; Savun, 2022). These dynamics are especially pronounced when rebel groups operate near camps and deliberately target displaced populations for recruitment or taxation. During Liberia’s second civil war, for instance, LURD rebels launched raids and forcibly recruited fighters from camps in neighboring Guinea (Achvarina and Reich, 2006).

Apart from direct involvement in violence, displaced populations may also influence conflict dynamics indirectly by reshaping local political and economic incentives. Their arrival can intensify competition over limited resources and shift demographic balances in ways that threaten established interests or identities (Jacobsen, 2005; Chambers, 1986). Such pressures can create openings for armed groups to delegitimize the state and mobilize local support. The risk of such mobilization is especially high when refugees belong to politically or ethnically distinct groups from the host population, thereby reinforcing local narratives of ethnic exclusion and perceived government favoritism toward outsiders (Rüegger, 2019; Fisk, 2019).¹

At the same time, a growing literature challenges the view that refugee presence is inherently destabilizing. In some contexts, refugee-hosting coincides with improved infrastructure, expanded services, and stronger local economic activity, especially where international support is substantial (Lehmann and Masterson, 2020; Zhou and Shaver, 2021; Zhou et al., 2023). These findings suggest that the security consequences of displacement depend on how host communities absorb population shocks and how external resources interact with local institutions. Building on this insight, I argue that refugee policy—particularly policies governing economic rights—shapes whether and how assistance tied to displacement produces stabilizing spillovers or distributive backlash.

Foreign Aid in Refugee-Hosting Settings: A Double-Edged Sword?

Foreign aid can reduce civil conflict by supporting development and altering armed groups' strategic environment (Collier and Hoeffler, 2002). A large literature highlights how aid can mitigate violence by enhancing state capacity and expanding economic op-

¹This discussion highlights pathways through which refugee presence may increase conflict risk, but it does not negate the many crucial social, political, and economic contributions refugees make in host societies (Jacobsen, 2005; Taylor et al., 2016; Zhou et al., 2023).

portunities that disincentivize rebellion (Grossman, 1991; Böhnke and Zürcher, 2013; Gehring et al., 2022). However, aid can also produce adverse effects by distorting local markets, entrenching patronage, and fueling grievances (Dietrich, 2021; Nielsen et al., 2011).

These tensions are particularly salient in refugee-hosting contexts. As displacement becomes more protracted and concentrated in the Global South, international donors have expanded efforts to regions burdened by large refugee populations to support basic needs, stabilize host communities, reduce pressure on services, and contain conflict spillovers (Taylor et al., 2016; Zhou et al., 2023). Yet the consequences of refugee-related aid vary widely.

In some settings, international aid helps improve infrastructure, stimulate markets, and ease the socioeconomic pressures associated with displacement. For refugees, it can reduce exposure to predatory actors and lessen reliance on informal or coercive networks (Nydal, 2023). For host communities, donor-funded projects may ease fiscal burdens on local governments and generate new livelihood opportunities. By improving material conditions and mitigating challenges linked to refugee inflows, aid may reduce the grievances and vulnerabilities that armed groups seek to exploit.

Yet, refugee-related aid can also generate new sources of instability. Influxes of external resources may encourage rent-seeking behavior and make host regions attractive targets for insurgent taxation and territorial control (Wood and Sullivan, 2015; Choi and Salehyan, 2013). When humanitarian assistance appears to benefit refugees disproportionately, it can inflame perceptions of inequality, especially in areas where host populations have long-standing grievances. Competition over aid-related jobs and services may further exacerbate intergroup tensions and foster local resentment, which armed ac-

tors can exploit to mobilize support and undermine state authority (Paler et al., 2020; Breslawski, 2024).

These contrasting outcomes highlight a central puzzle: why does aid stabilize some refugee-hosting areas while contributing to disorder in others? I argue that a key, often overlooked factor is domestic refugee policy. National regulations governing refugees' economic rights fundamentally shape how aid is absorbed, distributed, and perceived, as well as how aid affects intergroup relations between refugees and local communities. Only inclusive refugee policies amplify the conflict-mitigating effects of foreign aid. The next section develops this argument by theorizing the political economy and social consequences of aid effectiveness under different refugee policy regimes.

Refugee Policy and the Political Economy of Aid Effectiveness

Foreign aid has the potential to reduce civil conflict by easing local grievances and raising the costs of violence. Yet in refugee-hosting contexts, the effects of aid vary widely. Consider three contrasting cases: in Kenya, aid concentrated in refugee camps was contentious and deepened local grievances (Breslawski, 2024); in Bangladesh, humanitarian assistance strained public services and disrupted labor markets, increasing tensions (Khaled, 2021); in Uganda, by contrast, aid has been linked to sustained infrastructure development but no public backlash in hosting regions (Zhou et al., 2023).

What explains this variation in aid's effects across refugee-hosting contexts? I argue that refugee policy serves as an institutional filter that shapes how foreign aid influences civil conflict. When refugee policies are inclusive, aid is more likely to generate shared economic gains that benefit both displaced and host populations. These gains, such as increased employment, market activity, and access to services, can promote the implementation of aid projects and reduce material grievances, raising the opportunity

costs of rebellion. By contrast, restrictive policies limit refugees' ability to participate in the economy, reduce the local benefits of aid, and heighten the risk of competition and resentment between refugees and the locals.

My argument builds on recent work linking refugee-hosting to development, but shifts attention to national refugee rights. Zhou et al. (2023) show that in Uganda, inclusive resource allocation can benefit both hosts and refugees to prevent public backlash. While their focus is on distributive practices, I emphasize a complementary but distinct mechanism operating at the national policy level: the role of refugee rights. Specifically, I argue that policies granting refugees the right to work, move, and access services shape whether aid generates broader economic spillovers. In doing so, this paper identifies a key scope condition: the pacifying effects of aid are most likely to materialize when national refugee policies empower refugees to participate meaningfully in host economies.

When refugees have economic agency, aid can catalyze broader local development since better economic integration of refugees enhances the positive externalities of aid projects for neighboring communities. Refugees legally permitted to move, work, and access markets can contribute labor, trade, and circulate aid-funded goods, even in camp-based settings (Jacobsen, 2005; Taylor et al., 2016). Cash-based assistance can further amplify these effects by stimulating demand across host economies (Lehmann and Masterson, 2020). These spillovers improve livelihoods and can make violence less attractive by increasing its opportunity costs for both groups (Sexton, 2016).). Moreover, when locals see refugees as economic contributors under inclusive refugee policies, rather than as passive aid recipients, grievances and tensions toward refugees may decline. In turn, host communities may become more supportive of refugees' social and economic integration (MacDonald and Lichtenheld, 2025).

Restrictive refugee policies, by contrast, tend to isolate displaced populations and limit the developmental impact of aid. Refugees are excluded from economic life, while host communities may see aid as unfairly favoring outsiders. These dynamics can not only obstruct the implementation of aid projects, but also reinforce zero-sum perceptions and deepen local grievances, creating conditions that armed groups may exploit for recruitment (Paler et al., 2020; Choi and Salehyan, 2013; Lischer, 2005).

However, not all forms of refugee inclusion are equally relevant for shaping conflict outcomes. Among the various dimensions of refugee policy, economic rights are especially important for determining whether aid fosters inclusive development. These rights directly affect refugees' ability to engage in local economies and form material ties with host populations (Jacobsen, 2005; Lehmann and Masterson, 2020). By contrast, non-economic rights, such as legal residency and political participation, may contribute to long-term integration and improve welfare outcomes, but are less directly linked to the economic mechanisms through which aid reduces civil conflict. While such rights may have important indirect effects, such as strengthening trust and reducing social marginalization (MacPherson and Sterck, 2021), they may also encounter substantial resistance—or even provoke backlash—if they are not accompanied by economic integration and tangible material benefits from refugee hosting (Pevnick, 2024). Therefore, it is the extent of economic inclusion that most directly and immediately conditions whether foreign aid mitigates or exacerbates tensions in refugee-hosting areas.

Hypothesis 1: Foreign aid is more likely to reduce civil conflict in refugee-hosting provinces when host countries adopt economically inclusive refugee policies.

Research Design

This study focuses on refugee-hosting countries in the Global South for two reasons. First, while these countries host the majority of the world’s refugees, much of the literature centers on forced migration in the Global North (Mestheneos and Ioannidi, 2002; Hatton, 2016). Second, refugee-hosting contexts in the Global South differ significantly in capacity and institutional infrastructure (Blair et al., 2022). Unlike high-income countries, many states in the Global South lack the resources to accommodate large refugee populations and depend heavily on foreign aid. To evaluate how policies liberalizing refugees’ economic rights shape the effect of foreign aid on conflict, I apply a cross-level empirical design that examines different dimensions of the theoretical framework and provides both causal and suggestive evidence across the country, province, and individual levels.

The main analysis is conducted at the subnational level to estimate the effects of aid and refugee policy in refugee-hosting areas, where aid is implemented and conflict occurs. The unit of analysis is the province–country–year, following Zhou and Shaver (2021).² I restrict the sample to Global South countries that host refugees and receive at least one World Bank aid project during the study period. This enables comparisons across provinces in similar country contexts.³ While geocoded aid data cover World Bank projects through 2024, I focus on humanitarian projects before 2017 to avoid endogeneity concerns from the IDA18 refugee-host-community financing window, which made financing partly conditional on refugee-policy reforms.⁴ The main analysis therefore covers 1997-2016. Figure 1 maps the sample: 2,405 provinces across 124 Global South countries

²Province boundaries correspond to the GADM database.

³I operationalize the Global South as countries that fall into the low- or lower-middle-income categories at least once during the study period, approximating the lower-capacity settings most relevant to the theory (Blair et al., 2022).

⁴See the World Bank IDA18 Replenishment <https://ida.worldbank.org/en/replenishments/ida18-replenishment/ida18-regional-sub-window-for-refugees-host-communities> for details. Table A19 shows that the results are robust to using the full available sample.

that received World Bank aid between 1997 and 2016. Provinces included in the analysis are highlighted in orange, and refugee locations are indicated by red points. Of these, 686 provinces experienced refugee presence during the study period.

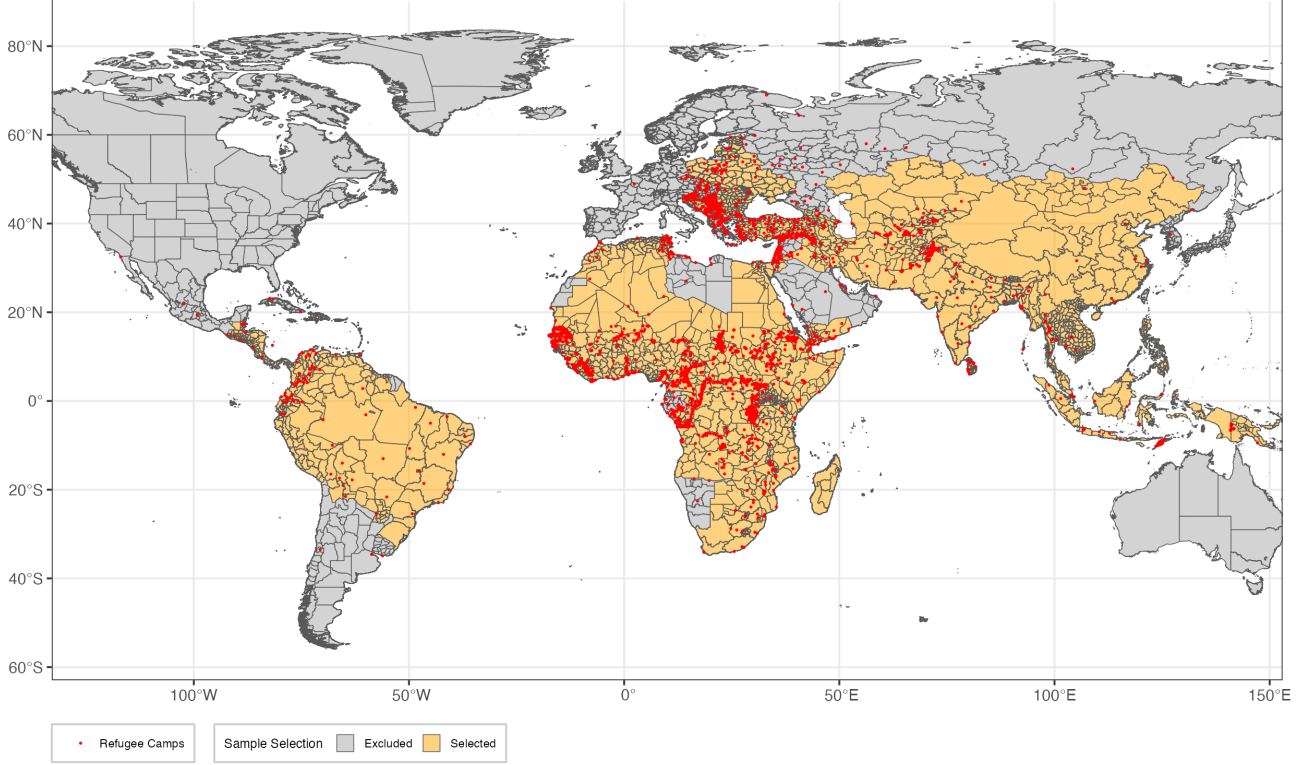


Figure 1: Sample Selection

The dependent variable, civil conflict, is measured as the presence of salient intrastate conflict between government forces and rebel groups in each province-year. Specifically, I follow Gehring et al. (2022) to construct a binary indicator equal to 1 if at least five battle deaths are recorded in a province-year and 0 otherwise. This measure better fits the rare-event structure of the data: only 11.1% of province-years experience any battle deaths (Chen and Roth, 2024).⁵ Conflict data come from the UCDP Georeferenced Event Dataset (GED); I define civil conflict as organized violence between a state government and one or more non-state actors (Sundberg and Melander, 2013).

⁵This binary measure captures civil conflict above a minimal intensity threshold and avoids counting isolated low-fatality events. It also better fits the rare-event structure of the data than log-transformed intensive measures. The findings are robust to alternative thresholds—any battle deaths and at least 25 battle deaths—as well as logged battle deaths and conflict events.

The main explanatory variable is foreign aid, measured using the newly released Geocoded Official Development Assistance Dataset (GODAD) (Bomprezzi et al., 2025), which documents 5,695 projects across more than 78,000 subnational locations from 1995 to 2023. I focus specifically on World Bank aid projects with humanitarian or refugee-related objectives, rather than on development projects more broadly. These include projects related to food and cash assistance, post-conflict reconstruction, water and sanitation, health and education facilities, and social infrastructure for integration and protection in the Global South. Such projects account for 79% of all World Bank projects during the study period 1997 – 2016.⁶ I use World Bank humanitarian projects for two reasons. First, because the paper examines how refugee policies moderate aid effectiveness, a data source less directly tied to individual donor geopolitical preferences offers a cleaner test: compared to bilateral donors, whose allocations are more closely tied to political considerations, the World Bank’s process is typically viewed as less susceptible to such dynamics, which reduces endogeneity concerns (Bomprezzi et al., 2025).⁷ Second, humanitarian aid is more closely tied to the paper’s mechanisms than large-scale development projects in industrial or manufacturing sectors, as it is more likely to support displaced populations directly and to circulate through population movement and local markets.⁸ For the measure, I use the logged value of committed aid (in constant 2014 USD), aggregated to the province–year level. Committed aid is used instead of disbursed aid due to substantial and systematic missingness in the latter, although Table A7 shows

⁶I also use all World Bank projects as an alternative measure to show that the results are not sensitive to this classification of aid. The first Column of Table A7 presents findings consistent with the main analysis.

⁷Donor preferences and donor–recipient bargaining over refugee policies are important issues for future research but lie beyond the scope of this study.

⁸An ideal measure of refugee-targeted aid would be expenditures and humanitarian projects implemented by refugee-related INGOs such as UNHCR and the WFP. Unfortunately, systematic global data of this kind are not currently available in open-access form. Developing such a dataset would be a valuable contribution for future research.

that the findings are not sensitive to this choice.⁹

I measure refugee presence using the Global Displacement Locations Dataset (Zhou and Shaver, 2021), which provides geo-coded data on camps, settlements, urban shelters, and non-camp settings from 1990 to 2019. I intentionally include this full range of refugee accommodation types because restrictive refugee policies often confine displaced populations to camps, whereas more inclusive policies allow for residence in urban or integrated non-camp settings. Capturing this variation is essential for measuring how refugee policy liberality conditions the effect of aid (Fisk, 2019). I construct a binary indicator for whether a province has any refugee settlement in a given year, capturing both camp-based and non-camp populations.¹⁰ This dichotomous measure reduces sensitivity to annual fluctuations in the number and type of recorded refugee sites, which often reflect temporary relocations, administrative reclassifications, or data inconsistencies rather than meaningful differences in refugee presence across provinces.¹¹

To measure host-country refugee policies, I use the Dataset of World Refugee and Asylum Policies (DWRAP), which codes *de jure* refugee laws across 193 countries (Blair et al., 2024, 2022).¹² The dataset captures five dimensions of refugee policy: access (right to enter), freedom of movement, livelihoods (right to work and own property), access to public services, and political participation.

⁹Disbursement data are often missing for countries with weaker institutional capacity, raising concerns about nonrandom selection. Committed aid avoids this bias and better captures donor intent, even if the project is not fully implemented. I log-transform committed aid amounts to reduce right skewness.

¹⁰Because UNHCR-based geocoded data record recognized settlement or accommodation sites rather than isolated individual cases, this measure captures substantively meaningful refugee presence rather than minor records involving only a handful of refugees.

¹¹Comprehensive subnational data on refugee population size are unavailable. While UNHCR and IOM collect detailed subnational data in some countries, coverage is sparse and not comparable across cases, making it unsuitable for cross-national analysis.

¹²While national laws may not fully capture local implementation, *de jure* measures provide a consistent and transparent cross-national baseline. They define the legal framework guiding state agencies and international organizations such as UNHCR, and are less vulnerable than expert surveys or case-specific reports to short-term political shifts or measurement bias (Blair et al., 2022). Since no cross-national dataset systematically captures subnational *de facto* refugee rights, *de jure* measures remain the most viable option.

Foreign aid should be especially effective when refugee policy liberalizes economic rights, such as freedom of movement and property rights, rather than political rights. I therefore separate refugee policies from the DWRAP database into two domains: economic and non-economic rights. The economic rights index includes policies related to freedom of movement, labor market access, and access to services such as health and education, capturing dimensions such as settlement, employment, land and property rights, and service provision. The non-economic rights index reflects policies concerning legal status, documentation, and political participation. Following Blair et al. (2022), I standardize both indices to range from 0 (most restrictive) to 1 (most inclusive).¹³ The main analysis uses the economic index of refugee rights, and robustness analyses using the composite DWRAP index yield consistent results.

To ensure correct temporal sequencing and mitigate simultaneity concerns, all explanatory and control variables are lagged. The lag structure reflects the theoretical sequence in which refugee context shapes how aid affects conflict outcomes, while minimizing post-treatment bias.¹⁴ Refugee presence and refugee policy are lagged by three years to allow sufficient time for displaced populations and institutional reforms to influence local political and economic conditions. Aid is lagged by two years, reflecting the typical timeline for World Bank project implementation and the delayed effects of those projects on conflict-related outcomes (Moloney, 2009).¹⁵ This approach follows prior studies using committed aid measures, including Gehring et al. (2022) and Dreher

¹³The rationale for this disaggregation and detailed information on the coding and construction of both indices are provided in Section A.4.

¹⁴The main findings are robust to alternative lag structures, as shown in Table A13 and Figure A5 in the Appendix.

¹⁵The period from commitment to first disbursement typically ranges from 6 to 12 months (<https://documents.worldbank.org/pt/publication/documents-reports/documentdetail/099207310162419629>). Given that infrastructure development and service delivery may further require an additional year or more, a two-year lag provides a reasonable approximation.

et al. (2021).¹⁶

Figure A1 provides a visual preview of the conditional relationship. The left column shows the full sample, where foreign aid has a weakly positive correlation with the likelihood of salient civil conflict. The middle and right columns focus on the subsamples with the most restrictive policies (bottom 10th percentile) and the most inclusive policies (top 10th percentile). Although both subsamples show a similar likelihood of salient civil conflict among provinces without aid, the relationship diverges as aid increases. Under inclusive policies, aid is negatively correlated with conflict, whereas under restrictive policies, the relationship is positive. Although descriptive, this pattern aligns with the theoretical expectation that aid reduces violence only in contexts where refugee policies facilitate economic participation and local integration.

Following Zhou and Shaver (2021), I include a set of covariates to account for potential confounders. These include the logged population aggregated from the PRIO-GRID dataset (Tollefsen et al., 2012), GDP per capita (in PPP-adjusted USD) aggregated from Kummu et al. (2025)’s downscaled global gridded database, as well as indicators for the presence of internally displaced persons (IDPs), returned refugees, and refugee populations in other provinces. To account for the possibility that refugee inflows alter local ethnic power dynamics and increase the risk of intergroup tensions (Rüegger, 2019), I also control for the presence of politically excluded ethnic groups, using data from the GeoEPR dataset (Vogt et al., 2015).

Estimation

To test the conditional effect of foreign aid on conflict, I begin with linear models that include a triple interaction between refugee policy, refugee presence, and foreign aid. This

¹⁶A detailed rationale for lag choices is provided in Section A.10 of the Appendix.

specification follows the theoretical argument. The interaction between humanitarian aid and refugee presence captures whether the effect of aid on conflict differs in refugee-hosting relative to non-hosting provinces, while the triple interaction further estimates whether that differential effect is conditional on national refugee-rights policy.

For the main analysis, I estimate a linear probability model (LPM) with province and country-by-year fixed effects. This specification provides a transparent baseline under minimal modeling assumptions and aligns with prior fixed-effects approaches in the conflict literature. Province fixed effects absorb time-invariant differences across subnational units, while country-by-year fixed effects account for national-level shocks that may jointly shape refugee policy, aid allocation, and conflict risk. One important feature of the data structure is that aid and conflict are observed at the province level, whereas the key moderator, refugee policy, varies at the country-year level. As a result, subnational observations within the same country may share common policy-related shocks and correlated errors. I therefore cluster standard errors at the country level, consistent with the level at which refugee policy varies.

The following model is estimated using the fixed-effects LPM specification:

$$\begin{aligned} \mathbf{Violence}_{i,c,t} = & \beta_0 + \beta_1 Aid_{i,c,t-2} + \beta_2 Refugee_{i,c,t-3} + \\ & \beta_3 Policy_{c,t-3} \times Refugee_{i,c,t-3} + \beta_4 Aid_{i,c,t-2} \times Policy_{c,t-3} + \\ & \beta_5 Aid_{i,c,t-2} \times Refugee_{i,c,t-3} + \beta_6 Aid_{i,c,t-2} \times Refugee_{i,c,t-3} \times Policy_{c,t-3} + \\ & \beta_7 X_{i,c,t-3} + \mu_{i,c} + \tau_{c,t} + \epsilon_{i,c,t} \end{aligned}$$

where $Violence_{i,c,t}$ denotes the presence of salient civil conflict in province i , country c , and year t . $Aid_{i,c,t-2}$ captures World Bank aid commitments lagged by two years; $Refugee_{i,c,t-3}$ is a binary indicator for refugee presence lagged by three years; and $Policy_{c,t-3}$ is the national-level refugee policy index, also lagged by three years.

$\mathbf{X}_{i,c,t-3}$ represents a vector of lagged control variables. $\mu_{i,c}$ and $\tau_{c,t}$ are province and country-by-year fixed effects.

β_1 represents the effect of aid in provinces without refugee presence under the most restrictive policy (policy = 0). $\beta_1 + \beta_4$ represents the effect of aid in non-refugee areas under inclusive policies at their ideally maximum level (policy = 1), $\beta_1 + \beta_5$ captures the effect of aid in refugee-hosting provinces under restrictive policies.¹⁷ $\beta_1 + \beta_4 + \beta_5 + \beta_6$ is the fully conditional estimate of aid's effect in refugee-hosting provinces under inclusive policies. Because refugee policy is measured at the country level, interaction terms involving policy capture average moderation effects in the fixed-effects specification.¹⁸

This fixed-effects design is intentionally conservative, but it has two limitations. First, country-by-year fixed effects absorb the additive effect of country-year refugee policy, meaning that the direct effect of refugee policy cannot be separately estimated. The interaction terms involving refugee policy should therefore be interpreted as average moderation effects identified through within-country-year differences in aid exposure and refugee presence. Second, the fixed-effects specification does not explicitly model cross-national heterogeneity in the moderating effect of refugee policy. I therefore report two supplementary analyses: multilevel mixed-effects models with country-level random effects and country-specific random slopes for refugee policy (Franzese, 2005), and country-year models showing that the findings are not driven by the subnational error structure.

Identification Strategy and Addressing Endogeneity

Estimating how foreign aid interacts with host-country refugee policies to shape conflict raises several endogeneity concerns. Aid is strategically allocated, refugee settlement is

¹⁷For the Global South sample, the maximum value of the economic policy index is 0.735.

¹⁸I do not discuss the main effect of refugee presence (β_2) or its interaction with refugee policy (β_3), since the focus is on the conditional effects of aid.

non-random, and refugee policy may respond to the same domestic and regional conditions that shape violence. I address these concerns with a layered design combining a shift-share IV, fixed effects, matching, and a rich set of controls, each targeting a distinct source of potential bias.

Endogeneity in Subnational Aid Allocation

A primary concern is that aid responds to local conflict risk and to the presence of displaced populations. To isolate plausibly exogenous variation in aid, I use a shift-share IV that interacts annual variation in donor capacity with each province’s historical propensity to receive aid (Nunn and Qian, 2014; Dreher et al., 2021; Gehring et al., 2022). The shift is International Development Association’s (IDA) liquidity in the year of commitment; as the World Bank’s concessional financing arm for low- and lower-middle-income countries, IDA’s annual financing capacity captures variation in aid availability across the developing world. The share is a time-invariant measure of subnational exposure, based on aid commitments during the first five years of the study period, before most of the analysis period.¹⁹ Their interaction generates predicted aid exposure unlikely to be driven by contemporaneous local conflict dynamics. Table A10 reports strong first-stage results across specifications, alleviating weak-instrument concerns.

I assess the plausibility of the shift-share design through diagnostic exercises reported in Appendix Section A.7.2. Following Gehring et al. (2022), I first examine whether the identifying variation reflects external IDA liquidity shocks rather than differential trends across historically high- and low-aid provinces. Figure A3 shows that conflict

¹⁹I construct the share from the first five years of aid commitments rather than average aid receipt over the full panel to ensure that exposure is predetermined rather than shaped by later outcomes. Full-period shares may incorporate project placements responding to subsequent conflict dynamics, weakening the instrument’s plausibility (Gehring et al., 2022). The five-year window captures each province’s pre-existing aid propensity while reducing sensitivity to allocations in any single year. The results are robust to using a shorter three-year baseline window (Appendix Section A.7.2).

differences between high- and low-baseline aid-share provinces do not track fluctuations in IDA liquidity, including when the sample is split by refugee presence and refugee-policy inclusiveness. This supports the interpretation that the shift component captures external variation in IDA liquidity rather than province-level conflict trends. Next, I conduct several additional sensitivity tests, including analyses that exclude the first five years used to construct the share component and an alternative share measure based on a shorter three-year baseline window (Table A11). I also estimate an alternative time-varying shift-share specification following Gehring et al. (2022) and conduct a temporal placebo test showing null effects of the instrument on pre-treatment outcomes (Figure A4). Together, these diagnostics suggest that the identifying variation is driven by IDA liquidity shocks rather than patterns that jointly shape aid allocation, refugee-policy design, and conflict outcomes. Because the model is just-identified, these tests cannot directly prove the exclusion restriction, but they provide informative evidence consistent with its plausibility.

One remaining concern is that donors may allocate aid in response to liberal refugee policies, as in Jordan, where European donors increased support after policy concessions during the Syrian refugee crisis (Kelberer, 2017). This concern can be mitigated both through the research design and through the structure of the data. First, this paper uses World Bank aid rather than aid from bilateral donors with specific geopolitical interests, which reduces concerns about donor-recipient policy bargaining, and excludes all province-years after 2016, when the World Bank began treating refugee policy as a condition for aid. Second, descriptive Figure A2 in Appendix Section A.2 also shows that there is no distinguishable difference in the average level of aid allocation among province-years with most restrictive, medium and most inclusive policy environments.

Selection into Refugee-Hosting Locations

A second concern is that provinces that host refugees may differ systematically from non-hosting provinces, as refugees disproportionately settle in border or peripheral regions with weaker state presence and higher baseline insecurity (Steele, 2019). To reduce selection on observables, I use Coarsened Exact Matching (CEM), to compare refugee-hosting and non-hosting provinces that are similar on baseline characteristics, including prior aid exposure and conflict intensity, and socioeconomic indicators used in the main analysis (Iacus et al., 2012).²⁰ The main results are substantively unchanged in the matched sample shown in Columns 1 to 3 in Table A15.

Selection into Refugee Policy Regimes

A third concern is that refugee policy is not randomly assigned. Current studies show that refugee policy reflects complex political considerations rather than a simple response to either foreign aid or conflict dynamics. Global South governments may liberalize refugee rights in response to strategic, institutional, and geopolitical pressures correlated with conflict risk, including anticipated refugee inflows, regional instability, shared ethnic ties, regime type, bureaucratic structures, reputational incentives, and donor pressure (Breslawski, 2024; Blair et al., 2022; Abdelaaty, 2021; Bermeo, 2017; Tsourapas, 2019). At the same time, cross-national evidence suggests that refugee policy is not mechanically driven by aid or conflict: governments often resist aid-for-policy exchanges because of concerns about donor reliability, conditionality, and domestic autonomy (Blair et al., 2022).

I treat policy selection as a potential source of confounding and address it in three ways. First, the baseline models include country-by-year fixed effects, which absorb

²⁰Table A17 shows that the CEM approach produces a highly balanced matched sample of provinces with and without refugee presence.

time-varying country-level factors that may influence both policy decisions and conflict risk. Identification of the moderating role of refugee policy therefore comes from within-country-year comparisons across provinces that differ in refugee presence and aid exposure.

Second, Columns 5-8 in Table A12 report models that include covariates plausibly related to both refugee policy and conflict, including V-Dem variables on Electoral and Liberal Democracy, population, GDP, ethnic ties between host and displaced populations, regional displacement and conflict shocks, and conflict in neighboring countries. The latter is especially important because neighboring conflict can generate refugee crises and shape governments' policy responses (Blair et al., 2022).

Third, Columns 4-6 in Table A15 apply coarsened exact matching to construct covariate-balanced comparisons across higher and lower levels (above median) of refugees' economic and non-economic rights, conditioning on the full set of country-level covariates. The moderating effect persists.²¹

Triangulating Across Levels of Analysis

A potential concern in the main analysis is that refugee policy is measured nationally while aid and outcomes are evaluated at the province level.²² Refugee rights are legislated nationally, and major donors typically negotiate program design with central authorities. National law therefore defines the *de jure* environment in which subnational aid is implemented and refugees' economic incorporation becomes possible. As a result, refugee policy functions in the analysis as a country-year moderator of the aid-conflict relationship, not as a source of variation across provinces within the same country. Province

²¹Table A17 shows that the CEM approach produces a highly balanced matched sample of provinces in country-years with inclusive versus restrictive economic policies.

²²Systematic, comparable measures of refugee-rights implementation are not currently available across countries or at the subnational level.

fixed effects absorb persistent subnational differences in administrative capacity and enforcement. If local implementation diverges from national law, the policy measure is an imperfect proxy for the *de facto* environment, likely attenuating the estimated moderation effects.

To address this concern, I triangulate the evidence at two additional levels. First, I replicate the analysis at the country-year level by constructing a measure of “refugee-targeted aid,” defined as World Bank humanitarian aid allocated to refugee-hosting provinces within each country-year. I then test whether its relationship with conflict is conditional on policies liberalizing refugees’ economic rights, using UCDP country-level civil conflict episodes (Davies et al., 2025) and measures of refugee-related violence from the Political and Societal Violence By and Against Refugees (POSVAR) database (Gineste and Savun, 2019). Second, I provide suggestive individual-level evidence from a cross-national survey on the economic lives of refugees by Betts et al. (2024). This analysis examines whether refugees’ perceptions of *de facto* policy implementation shape their sense of security and integration into host communities. Together, these analyses show that the findings are not sensitive to the level of aggregation or reliance on *de jure* policy measures, while linking macro-level violence records to micro-level attitudes and behavior not captured in the province-level analysis alone.

Results

Province-Level Evidence

Table 1, Models 1-4, reports the baseline linear probability model estimates with province and country-by-year fixed effects.²³ Model 1 estimates the relationship between humanitarian aid and conflict conditional on refugee presence only. Without taking refugee

²³Coefficient estimates for all control variables are presented in Table A18.

policy into consideration, aid has no statistically significant effect on the extensive margin of civil conflict, though the point estimate for refugee-hosting areas is positive. Once refugee policy is introduced in Model 2, a clear pattern emerges: the effect of aid on conflict is conditional on the policy environment. In countries with the most restrictive refugee policies, humanitarian projects in refugee-hosting provinces are associated with a higher likelihood of salient civil conflict. By contrast, under more inclusive policies, such aid projects significantly reduce the likelihood of conflict. Moreover, the model specification makes clear that the conflict-reducing effect of aid under more inclusive policies is concentrated in provinces that host refugees. In provinces without refugees, aid has no discernible impact on conflict, even when national policies are inclusive, suggesting that inclusive policies alone are insufficient; the presence of refugees is a necessary condition for aid to have a pacifying effect.

	Economic Rights				Non-Economic Rights			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Aid	-0.001 (0.000)	0.000 (0.001)	0.000 (0.001)	0.000 (0.001)	-0.003 (0.003)	-0.002 (0.003)	-0.001 (0.000)	-0.003 (0.002)
Aid x Policy		-0.002 (0.002)	-0.003 (0.002)	-0.002 (0.002)	0.013 (0.013)	0.010 (0.012)	0.000 (0.003)	0.018 (0.014)
Aid x Refugee	0.001 (0.001)	0.005** (0.002)	0.005** (0.002)	0.005** (0.002)	0.012* (0.007)	0.011 (0.007)	0.002 (0.002)	0.000 (0.006)
Aid × Refugee × Policy		-0.014** (0.006)	-0.014** (0.006)	-0.013** (0.006)	-0.078*** (0.029)	-0.069*** (0.026)	-0.006 (0.007)	-0.025 (0.023)
Refugee	0.005 (0.011)	-0.028 (0.035)	-0.027 (0.036)	-0.029 (0.034)	-0.118 (0.087)	-0.100 (0.085)	-0.014 (0.030)	-0.001 (0.077)
Refugee x Policy		0.117 (0.098)	0.115 (0.098)	0.112 (0.095)	0.875** (0.336)	0.775** (0.318)	0.072 (0.106)	0.346 (0.316)
Conflict				0.084*** (0.016)		0.084*** (0.012)	0.085*** (0.016)	0.085*** (0.016)
Full Control			✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Model	OLS	OLS	OLS	OLS	IV	IV	OLS	IV
Num. obs.	47795	47795	47795	47795	47795	47795	47795	47795
R ² (full model)	0.559	0.559	0.559	0.562	0.559	0.562	0.562	0.562
R ² (within)	0.000	0.001	0.001	0.008	0.001	0.008	0.008	0.008

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table 1: Main Results

Models 3 and 4 introduce the full set of covariates and a lagged dependent variable to account for time-dependent unobserved heterogeneity, such as a province's underlying

risk of violence. The results remain stable, suggesting that the conditional effects are not artifacts of observed confounders or persistent conflict propensity. These patterns underscore the argument that both the policy environment and refugee presence must align for aid to mitigate tensions. This provides strong support for Hypothesis 1.

While the baseline models provide a useful benchmark and reveal the expected conditional pattern, the estimated effect of aid may still be biased by unobserved time-varying factors. To address this concern, I replicate Models 3 and 4 using the shift-share instrumental variable described earlier and validated through the diagnostic tests in Appendix Section A.7.2. The first-stage results in Table A10 show strong instrument relevance, and the sensitivity tests on shock exogeneity and additional robustness checks in Figure A3 and Table A11 provide indirect support for the exclusion restriction.²⁴ Models 5 and 6 in Table 1 show the second-stage IV estimates for specifications with the full set of covariates and a dynamic model controlling for the lagged DV.²⁵

Both models support a causal interpretation of the conditional effect: among refugee-hosting provinces, the marginal effect of aid becomes conflict-reducing as economic refugee policy becomes more inclusive. The effect size is roughly five times larger than in the OLS estimates, consistent with reverse causality—aid is disproportionately allocated to already-unstable areas—biasing the OLS coefficient toward zero. The IV estimates correct for this bias by isolating variation in aid exposure unrelated to local conflict conditions.²⁶

The results also point to an important asymmetry, however. Across Models 2–6, liberal

²⁴The interaction term is estimated following Nunn and Qian (2014)’s approach. See Appendix Section A.7.1 for details.

²⁵In addition to temporal interdependence, a remaining concern is that aid distribution and conflict outcomes may generate spatial spillovers across neighboring provinces. Table A14 addresses this concern by adopting specifications with spatially lagged dependent variables and aid allocation. The findings remain consistent with the main results.

²⁶As an additional robustness check, I replicate the baseline models using matched data to compare refugee-hosting and non-hosting provinces. The results, presented in Table A15, are consistent with the main findings.

economic rights on their own—in provinces that receive little or no aid—either have no effect on conflict or, in some specifications, increase it. Inclusive refugee policy and foreign aid thus appear to be complements rather than substitutes: policy liberalization mitigates conflict only when paired with material support, not as a standalone policy lever.

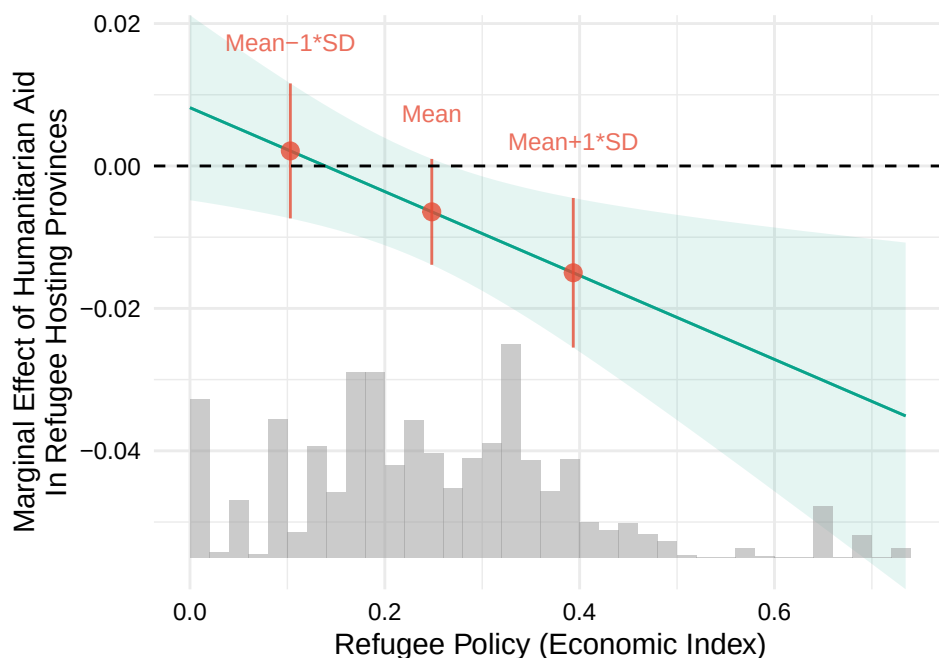


Figure 2: Marginal Effects of Aid Conditional on the Refugee Policy in Refugee-Hosting Provinces

To illustrate the substantive effects, Figure 2 plots the marginal effect of humanitarian aid on conflict, conditional on the liberality of refugee policies in refugee-hosting provinces.²⁷ The results show that in provinces operating under top-quartile inclusive refugee policies, moving from the mean level of aid in areas with refugees (49,000 USD) to the 75th percentile (1.26 million USD) is associated with an approximately 3.7 percentage point reduction in the likelihood of salient civil conflict. This magnitude is consistent with theoretical expectations and substantively meaningful, especially given the rarity of civil conflict at the province-year level.

²⁷Based on Model 6 in Table 1.

Models 7 and 8 present a scope-condition test showing that the conflict-mitigating effects of foreign aid emerge only under inclusive economic refugee policies. While non-economic dimensions of refugee policy, including legal residency and broader social and political rights, may reduce hostility through other channels, they are less directly linked to aid effectiveness. Consistent with this expectation, foreign aid has a statistically insignificant effect on conflict in refugee-hosting areas when countries adopt more inclusive non-economic refugee policies, under both LPM (Model 7) and 2SLS (Model 8) specifications.²⁸ Together, these findings indicate that the moderating effect of refugee policy does not arise from broad liberalization across all dimensions. Rather, it is economic rights that condition aid effectiveness by shaping how refugees can utilize resources and how benefits spill over to local communities.

Taken together, the findings provide consistent evidence that the conflict-reducing effect of foreign aid depends on both the presence of refugees and the policy environment into which aid is delivered. Inclusive refugee policies, in particular those related to economic rights, strengthen the capacity of aid to reduce violence, while restrictive policies may blunt or reverse these effects.

Robustness Tests

I assess whether the main conditional relationship is robust to alternative outcomes, measures, samples, and model specifications. Across all of these checks, the results remain consistent with the main findings.

First, the findings are robust across alternative operationalizations of civil conflict, including two binary thresholds—any battle deaths (> 0) and at least 25 battle deaths (≥ 25)—and continuous intensive-margin measures (Table A8). The results also hold when

²⁸Table A5 confirms that this pattern holds in different model specifications.

using continuous refugee presence, disbursed rather than committed aid, and alternative measures of World Bank aid, including all projects and development projects (Table A7). I further probe the scope of the argument using UCDP and ACLED data (Raleigh et al., 2010; Sundberg and Melander, 2013) to examine non-state violence and social conflict. Both outcomes exhibit the same conditional pattern under inclusive refugee policies, providing diagnostic evidence that aid also mitigates broader forms of intergroup tension (Columns 1–4 in Table A9). Finally, the findings hold using the general refugee-policy index (Table A5) and disaggregated economic-rights subcomponents, including livelihood rights and access to public services. The results for freedom of movement are more mixed, likely because movement restrictions and encampment policies are themselves correlated with where refugees settle and where aid is concentrated, particularly in vulnerable border areas. Excluding movement from the economic-rights index yields results consistent with the main findings (Table A6).

Second, the findings are not an artifact of sample selection. Columns 5–8 in Table A9 show that the results hold in samples restricted to countries that either changed or liberalized refugee policies at least once, suggesting that the moderating effect is not driven by persistent slow-moving cross-country differences in policy regimes. Although the main analysis excludes post-2016 province-years to avoid endogeneity concerns tied to the World Bank’s 2017 refugee-policy financing window, Table A19 shows that the results remain stable in the full 1997–2022 sample.

Third, the results are not an artifact of modeling assumptions. One concern is the hierarchical data structure: refugee policy is measured at the country level, while aid and conflict are measured at the province level. The main specification provides a transparent baseline under minimal assumptions, but it does not directly model cross-national

heterogeneity in policy effects and may sacrifice precision as a result. I therefore estimate multilevel mixed-effects models that separate within- and between-country variation in policy effects (Franzese, 2005). The findings remain robust (Appendix Section A.8, Columns 1–4 in Table A12). I also account for spatial dependence by re-estimating models with spatial lags of the outcome and aid; the conclusions remain unchanged (Table A14).

Country-Level Evidence

In addition to the province-level analysis, I conduct a country-year analysis using the same Global South sample to assess robustness at this level of aggregation. I measure national refugee aid as the logged amount of all World Bank humanitarian aid allocated to refugee-hosting areas in a given country-year. The main outcome variable is a binary indicator for whether the country experienced at least one episode of civil conflict, based on the UCDP organized violence database (Davies et al., 2025). I also draw on the POSVAR database (Gineste and Savun, 2019) to test the proposed mechanism more directly, using its 0–3 ordinal measures of refugee-related violence. The theory implies that, under economically inclusive policies, refugee-targeted aid with local spillovers should reduce civil conflict and violence against refugees by easing tensions between refugees and host populations. As in the province-level analysis, I use a shift-share IV to address aid-allocation endogeneity.²⁹

Table 2 presents the results of the country-level analysis. Columns 1 and 2 are consistent with the province-level findings under both OLS and IV: aid to refugee-hosting areas alone is not sufficient to reduce conflict risk—the pacifying effect emerges only under inclusive policies that grant refugees liberal economic rights. Columns 3 through 6 provide further support for the proposed mechanism: once governments liberalize refugees’ eco-

²⁹See Appendix Section A.9 for details on operationalization, covariates, and estimation.

conomic rights, aid allocated to refugee-hosting areas significantly reduces violence against refugees by both armed actors and civilians, consistent with the idea that economic spillovers ease intergroup tensions. Columns 7 and 8, by contrast, show no evidence that aid and inclusive policies reduce violence committed by refugees themselves—though this is unsurprising given how rare such events are in the sample, accounting for only 1.47% of observations.

	Civil Conflict		Rebel Against Ref.		Civ. Against Ref.		Ref. Against Civ.	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Refugee Aid	0.015*** (0.003)	0.006 (0.016)	0.015*** (0.005)	0.039** (0.016)	0.010* (0.005)	0.020 (0.025)	0.003 (0.003)	0.002 (0.006)
Refugee Aid x Policy	−0.038*** (0.014)	−0.269*** (0.073)	−0.044** (0.019)	−0.290*** (0.073)	−0.034* (0.019)	−0.396*** (0.115)	0.006 (0.010)	0.029 (0.029)
Policy	−0.544 (0.357)	10.049*** (2.981)	−1.807*** (0.509)	8.454** (3.352)	−0.832* (0.480)	17.948*** (5.430)	−0.432 (0.264)	−1.296 (1.375)
Conflict	0.145*** (0.025)	0.133*** (0.049)	0.090*** (0.026)	−0.004 (0.042)	0.073*** (0.026)	−0.216** (0.088)	−0.001 (0.029)	0.014 (0.033)
Full Control	✓	✓	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Num. obs.	1893	1881	1696	1684	1696	1684	1696	1684
Num. Country	111	109	110	108	110	108	110	108
Num. Year	19	19	17	17	17	17	17	17

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table 2: Country-level Evidence

Policy Implementation and Micro-Level Evidence

The main analysis examines how national *de jure* policy moderates province-level aid effectiveness, but two measurement concerns remain: legal rights may be unevenly implemented in practice, and World Bank projects fund broad investments rather than assistance that refugees receive directly. I therefore triangulate with individual-level data from Betts et al. (2024), a multi-country survey of six refugee sites in Uganda, Kenya, and Ethiopia conducted between 2016 and 2018. The survey captures refugees’ reports of de facto economic-rights implementation, dependence on humanitarian assistance, and outcomes including perceived security, local integration, trust in hosts, and exchanges of

food and goods with local residents.

I estimate individual-level models that interact humanitarian-aid dependence with perceived policy implementation to assess whether aid is more strongly associated with integration and security where economic rights are implemented in practice.³⁰ The results provide suggestive evidence consistent with the province- and country-level findings: where refugees report more inclusive implementation, aid dependence is associated with higher perceived security, stronger integration, greater trust in hosts, and more frequent exchanges of food and goods with local communities. These exchange outcomes directly align with the paper’s mechanism of economic spillovers and interdependence between refugees and host populations.

	Security	Integration	Trust	Exchange
Refugee Policy (De Facto)	1.646*** (0.130)	0.383*** (0.094)	0.476*** (0.072)	0.002 (0.097)
Dependence on Aid	−0.336* (0.173)	−0.378*** (0.121)	−0.197** (0.097)	−0.354*** (0.127)
Dependence on Aid x Refugee Policy (De Facto)	0.659** (0.271)	0.555*** (0.190)	0.261* (0.151)	0.344* (0.200)
Full Control	✓	✓	✓	✓
Location FE	✓	✓	✓	✓
Arrival Year FE	✓	✓	✓	✓
Group FE	✓	✓	✓	✓
Urban FE	✓	✓	✓	✓
Num. obs.	2167	1308	2107	1324
R ² (full model)	0.355	0.422	0.415	0.216
R ² (within)	0.142	0.058	0.061	0.072

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table 3: De Facto Refugee Policy, Aid, and Local Integration Outcomes

Mechanism Tests

Local Development

I argue that economically inclusive refugee policies reduce conflict by enabling aid to generate broad-based local economic gains in refugee-hosting areas. This spillover logic

³⁰See Appendix Section A.5 for details on operationalization and estimation strategy.

follows the established link between economic development and lower conflict risk, operating through reduced grievances and higher opportunity costs of violence (Collier and Hoeffler, 2004). Consistent with work showing that aid’s developmental effects depend on institutional context (Bearce and Tirone, 2010; Burnside and Dollar, 2000), I contend that refugee policy is a critical constraint shaping whether donor-funded interventions generate local economic improvements.

I first examine whether aid’s developmental effects in refugee-hosting provinces vary with policy inclusiveness. I then turn to project evaluation data to assess whether implementation performance for refugee-exposed projects shifts as refugee policy changes over the project cycle. The province-level outcomes provide the most direct evidence on the spillover process itself, while the project ratings offer a complementary diagnostic of the implementation environment in which that process unfolds.

Figure 3 plots the marginal effect of aid on two province-level proxies for economic development—GDP per capita aggregated from gridded data (Kummu et al., 2025) and night-time lights (Zhao et al., 2021)—across levels of policy inclusiveness, based on the instrumental-variable model specification.³¹

The estimated marginal effect of aid varies sharply with refugee policy. In provinces governed by restrictive policies, aid is associated with weaker—and in some specifications negative—development outcomes. Under more inclusive policies, by contrast, aid is strongly and positively related to both GDP growth and night-time lights. This divergence supports the proposed spillover mechanism: inclusive refugee policies appear to expand foreign aid’s capacity to generate local economic gains, whereas restrictive policies limit that impact.

³¹The estimates use fixed-effects specifications with the same shift-share IV approach as Table 1 to address endogenous aid allocation. Lagged conflict outcomes are included to ensure that economic improvements do not simply reflect prior reductions in violence. Appendix Section A.3.1 provides details.

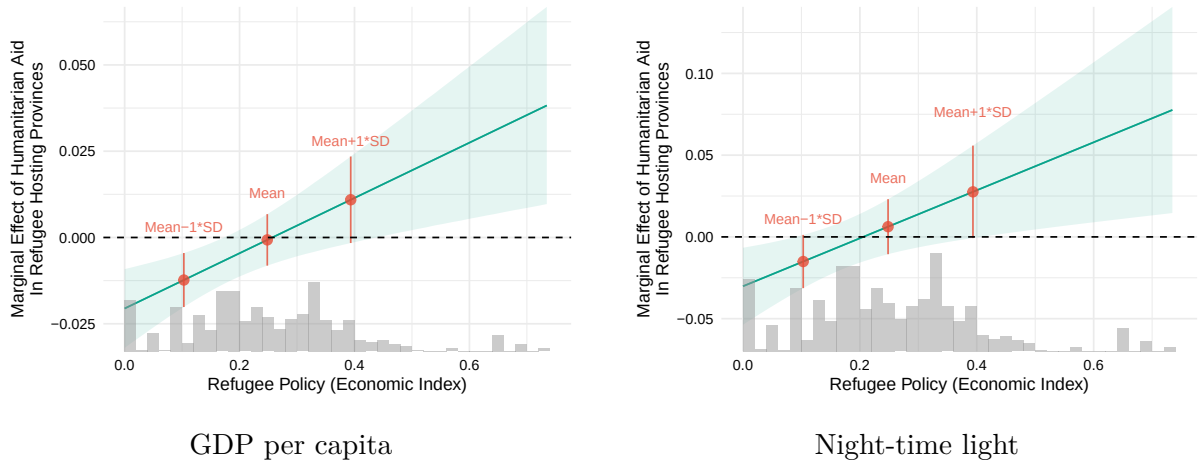


Figure 3: Foreign Aid, Refugee Policy, and Local Economic Development in Refugee-Hosting Provinces

Under restrictive policies, refugees face constraints on movement, employment, and access to services that restrict their ability to participate in local markets and to share in donor-supported economic activity. In such settings, aid may remain concentrated in camps or exacerbate distributional tensions with host communities, curbing its developmental effects. Inclusive policies, in contrast, expand opportunities for labor market integration and mobility, enabling broader participation in economic gains and fostering local conditions more conducive to peace.

Project Implementation Performance

I further triangulate the province-level evidence by examining whether World Bank projects with greater exposure to refugee-hosting provinces perform better, as reflected in higher project-evaluation ratings, when refugee policy liberalizes. If economic inclusion creates lower-grievance environments in which aid benefits both refugees and host communities, more exposed projects should face stronger local buy-in, fewer implementation frictions, and greater success in meeting their humanitarian or development goals (Isham and Kaufmann, 1999). World Bank ex post ratings do not directly measure spillovers,

but offer a complementary project-level diagnostic.

I analyze Implementation Completion Report Review (ICRR) ratings from the World Bank’s Independent Evaluation Group, which evaluate completed projects on a six-point scale from highly unsatisfactory to highly satisfactory (Isham and Kaufmann, 1999). Using geocoded project evaluation data (AidData, 2017), I compile nearly 1,560 projects in Global South countries evaluated between 1997 and 2016. For each project, exposure to refugee-hosting provinces is measured as the share of a project’s covered province-years in which refugees were present during implementation. Refugee-policy change is measured as the change in the national policy index between the project start and end years. I then test whether policy change is more strongly associated with ratings for more refugee-exposed projects, controlling for fixed effects, baseline covariates, and project characteristics.³²

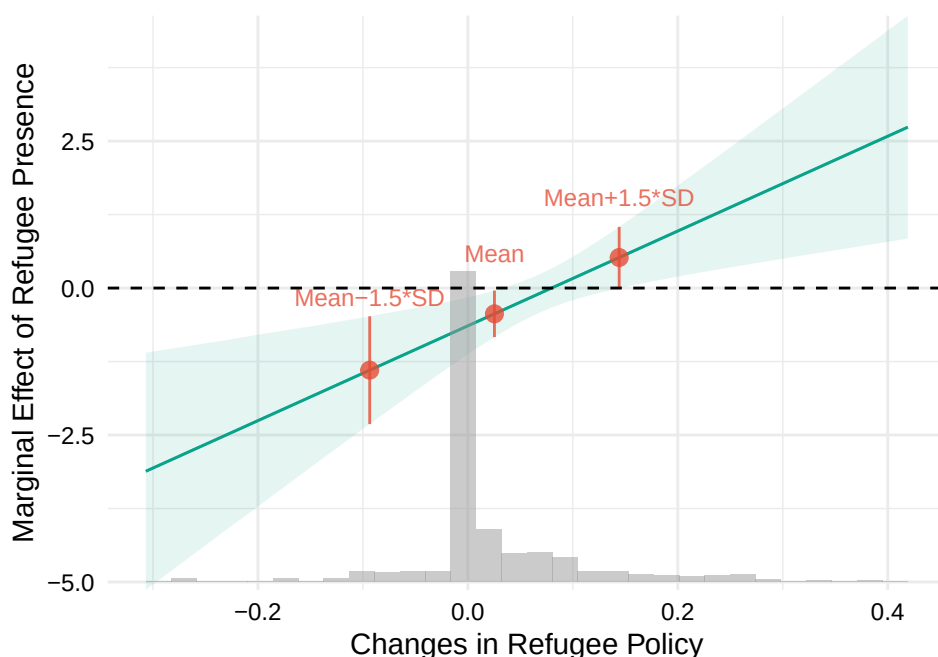


Figure 4: Refugee Policy Change and World Bank Project Performance for Refugee-Exposed Projects

Figure 4 provides suggestive evidence consistent with this expectation. Projects more

³²Table A3b reports full specifications.

exposed to refugee-hosting provinces receive lower performance ratings when refugee policy becomes more restrictive over the project cycle, but higher ratings when policy liberalizes.³³ This pattern is consistent with the claim that inclusive policies reduce political frictions and improve the implementation environment for projects exposed to refugee-hosting areas. Together with the province-level development results, these findings triangulate the paper’s central claim—that refugee policy conditions whether foreign aid produces the local conditions needed for conflict reduction.

Conclusion

Can foreign aid reduce conflict in areas that host refugees? This study shows that the answer depends on the policies adopted by host governments. While earlier research warned that aid could worsen tensions in refugee-hosting areas (Choi and Salehyan, 2013; Nunn and Qian, 2014), recent studies suggest that it can also promote peace by boosting local development (Taylor et al., 2016; Lehmann and Masterson, 2020; Zhou et al., 2023). My findings reconcile these mixed results. Using subnational data from across the Global South, I show that foreign aid reduces conflict only when countries adopt inclusive refugee policies, especially those that grant refugees economic rights. These policies enable local communities to share in the economic benefits of aid, mitigate intergroup grievances, and increase the opportunity costs of violence.

These results underscore the importance of the domestic policy context in shaping the effects of foreign aid. Much of the existing work on aid effectiveness focuses on regime type and overall institutional quality (Svensson, 2000; Burnside and Dollar, 2000). While political economy research has long emphasized that aid’s impact is conditioned by domestic institutions, this study shows that refugee policy plays a similarly important

³³Figure 4 is based on Column (3) of Table A3b.

moderating role. Rather than treating refugee governance as purely humanitarian, this study treats it as consequential for understanding political violence.

These findings also carry implications for policymakers and donors. First, improving refugee rights is not just normatively desirable; it also makes aid more effective. Restrictive policies, by contrast, limit the positive spillovers of aid and may even fuel instability. Second, simply increasing aid to countries with poor refugee policies is unlikely to generate meaningful local development or reduce conflict. Donors should support not only humanitarian assistance but also programs, through NGOs or civil society groups, that help refugees integrate into local communities. At the same time, governments that liberalize refugee policy without accompanying material support may see little benefit—and in some cases heightened tension—underscoring that policy reform and financial commitment function as complements rather than substitutes.

Finally, the study raises a cautionary note. Countries that have taken steps to liberalize refugee policies may struggle to sustain these efforts if international aid declines. In particular, shifting foreign policy priorities in major donor countries could significantly reduce assistance reaching refugee-hosting areas. This presents a serious challenge for governments that depend on external assistance to implement and maintain inclusive policies. As global power dynamics evolve, ensuring continued support for host communities will require sustained commitments from traditional donors as well as increased engagement by multilateral institutions such as UNHCR. Ultimately, inclusive refugee policies and stable international assistance are mutually reinforcing and both are essential for fostering peace in refugee-hosting regions.

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A Appendix

A.1 Descriptive Statistics

Var	Min	Max	Mean	Std.Dev	N
Year	1997	2016			20
Provinces					2405
Countries					124
WB Humanitarian Aid (lag = 2)	0	21.475	9.303	7.118	47757
Refugee Policy Index (lag = 3)	0	0.599	0.234	0.123	47757
Economic Policy Index (lag = 3)	0	0.735	0.266	0.145	47757
Non-Economic Policy Index (lag = 3)	0	0.804	0.25	0.146	47757
Civil Conflict (dummy)	0	1	0.078	0.269	47757
Civil Conflict (dummy, lag = 3)	0	1	0.079	0.27	47757
Refugee (dummy, lag = 3)	0	1	0.101	0.302	47757
Returnee (dummy, lag = 3)	0	1	0.004	0.063	47757
IDP (dummy, lag = 3)	0	1	0.006	0.078	47757
Population (logged, lag = 3)	0	19.094	10.881	5.419	47757
GDP per capita (lag = 3)	0	12.518	8.115	1.089	47757
Excluded Ethnicity (dummy, lag = 3)	0	1	0.543	0.498	47757
IDA Funding Position (lag = 3)	76.8	116.4	93.461	12.075	47757
Share of Aid (1995-1999)	0	1	0.039	0.089	47757

Table A1: Descriptive Statistics

A.2 Distribution of World Bank Aid in Refugee-Hosting Countries

Table A2 shows that although only 13.3% of provinces in refugee-hosting countries host refugees, these provinces tend to receive significantly more World Bank aid on average.

Aid Types	Without Refugees	With Refugees
World Bank	38.82	91.19
Education	7.83	28.65
Government & Civil Society	26.80	48.59
Energy	15.33	40.67
Health	7.30	29.61
Water & Sanitation	11.46	24.61
Trans & Comm	21.13	38.85
Agriculture	9.49	16.93
Food	0.30	2.32
Reconstruction	10.48	26.34

Table A2: Yearly Average World Bank Aid Receipt in Regions With and Without Refugees (in Millions of USD)

A.3 Mechanism Test

A.3.1 Economic Development

To evaluate the impact of foreign aid on local economic development, I use province-year measures of GDP per capita and night-time lights. I aggregate PPP-adjusted GDP

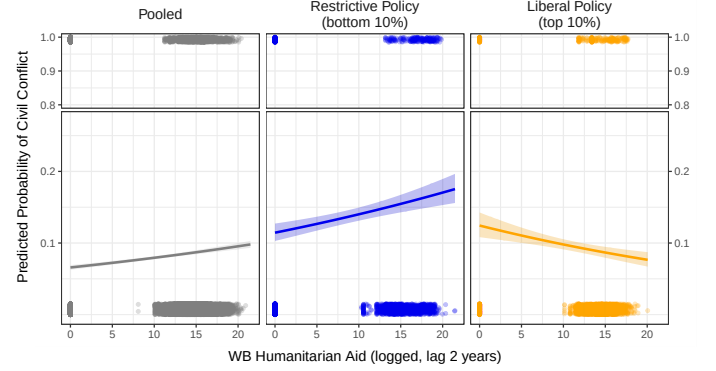


Figure A1: Correlation Between Aid and Civil Conflict by Refugee Policy Liberality

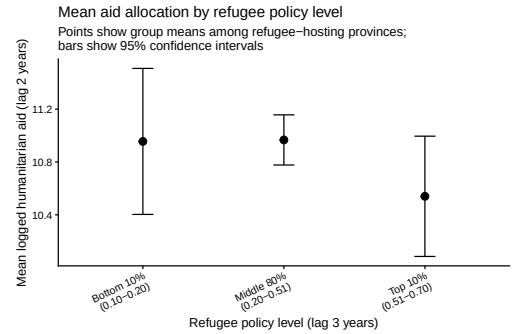


Figure A2: Aid Distribution by Refugee Policy Liberality

per capita data and night-time lights to the province-year level and log-transform both measures to reduce skewness. Table A3a reports the instrumental-variable specifications for the two proxies of economic development. The findings are consistent with theoretical expectations: in countries with inclusive refugee policies in the economic domain, foreign aid increases both night-time lights and local GDP per capita in refugee-hosting areas. The results remain stable after controlling for lagged conflict, suggesting that the observed development effects are unlikely to be driven by scenarios in which refugee policy first stabilizes local communities and only subsequently improves economic outcomes.

	Night-time Light		Local GDP	
	(1)	(2)	(3)	(4)
Aid	-0.019** (0.007)	-0.019** (0.007)	-0.007** (0.003)	-0.007** (0.003)
Aid x Policy	0.025 (0.028)	0.025 (0.028)	0.021 (0.015)	0.021 (0.015)
Aid x Refugee	-0.011 (0.010)	-0.011 (0.010)	-0.014** (0.006)	-0.014** (0.006)
Aid x Refugee x Policy	0.122*** (0.045)	0.121*** (0.045)	0.059** (0.023)	0.059** (0.023)
Refugee	0.193* (0.114)	0.191* (0.114)	0.142** (0.063)	0.142** (0.063)
Refugee x Policy	-1.455*** (0.529)	-1.449*** (0.529)	-0.659** (0.265)	-0.654** (0.265)
Conflict		-0.006 (0.005)		-0.004 (0.006)
Full Control	✓	✓	✓	✓
Province FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓
Num. obs.	45401	45401	47795	47795
R ² (full model)	0.958	0.958	0.993	0.993
R ² (within)	0.021	0.021	0.002	0.002

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

(a) Economic Development

	(1)	(2)	(3)	(4)
Refugee	-0.708** (0.294)	-0.681** (0.310)	-0.643** (0.295)	-0.570*** (0.196)
Policy Shift	-2.555* (1.443)	-2.794 (1.683)	-2.745 (1.974)	-2.423 (2.264)
Refugee x Policy Shift	6.924** (2.937)	8.425*** (2.985)	8.064** (3.232)	10.869* (5.570)
Full Control	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Country FE	✓	✓	✓	✓
Country FE x Year FE	✓	✓	✓	✓
Quadratic Trend			✓	✓
Year Specification	Eval. Year	Eval. Year	Eval. Year	Start Year
Sample	Full	Global South	Global South	Global South
Num. obs.	1560	1473	1473	1496
Num. Country	101	95	95	102
Num. Year	17	17	17	15
Num. Country x Year	475	448	448	447

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

(b) Aid Effectiveness

Table A3: Foreign Aid, Refugee Policy, and Economic Mechanisms

A.3.2 Project-Level Performance

The underlying logic for the project-level analysis is that for projects operating in provinces hosting refugees, shifts in refugee policy—toward either liberalization or restriction—should meaningfully shape their ability to achieve stated development goals. I estimate the following model using fixed-effects OLS specifications:

$$\mathbf{IEG\ Satisfactory}_{i,c} = \beta_0 + \beta_1 \mathit{Refugee}_{i,c} \times \mathit{PolicyShift}_c + \beta_2 \mathit{Refugee}_{i,c} + \beta_3 \mathit{PolicyShift}_c + \beta_4 \mathit{X}_{i,c, \mathit{StartYear}} + \tau_{\mathit{EvalYear}} + \epsilon_{i,c}$$

where the outcome is the IEG performance rating for project i in the recipient country c . Project-level refugee exposure, $Refugee_{i,c}$, is measured as the proportion of province-years covered by the project that have hosted refugees. Refugee-policy change, $PolicyShift_c$, is defined as the difference between the country’s policy score at the end and start of the project cycle ($PolicyShift_{c,EndYear} - PolicyShift_{c,StartYear}$). I control for the full set of covariates used in the main analysis and include exposure to conflict at the start year to avoid post-treatment bias. Models also incorporate country-by-year fixed effects and are weighted by project size (aid volume). The coefficient, β_1 , captures the moderating role of refugee policy on project performance and is expected to be positive.

Table A3b presents the results. Models 1–3 use OLS with country-by-year fixed effects to absorb recipient-country shocks at the time of project evaluation. Model 1 includes all projects with IEG evaluations, while Models 2–3 restrict the sample to Global South countries in the main analysis. Model 4 instead defines fixed effects by project initiation year. Across specifications, aid project performance improves in refugee-hosting areas when governments liberalize refugee policies.

A.4 Economic and Non-Economic Refugee Policies

The theory suggests that inclusive refugee policies reduce conflict by strengthening the economic effects of foreign aid, especially when they expand refugees’ economic rights rather than political rights or legal status. Because economic-rights components may operate jointly, I construct an inverse-covariance weighted summary index of the policy components most directly tied to refugees’ economic incorporation and local development. The DWRAP dataset codes 68 refugee policy indicators, grouped into 5 thematic categories. From these, I construct two indices: (1) an economic refugee policy index, based on categories covering livelihoods, services (e.g., aid, health and education), and

movement; and (2) a non-economic index, based on categories related to legal status, and political participation. Table A4 provides a detailed illustration of how the economic and non-economic policy indices are constructed.

Economic Refugee Policies			Non-Economic Refugee Policies		
Generated Using Inverse-Covariance Weighted (ICW) Summary Indices					
Livelihoods	Property Employment	Land	Access	Control Family	Status Recourse
Services	Aid Health	Education	Participation	Citizenship Political Rights	
Movement	Settlement	Documents			

Table A4: Generating Economic and Non-Economic Policies

Table A5 reports estimates of the conditional effect of foreign aid on conflict using the non-economic refugee rights index (Columns 5-8) and the general policy index (Columns 1-4) from the DWRAP database. Across the OLS and instrumental-variable models, the results are consistent: while the main analysis shows that foreign aid significantly reduces civil conflict in provinces governed by inclusive economic refugee policies, this effect does not extend to non-economic dimensions of refugee policy. This suggests that economic incorporation—rather than legal status or recognition alone—facilitates the developmental spillovers through which aid reduces violence. Moreover, the moderating effect of the general policy index is negatively significant though much smaller across the IV specifications, indicating that broad refugee-policy liberalization may be less effective than targeted liberalization of refugees’ economic rights.

Finally, I examine how subcomponents of liberal economic rights shape the effect of foreign humanitarian aid on conflict. Columns 1–6 of Table A6 report the moderating effects of three types of economic policies: freedom of movement, including encampment requirements and restrictions on mobility (Columns 1-2); livelihoods, including property and labor rights (Columns 3-4); and public services, including access to aid, education,

	General Policy Index				Non-Economic Policy			
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
Aid	0.000 (0.001)	0.000 (0.001)	-0.004 (0.003)	-0.003 (0.003)	-0.001 (0.001)	-0.001 (0.000)	-0.003 (0.002)	-0.003 (0.002)
Aid x Policy	-0.002 (0.003)	-0.002 (0.003)	0.024 (0.017)	0.020 (0.016)	0.001 (0.003)	0.000 (0.003)	0.021 (0.015)	0.018 (0.014)
Aid x Refugee	0.004* (0.002)	0.004* (0.002)	0.005 (0.007)	0.004 (0.007)	0.002 (0.002)	0.002 (0.002)	0.001 (0.006)	0.000 (0.006)
Aid x Refugee x Policy	-0.013* (0.008)	-0.012* (0.007)	-0.052* (0.029)	-0.045* (0.027)	-0.006 (0.007)	-0.005 (0.007)	-0.030 (0.026)	-0.025 (0.023)
Refugee	-0.018 (0.035)	-0.020 (0.033)	-0.048 (0.085)	-0.035 (0.081)	-0.009 (0.031)	-0.011 (0.029)	-0.008 (0.082)	-0.001 (0.077)
Refugee x Policy	0.108 (0.119)	0.106 (0.114)	0.613* (0.359)	0.526 (0.330)	0.068 (0.111)	0.066 (0.105)	0.399 (0.348)	0.346 (0.316)
Conflict		0.084*** (0.016)		0.084*** (0.016)		0.085*** (0.016)		0.085*** (0.016)
Full Control	✓	✓	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Num. obs.	47935	47935	47935	47935	47795	47795	47795	47795
R ² (full model)	0.604	0.607	0.604	0.607	0.604	0.607	0.604	0.607
R ² (within)	0.001	0.009	0.001	0.008	0.001	0.008	0.001	0.008

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table A5: Foreign Aid, Refugee Policy Components and Armed Conflict

and healthcare (Columns 5-6). The instrumental-variable specifications show a consistent conflict-mitigating effect of foreign aid when host governments adopt liberal refugee policies on livelihoods and public services. By contrast, the effect is smaller and statistically insignificant for policies regulating freedom of movement, although the coefficient remains consistently negative.

One possible explanation is that compared to liberal refugee rights that facilitate their economic integration, the local benefit of allowing their freedom of movement is mixed. On one hand, it may foster social integration; on the other, it may increase exposure to forced recruitment or heighten perceived cultural threat among host populations. Moreover, policies regulating encampment and movement may also shape the location of refugee settlements and the distribution of aid projects. First, host countries that confine refugees to camps may establish settlements in border areas that are more vulnerable to conflict spillovers from countries of origin. Second, the concentration of camp settlements may attract greater aid resources. These patterns may generate an upward bias in the relationship between aid, refugee presence, and conflict, offsetting the underlying

conflict-mitigating effect. To address this concern, I construct an alternative economic policy index that excludes the camp-requirement component. The instrumental-variable specifications in Columns 7-8 show findings consistent with the main model, indicating that the results are not driven by this potential source of bias.

	Movement		Livelihoods		Services		Alternative Measure	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Aid	-0.004 (0.003)	-0.003 (0.003)	-0.001 (0.004)	-0.001 (0.003)	-0.001 (0.003)	-0.001 (0.003)	-0.001 (0.003)	-0.001 (0.003)
Aid x Policy	0.011 (0.008)	0.009 (0.007)	0.005 (0.008)	0.002 (0.008)	0.009 (0.014)	0.006 (0.013)	0.007 (0.013)	0.004 (0.012)
Aid x Refugee	0.007 (0.007)	0.006 (0.007)	0.003 (0.007)	0.003 (0.006)	0.004 (0.006)	0.004 (0.006)	0.006 (0.007)	0.005 (0.006)
Aid x Refugee x Policy	-0.032* (0.019)	-0.027 (0.017)	-0.058* (0.030)	-0.052* (0.027)	-0.046* (0.025)	-0.042* (0.023)	-0.064** (0.028)	-0.058** (0.026)
Refugee	-0.048 (0.097)	-0.034 (0.091)	-0.022 (0.080)	-0.017 (0.075)	-0.041 (0.075)	-0.036 (0.071)	-0.053 (0.080)	-0.046 (0.075)
Refugee x Policy	0.336 (0.242)	0.286 (0.218)	0.654* (0.350)	0.589* (0.326)	0.562* (0.302)	0.517* (0.281)	0.754** (0.327)	0.691** (0.309)
Conflict		0.084*** (0.016)		0.084*** (0.016)		0.084*** (0.016)		0.084*** (0.016)
Full Control	✓	✓	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Num. obs.	47935	47935	47935	47935	47935	47935	47795	47795
R ² (full model)	0.604	0.607	0.604	0.607	0.604	0.607	0.604	0.607
R ² (within)	0.001	0.008	0.001	0.008	0.001	0.008	0.001	0.008

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table A6: The Moderating Effect of Refugee Policies on Movement, Livelihoods and Public services

A.5 De Jure Versus De Facto Refugee Policies

In this section, I use Betts et al. (2024)’s cross-national survey data to capture implementation on the ground. The data provides individual-level measures of perceived refugee-policy practices across refugee-hosting sites in Uganda, Kenya, and Ethiopia from 2016 to 2018., which allows me to conduct a detailed individual-level comparison of how *de jure* and *de facto* refugee policies moderate the effect of aid on individual-level integration outcomes across countries and settlement contexts.

The estimation strategy is as follows. The outcome variable, *Integration Outcome*_{*i,c,g,t*}, is measured at the individual-country-group-year level. I focus on four proxies for refugee

integration: (1) perceived security, (2) the perceived degree of local integration, (3) the extent to which hosts can be trusted, and (4) the frequency of meal exchange. The first three are measured on a four-point ordinal scale, with 4 indicating the highest level of security and integration. The fourth is a behavioral measure capturing the extent to which refugees share meals and daily goods with locals. The coefficient β_1 captures the moderating effect of interest, $Aid_{i,c,g,t} \times Policy_{i,c,g,t}$. $Policy_{i,c,g,t}$ measures perceived *de facto* policy practice in three domains: freedom of movement, opportunities to work, and permission to buy goods and access markets. I aggregate these items into an ICW summary index consistent with the main analysis. $Aid_{i,c,g,t}$ captures refugees' individual-level dependence on external assistance from the WFP and UNHCR, operationalized as the share of humanitarian aid in their annual income. I also control for individual covariates, including age, religion, gender, employment status, total assets, years of arrival and pre-displacement exposure to violence. Finally, $\mu_{c,g,t}$ denotes settlement-by-year-by-group fixed effects, which absorb country-year differences in *de jure* policy environments and capture settlement- and group-specific variation in how these policies are implemented across urban and camp settings.

$$\text{Integration Outcome}_{i,c,g,t} = \beta_0 + \beta_1 Aid_{i,c,g,t} \times Policy_{i,c,g,t} + \beta_2 Aid_{i,c,g,t} + \beta_3 Policy_{i,c,g,t} + \beta_4 X_{i,c,g,t} + \mu_{c,g,t} + \epsilon_{i,c,g,t}$$

A.6 Alternative Measurement and Alternative Sample

First, I replicate the main analysis using total World Bank committed aid, development aid, humanitarian aid disbursements, as well as a continuous measure of refugee presence using the per-1,000-capita share of refugee camps. The results in Table A7 remain consistent across the IV specifications.

Second, I replicate the analysis using two alternative thresholds (> 0 and ≥ 25)

	Total Aid Projects		Developmental Aid		Aid Disbursement		# of Refugee Settlements (per 1000 pop.)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Aid	-0.003 (0.004)	-0.003 (0.004)	0.001 (0.003)	0.001 (0.003)	-0.004 (0.004)	-0.003 (0.004)	-0.002 (0.003)	-0.001 (0.003)
Aid x Policy	0.012 (0.014)	0.009 (0.013)	0.008 (0.010)	0.006 (0.010)	0.017 (0.015)	0.013 (0.014)	0.012 (0.013)	0.009 (0.012)
Aid x Refugee	0.015 (0.010)	0.013 (0.010)	0.017 (0.012)	0.015 (0.012)	0.014* (0.008)	0.012 (0.008)	0.795*** (0.211)	0.827*** (0.272)
Aid x Refugee x Policy	-0.089** (0.037)	-0.079** (0.036)	-0.094* (0.049)	-0.085* (0.048)	-0.082** (0.032)	-0.072** (0.030)	-3.326*** (1.230)	-3.933** (1.704)
Refugee	-0.185 (0.145)	-0.160 (0.142)	-0.201 (0.162)	-0.181 (0.158)	-0.126 (0.086)	-0.109 (0.084)	-5.717** (2.186)	-6.390** (3.009)
Refugee x Policy	1.167** (0.514)	1.040** (0.499)	1.161* (0.655)	1.058* (0.630)	0.858** (0.356)	0.763** (0.338)	25.197 (15.871)	32.904 (21.264)
Conflict		0.084*** (0.016)		0.084*** (0.016)		0.084*** (0.016)		0.083*** (0.016)
Full Control	✓	✓	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Num. obs.	47795	47795	47795	47795	47795	47795	47795	47795
R ² (full model)	0.604	0.607	0.604	0.607	0.604	0.607	0.604	0.607
R ² (within)	0.001	0.008	0.001	0.009	0.001	0.009	0.002	0.010

***p < 0.01; **p < 0.05; *p < 0.1

Table A7: Alternative Aid Measure and Continuous Refugee Presence

of battle deaths for the binary conflict indicators as well as two continuous outcome variables: battle deaths and the count of conflict events.³⁴ Table A8 present the results from the instrumental-variable specifications with and without lagged conflict outcomes. The findings remain consistent: aid continues to exert a pacifying effect in refugee-hosting provinces under inclusive refugee policies.

Third, the theory also expects inclusive refugee policies to reduce intergroup tensions by enabling aid benefits to spill over from refugees to local communities. Columns 1-4 in Table A9 test this implication using alternative outcomes related to communal and social conflict.³⁵ Columns 1–2 use UCDP non-state violence, while Columns 3–4 use ACLED social conflict. Across the 2SLS specifications, foreign aid consistently reduces both forms of intergroup tension under inclusive refugee policies.

Finally, to address the identification concern that some countries maintained consis-

³⁴I take the natural log of both measures to reduce skewness. Because the conflict outcomes include zero values, I use the log-plus-one transformation throughout.

³⁵For communal violence, I use a dummy for any non-state violence event in UCDP. Because UCDP applies a 25-battle-death threshold at the conflict-dyad level, it may miss smaller-scale localized conflict. I therefore use ACLED to construct an alternative measure based on battles between non-state actors, such as political and social militias, which closely correspond to UCDP’s category of “non-state violence.”

	> 0		≥ 25		Battle Deaths (Logged)		Conflict Events (Logged)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Aid	-0.003 (0.004)	-0.002 (0.004)	-0.002 (0.003)	-0.002 (0.002)	-0.012 (0.013)	-0.008 (0.011)	-0.012 (0.008)	-0.008 (0.006)
Aid x Policy	0.010 (0.015)	0.006 (0.014)	0.007 (0.011)	0.003 (0.009)	0.034 (0.053)	0.011 (0.044)	0.034 (0.032)	0.012 (0.025)
Aid x Refugee	0.010 (0.007)	0.009 (0.007)	0.011* (0.007)	0.009 (0.006)	0.045 (0.032)	0.030 (0.031)	0.031 (0.021)	0.016 (0.017)
Aid x Refugee x Policy	-0.061** (0.028)	-0.055** (0.027)	-0.055** (0.026)	-0.046* (0.025)	-0.311** (0.130)	-0.243** (0.119)	-0.197** (0.086)	-0.125* (0.066)
Refugee	-0.104 (0.100)	-0.094 (0.097)	-0.096 (0.071)	-0.077 (0.069)	-0.378 (0.382)	-0.250 (0.373)	-0.269 (0.235)	-0.129 (0.202)
Refugee x Policy	0.728** (0.360)	0.671* (0.349)	0.577** (0.281)	0.481* (0.267)	3.446** (1.507)	2.722* (1.393)	2.155** (0.971)	1.366* (0.771)
Conflict		0.068*** (0.011)		0.101*** (0.022)		0.143*** (0.024)		0.235*** (0.032)
Full Control	✓	✓	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Num. obs.	47795	47795	47795	47795	47795	47795	47795	47795
R ² (full model)	0.635	0.637	0.570	0.575	0.683	0.689	0.740	0.754
R ² (within)	0.001	0.006	0.002	0.013	0.002	0.023	0.003	0.058

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table A8: Alternative Measurements of Conflict

tently restrictive or inclusive refugee policies, I replicate the analysis using two restricted samples: 1) policy switchers, defined as countries that changed their refugee policies at least once; and 2) ever-liberalizers, defined as countries that improved refugees' economic rights at least once. Instrumental-variable results reported in Columns 5-8 in Table A9 remain consistent with the main findings.

	Non-State Conflict		Social Conflict		Switchers Only		Liberalizers Only	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Aid	0.000 (0.002)	0.000 (0.002)	-0.013 (0.010)	-0.013 (0.013)	-0.002 (0.004)	-0.001 (0.004)	-0.001 (0.004)	-0.001 (0.004)
Aid x Policy	0.004 (0.008)	0.003 (0.008)	0.063 (0.053)	0.063 (0.064)	0.014 (0.014)	0.010 (0.012)	0.016 (0.014)	0.012 (0.013)
Aid x Refugee	0.007 (0.006)	0.006 (0.006)	0.039** (0.017)	0.035** (0.016)	0.006 (0.009)	0.005 (0.009)	0.005 (0.009)	0.004 (0.008)
Aid x Refugee x Policy	-0.038* (0.022)	-0.036* (0.021)	-0.133* (0.079)	-0.145* (0.082)	-0.061* (0.031)	-0.054* (0.030)	-0.062** (0.031)	-0.054* (0.030)
Refugee	-0.074 (0.075)	-0.067 (0.073)	-0.385* (0.203)	-0.375* (0.197)	-0.060 (0.109)	-0.047 (0.107)	-0.048 (0.105)	-0.035 (0.103)
Refugee x Policy	0.443* (0.264)	0.421 (0.257)	1.422 (1.029)	1.684 (1.060)	0.729* (0.372)	0.648* (0.360)	0.735* (0.370)	0.645* (0.357)
Conflict		0.039*** (0.014)		-0.032** (0.012)		0.073*** (0.019)		0.080*** (0.019)
Full Control	✓	✓	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Num. obs.	47795	47795	16301	13306	37911	37911	35422	35422
R ² (full model)	0.443	0.444	0.484	0.490	0.620	0.622	0.615	0.617
R ² (within)	0.000	0.002	0.003	0.003	0.001	0.007	0.001	0.008

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table A9: Alternative Violence Outcomes and Policy Shift

A.7 Instrumental Variable Approach

A.7.1 First Stage Estimations

The main model in Table 1 contains four endogenous variables “*Aid*”, “*Aid* × *Refugee*”, “*Aid* × *Policy*” and “*Aid* × *Refugee* × *Policy*”. To address the issue of endogenous interaction terms, I incorporate the instrumental variable $Instrument_{i,c,t-2} = IDA \text{ Funding Position}_{t-3} \times Aid \text{ Share}_{i,c}^{1995-1999}$, along with its pairwise and triple interaction terms with refugee presence and refugee policy, into the first-stage estimation. As a result, the first stage encompasses four models:

$$\begin{aligned} Aid_{i,c,t-2} = & \beta_0 + \beta_1 Instrument_{i,c,t-2} + \beta_2 Instrument_{i,c,t-2} \times Refugee_{i,c,t-3} + \\ & \beta_3 Instrument_{i,c,t-2} \times Policy_{c,t-3} + \beta_4 Instrument_{i,c,t-2} \times Refugee_{i,c,t-3} \times Policy_{c,t-3} + \\ & \beta_8 X_{i,c,t-3} + \mu_{i,c} + \tau_t + \epsilon_{i,c,t} \end{aligned}$$

$$\begin{aligned} Aid_{i,c,t-2} \times Refugee_{i,c,t-3} = & \beta_0 + \beta_1 Instrument_{i,c,t-2} + \beta_2 Instrument_{i,c,t-2} \times Refugee_{i,c,t-3} + \\ & \beta_3 Instrument_{i,c,t-2} \times Policy_{c,t-3} + \beta_4 Instrument_{i,c,t-2} \times Refugee_{i,c,t-3} \times Policy_{c,t-3} + \\ & \beta_8 X_{i,c,t-3} + \mu_{i,c} + \tau_t + \epsilon_{i,c,t} \end{aligned}$$

$$\begin{aligned} Aid_{i,c,t-2} \times Policy_{c,t-3} = & \beta_0 + \beta_1 Instrument_{i,c,t-2} + \beta_2 Instrument_{i,c,t-2} \times Refugee_{i,c,t-3} + \\ & \beta_3 Instrument_{i,c,t-2} \times Policy_{c,t-3} + \beta_4 Instrument_{i,c,t-2} \times Refugee_{i,c,t-3} \times Policy_{c,t-3} + \\ & \beta_8 X_{i,c,t-3} + \mu_{i,c} + \tau_t + \epsilon_{i,c,t} \end{aligned}$$

$$\begin{aligned} Aid_{i,c,t-2} \times Refugee_{i,c,t-3} \times Policy_{c,t-3} = & \beta_0 + \beta_1 Instrument_{i,c,t-2} + \beta_2 Instrument_{i,c,t-2} \times Refugee_{i,c,t-3} + \\ & \beta_3 Instrument_{i,c,t-2} \times Policy_{c,t-3} + \beta_4 Instrument_{i,c,t-2} \times Refugee_{i,c,t-3} \times Policy_{c,t-3} + \\ & \beta_8 X_{i,c,t-3} + \mu_{i,c} + \tau_t + \epsilon_{i,c,t} \end{aligned}$$

The shift variable, $IDA \text{ Funding Position}_{t-3}$, is calculated as current available funding divided by total promised funding in a given year: $\frac{Currently \text{ Available Budget}_{t-3}}{Undisbursed \text{ Committed Funding}_{t-3}}$.

The province-level exposure share, $Aid \text{ Share}_{i,c}^{1995-1999}$, is a time-invariant measure of each province’s baseline propensity to receive aid, calculated as the share of committed aid allocated to province i in country c among total aid commitments during the first five years of available aid data: $\frac{\sum_{1995}^{1999} Aid_{i,c,t}}{\sum_{1995}^{1999} TotalAid_t}$. This approach ensures that endogenous future aid allocations are not incorporated into the share component. The direct effect of IDA funding position is absorbed by year fixed effects, while the time-invariant share variable,

$Aid\ Share_{i,c}^{1995-1999}$, is absorbed by province fixed effects. Identification therefore comes from the interaction between baseline subnational aid exposure and annual IDA liquidity shocks, rather than from either component alone. $X_{i,c,t-3}$ includes all controls in the main model, as well as $Refugee_{i,c,t-3}$, $Policy_{c,t-3}$, and their interaction term. Table A10 displays the first-stage results for the Two-Stage Least Squares model estimations with and without lagged conflict outcomes. The directions of the effects align with theoretical expectations: a location receives more aid when it has had greater prior exposure to aid and when IDA allocates a larger volume of funding in a given year.

	IV (Without Lagged DV)				IV (With Lagged DV)			
	Aid	Aid × Policy	Aid × Refugee	Aid × Policy × Refugee	Aid	Aid × Policy	Aid × Refugee	Aid × Policy × Refugee
Aid Share x IDA Position	0.547*** (0.081)	0.096*** (0.020)	0.044** (0.021)	0.010 (0.006)	0.547*** (0.081)	0.096*** (0.020)	0.044** (0.021)	0.010 (0.006)
Aid Share x IDA Position x Refugee	-0.016 (0.041)	0.015 (0.011)	0.198*** (0.045)	0.013 (0.010)	-0.015 (0.041)	0.015 (0.012)	0.198*** (0.045)	0.013 (0.010)
Aid Share x IDA Position x Policy	-0.033 (0.083)	0.111*** (0.037)	-0.015 (0.018)	-0.012* (0.006)	-0.032 (0.083)	0.111*** (0.037)	-0.015 (0.018)	-0.012* (0.006)
Aid Share x IDA Position x Policy x Refugee	0.134 (0.118)	-0.051 (0.041)	-0.243** (0.117)	0.061* (0.035)	0.132 (0.119)	-0.052 (0.042)	-0.243** (0.117)	0.061* (0.035)
Conflict					-0.131 (0.170)	-0.066 (0.066)	0.009 (0.060)	-0.012 (0.018)
F Statistics	295.965	142.970	456.572	244.882	296.069	143.404	456.565	244.614
Full Control	✓	✓	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Num. obs.	47795	47795	47795	47795	47795	47795	47795	47795
Adj. R ² (full model)	0.786	0.818	0.862	0.875	0.786	0.818	0.862	0.875
Adj. R ² (proj model)	0.025	0.013	0.661	0.715	0.025	0.013	0.661	0.715

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table A10: First Stage Results

A.7.2 Validating the Instrumental Variable Strategy

To strengthen the credibility of the exclusion restriction, I implement several complementary diagnostics and robustness checks.

The shift-share instrument interacts a province's historical likelihood of receiving IDA aid (the share) with IDA liquidity in year $t-3$ (the shift). The identifying assumption is that historical aid exposure proxies the location's potential to receive aid in the future, while year-to-year variation in donor liquidity is exogenous to contemporary conflict dynamics. This implies that, conditional on controls and fixed effects, the instrument affects

conflict only through its influence on local aid allocation.

To assess this assumption, I conduct four diagnostic tests:

1. **Exogeneity of the Shock:** A crucial assumption of the shift-share instrument is that the IV effect captures the actual exogenous shock from World Bank IDA liquidity, rather than long-term trends in the difference between areas with high and low values of the share variable, i.e., the selection into aid allocation. If the shift variable is a one-time shock, such as camp closure or border fortification, then this assumption is equivalent to the common-trend assumption in a DID design: there is no significant difference between treated and non-treated groups before the shock takes place. However, because the shift variable in this study is the time-varying liquidity of World Bank IDA, I instead apply Gehring et al. (2022)’s sensitivity test to ensure that there is no overlap between i) the trend in differences in conflict outcomes between areas with below- and above-average aid shares and ii) the trend in the IDA funding position. Figure A3 presents the results. The blue curve shows the yearly trend of the exogenous shift variable, “IDA funding position.” Panel (a) illustrates a fabricated problematic scenario in which the validity of the IV would be violated: the trend in the High–Low aid recipients’ difference in battle outcomes aligns perfectly with the trend in IDA positions. Panel (b) shows that the actual trends in battle deaths do not follow this pattern; the two curves diverge substantially. Panels (c) and (d) further split the sample by inclusive versus restrictive refugee policies and by the presence or absence of refugees. In none of these cases do the curves overlap with the shift variable. This evidence supports the assumption that the share component is not confounded by differential conflict trajectories.

2. **Restricted Samples and Alternative Baseline Years:** Because the baseline aid share is constructed using aid allocations from 1995–1999, one concern is that the years used to

construct the share overlap with the early years of the analysis. To address this concern, I conduct two sensitivity tests. Columns 1 and 2 of Table A11 replicate the main analysis using a restricted sample that excludes observations from the baseline-share period and retains only post-1999 observations. Columns 3 and 4 instead construct the share using a shorter 1995–1997 baseline window, ensuring that the exposure measure is based on aid allocation prior to the main analysis period. The results remain substantively consistent across these specifications, providing further reassurance that the share component is not inadvertently capturing future aid allocation patterns or endogenous responses to later conflict dynamics.

3. *Alternative Instrument Specification:* To assess the robustness of the shift–share design, I re-estimate the model using an alternative instrument that replaces the time-invariant baseline aid share with a time-varying measure of each province’s aid propensity. Following Gehring et al. (2022), I define this measure as the cumulative share of years in which province i in country c received aid prior to year t : $Aid\ Prob_{i,c,t} =$

$$\frac{\sum_{s=1995}^{t-1} ReceivingAid_{i,c,s}}{t-1995}.$$

The logic is that provinces with a stronger history of aid receipt are more likely to receive aid in subsequent years. After controlling for this time-varying aid propensity, the interaction between aid propensity and IDA funding shocks provides an alternative source of plausibly exogenous variation in aid exposure. Columns 5 and 6 of Table A11 report the results. The estimates remain consistent with the main findings, strengthening confidence that the results are not driven by the specific construction of the baseline share component.

4. *Temporal Placebo Tests:* Finally, I conduct a temporal placebo test to further assess the validity of the instrumental variable strategy. The logic is that if the instrument

	Restricted Sample		Shorter Window		Alternative IV	
	(1)	(2)	(3)	(4)	(5)	(6)
Aid	-0.003 (0.004)	-0.002 (0.004)	-0.002 (0.004)	-0.002 (0.004)	0.001 (0.004)	0.001 (0.004)
Aid x Policy	0.019 (0.016)	0.017 (0.015)	0.010 (0.013)	0.008 (0.012)	0.003 (0.006)	0.003 (0.006)
Aid x Refugee	0.011 (0.007)	0.010 (0.007)	0.011 (0.007)	0.008 (0.007)	0.007* (0.004)	0.007* (0.004)
Aid x Refugee x Policy	-0.069*** (0.025)	-0.062** (0.025)	-0.078** (0.031)	-0.067** (0.029)	-0.030* (0.016)	-0.027* (0.016)
Refugee	-0.100 (0.093)	-0.087 (0.093)	-0.098 (0.087)	-0.078 (0.086)	-0.058 (0.055)	-0.055 (0.052)
Refugee x Policy	0.772** (0.300)	0.700** (0.295)	0.881** (0.369)	0.761** (0.346)	0.307 (0.204)	0.286 (0.194)
Conflict		0.056*** (0.016)		0.084*** (0.016)		0.084*** (0.016)
Full Control	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓	✓
Num. obs.	40580	40580	47795	47795	47795	47795
R ² (full model)	0.586	0.587	0.559	0.562	0.559	0.562
R ² (within)	0.001	0.005	0.001	0.008	0.001	0.008

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table A11: Sensitive Tests for the Instrumental Variable

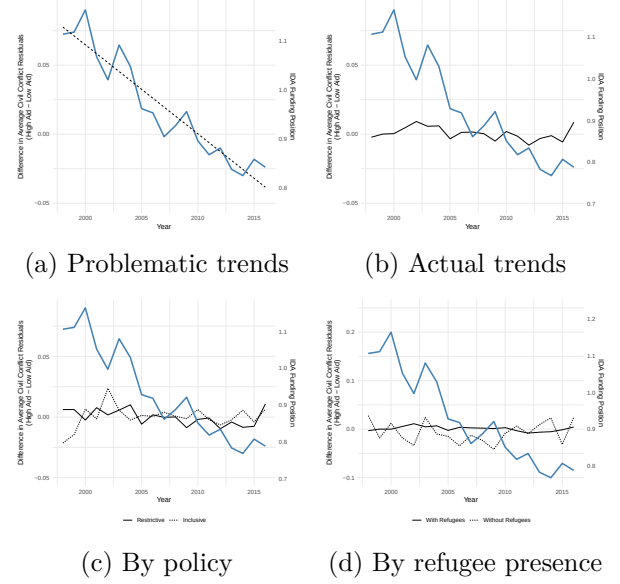


Figure A3: Conflict Trends Between Areas With Low and High Shares of Aid

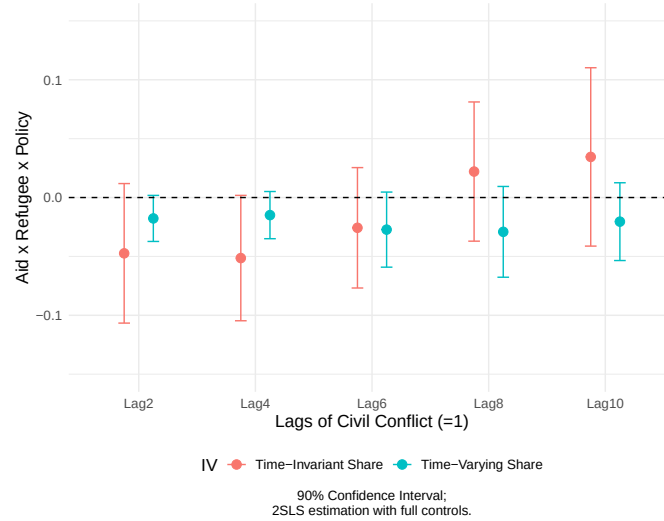


Figure A4: Temporal Placebo Tests

is fully exogenous and not confounded by other factors, it should not affect outcomes that occurred prior to aid allocation. Figure A4 presents the IV estimates of the triple interaction term $Aid_{i,c,t-2} \times Refugee_{i,c,t-3} \times Policy_{c,t-3}$ using both the time-invariant and time-varying share instruments, with civil conflict lagged by 2, 4, 6, 8, and 10 years as the outcome variable. None of the estimates are statistically distinguishable from zero, providing additional support for the validity of the instrumental variable specification.

Together, these checks provide converging support for the validity of the instrument. While the exclusion restriction cannot be directly tested, the evidence indicates that the instrument is unlikely to be systematically correlated with unobserved conflict dynamics. This bolsters confidence that the variation used for identification is plausibly exogenous—driven by changes in donor capacity rather than by local conflict conditions or strategic aid allocation.

A.8 Multi-level Model Specifications and Country-Level Controls

In this section, I estimate a multilevel mixed-effects model that accounts for the hierarchical structure of the data. The model includes a country-level random slope for refugee policy, allowing its moderating effect to vary across countries. To avoid overfitting and convergence problems, I include random intercepts for provinces and years, which capture unit-level differences and common temporal shocks. This framework allows for partial pooling, separates within- and between-country variation in policy effects, and improves the precision of the interaction estimates.

$$\begin{aligned}
\mathbf{Violence}_{i,c,t} = & \beta_0 + \beta_1 Aid_{i,c,t-2} + \beta_2 Refugee_{i,c,t-3} + \beta_3 Policy_{c,t-3} \\
& + \beta_4 Policy_{c,t-3} \times Refugee_{i,c,t-3} + \beta_5 Aid_{i,c,t-2} \times Policy_{c,t-3} \\
& + \beta_6 Aid_{i,c,t-2} \times Refugee_{i,c,t-3} \\
& + \beta_7 Aid_{i,c,t-2} \times Refugee_{i,c,t-3} \times Policy_{c,t-3} \\
& + X'_{i,c,t-3} \beta_8 + Z'_{c,t-3} \beta_9 \\
& + \zeta_c + u_{i(c)} + \lambda_t + b_c Policy_{c,t-3} + \epsilon_{i,c,t}.
\end{aligned}$$

Compared with the baseline two-way fixed-effects model, the multilevel specification accounts for persistent across-province differences, common year-level shocks, time-invariant across-country differences, and cross-country heterogeneity in the association between refugee policy and conflict. Specifically, ζ_c is a country-level random intercept, $u_{i(c)}$ is a province-level random intercept nested within country, and λ_t is a year-level random intercept. The term $b_c Policy_{c,t-3}$ allows the refugee-policy effect to vary across countries. All random effects are assumed to follow mean-zero normal distributions.

	Province Controls				Province + Country Controls			
	MLM	MLM	MLM-IV	MLM-IV	MLM	MLM	MLM-IV	MLM-IV
Aid	0.000 (0.000)	0.000 (0.000)	0.003* (0.002)	0.002 (0.002)	0.001* (0.000)	0.000 (0.000)	0.002 (0.002)	0.000 (0.001)
Aid x Policy	-0.000 (0.001)	-0.000 (0.001)	-0.011* (0.006)	-0.010* (0.005)	-0.001 (0.001)	-0.001 (0.001)	-0.012** (0.006)	-0.007 (0.005)
Aid x Refugee	0.004*** (0.001)	0.004*** (0.001)	0.012*** (0.004)	0.009** (0.004)	0.002 (0.001)	0.002 (0.001)	0.011*** (0.004)	0.008** (0.004)
Aid x Refugee x Policy	-0.010** (0.004)	-0.010** (0.004)	-0.056*** (0.016)	-0.040*** (0.014)	-0.009** (0.004)	-0.008* (0.004)	-0.056*** (0.017)	-0.037** (0.015)
Policy	-0.489*** (0.184)	-0.379** (0.149)	-0.395** (0.194)	-0.288* (0.156)	-0.178 (0.128)	-0.097 (0.086)	-0.073 (0.138)	-0.017 (0.091)
Refugee	0.000 (0.017)	-0.010 (0.016)	-0.099** (0.046)	-0.080* (0.041)	-0.017 (0.017)	-0.019 (0.017)	-0.126*** (0.048)	-0.090** (0.044)
Refugee x Policy	0.045 (0.051)	0.064 (0.050)	0.603*** (0.184)	0.435*** (0.162)	0.109** (0.054)	0.098* (0.053)	0.682*** (0.194)	0.450*** (0.175)
Conflict		0.202*** (0.004)		0.202*** (0.004)		0.169*** (0.005)		0.169*** (0.005)
Refugees Stock (Country Level)					0.002*** (0.001)	0.002** (0.001)	0.002** (0.001)	0.001* (0.001)
IDPs Stock (Country Level)					-0.000 (0.000)	-0.001*** (0.000)	-0.000 (0.000)	-0.001*** (0.000)
Returnees Stock (Country Level)					0.001*** (0.000)	0.002*** (0.000)	0.002*** (0.000)	0.002*** (0.000)
Battle Deaths in Neighboring Countries (Country Level)					0.007*** (0.001)	0.006*** (0.001)	0.007*** (0.001)	0.006*** (0.001)
Domestic Battle Deaths (Country Level)					0.015*** (0.001)	0.008*** (0.001)	0.015*** (0.001)	0.009*** (0.001)
V-DEM Liberal Democracy (Country Level)					0.275*** (0.047)	0.236*** (0.046)	0.270*** (0.047)	0.230*** (0.046)
V-DEM Electoral Democracy (Country Level)					-0.284*** (0.043)	-0.240*** (0.043)	-0.268*** (0.044)	-0.223*** (0.043)
Elite Kinship Ties (Country Level)					0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)
GDP (Country Level)					0.014*** (0.005)	0.008 (0.005)	0.015*** (0.005)	0.009* (0.005)
Population (Country Level)					-0.029*** (0.006)	-0.018*** (0.006)	-0.030*** (0.006)	-0.018*** (0.006)
Full Control	✓	✓	✓	✓	✓	✓	✓	✓
Province RE	✓	✓	✓	✓	✓	✓	✓	✓
Year RE	✓	✓	✓	✓	✓	✓	✓	✓
Country RE	✓	✓	✓	✓	✓	✓	✓	✓
Country Random Slope	✓	✓	✓	✓	✓	✓	✓	✓
AIC	-14604.035	-16426.389	-14619.948	-16440.196	-13525.016	-14570.149	-13544.877	-14589.792
Num. obs.	47803	47803	47803	47803	42742	42742	42742	42742
Var: Province	0.016	0.010	0.016	0.010	0.017	0.011	0.017	0.011
Var: Policy (Random Slope)	3.136	2.000	3.183	1.977	1.341	0.547	1.325	0.473
Var: Country	0.214	0.128	0.214	0.127	0.086	0.036	0.085	0.032
Var: Year	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Var: Residual	0.038	0.037	0.038	0.037	0.037	0.037	0.037	0.037

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table A12: Multi-level Model Specifications and Country-Level Controls

In addition, I include country-level covariates, $Z'_{c,t-3}\beta_9$, to account for national factors that may shape refugee policy and conflict. I control for the logged average number of civil conflict events in domestic and neighboring countries, since nearby civil wars may

both affect refugee policy liberalization and increase instability in host states. I also include a measure of shared ethnicity between host elites and potential refugees. To capture regime type, I include V-Dem’s Electoral and Liberal Democracy Index. I also control for logged country-level GDP and population from the World Bank, as well as logged country-year stocks of refugees, IDPs, and returnees from the UNHCR Refugee Population Statistics Database.

Table A12 reports multilevel mixed-effects specifications with (Columns 5–8) and without (Columns 1–4) country-level covariates. Both the multilevel models and the instrumental-variable approach produce results consistent with the main findings.

A.9 Country-Level Analysis

In the country-level analysis, I aggregate World Bank humanitarian aid commitments to the country-year level, distinguishing aid in refugee-hosting areas from aid elsewhere. The sample scope, refugee-policy measure, and lag structure follow the province-level analysis. Covariates follow the multilevel specifications. To address endogenous aid allocation, I use a shift–share instrument based on the World Bank IDA funding position and each country’s 1995–1999 share of World Bank aid. Although the POSVAR outcomes are ordinal 0–3 measures, I treat them linearly to minimize assumptions and avoid convergence issues in models with high-dimensional fixed effects.

A.10 Justification for Lag Structure

I adopt a lag structure that aligns with the temporal logic of the theoretical framework and is informed by empirical evidence on the implementation timelines of refugee policy and foreign aid. This approach is designed to mitigate simultaneity bias by ensuring that the key independent variables temporally precede the observed conflict outcomes.

Foreign aid is lagged by two years to reflect the typical delay between project approval and the point at which projects begin to affect conditions on the ground. **Refugee policy** is lagged by three years, allowing time for national legal reforms to diffuse through administrative systems and shape governance practices at the local level. **Refugee presence** and all other covariates are also lagged by three years to match the lag structure of **Refugee policy**. Using shorter lags for these variables could introduce post-treatment bias if refugee settlement patterns or socioeconomic conditions are themselves shaped by the policy regime. This lag structure also reflects the institutional and social processes through which displacement affects conflict, which unfold gradually as host communities adapt to demographic changes and shifting service demands.

Several robustness tests confirm that the findings are not sensitive to the lag structure. As Table A13 and Figure A5 show, the main results remain robust across alternative lag specifications. The only exception arises when aid is lagged by only one year, in which case the OLS estimate of the triple interaction becomes less precise. This pattern is consistent with the expectation that aid projects take time to influence conflict outcomes.

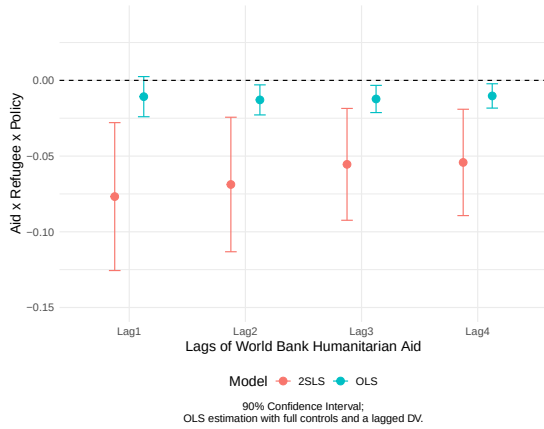


Figure A5: Regressions with Different Lags of World Bank Aid

	Covariates (Lag 1)		Covariates (Lag 2)		Policy & Refugee (Lag 1)		Policy & Refugee (Lag 2)	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
Aid	0.000 (0.001)	-0.002 (0.003)	0.000 (0.001)	-0.002 (0.003)	0.000 (0.001)	-0.005 (0.004)	0.000 (0.001)	-0.004 (0.004)
Aid x Policy	-0.002 (0.002)	0.008 (0.010)	-0.002 (0.002)	0.008 (0.011)	-0.002 (0.002)	0.022 (0.015)	-0.002 (0.002)	0.018 (0.014)
Aid x Refugee	0.004** (0.002)	0.008 (0.006)	0.005** (0.002)	0.011* (0.006)	0.006** (0.002)	0.009 (0.008)	0.005* (0.002)	0.009 (0.008)
Aid x Refugee x Policy	-0.012** (0.005)	-0.055** (0.022)	-0.013** (0.006)	-0.066*** (0.024)	-0.018** (0.007)	-0.058** (0.027)	-0.013* (0.007)	-0.060** (0.028)
Refugee	-0.034 (0.029)	-0.081 (0.072)	-0.030 (0.032)	-0.102 (0.076)	-0.043 (0.037)	-0.076 (0.094)	-0.028 (0.036)	-0.083 (0.092)
Refugee x Policy	0.122 (0.080)	0.632** (0.256)	0.114 (0.091)	0.746*** (0.280)	0.159 (0.105)	0.626* (0.317)	0.109 (0.106)	0.666** (0.327)
Conflict	0.239*** (0.027)	0.240*** (0.027)	0.146*** (0.025)	0.145*** (0.025)	0.084*** (0.016)	0.085*** (0.016)	0.084*** (0.016)	0.084*** (0.016)
Full Control	✓	✓	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Num. obs.	47795	47795	47795	47795	47935	47935	47935	47935
R ² (full model)	0.585	0.585	0.569	0.569	0.562	0.562	0.562	0.562
R ² (within)	0.059	0.059	0.023	0.023	0.009	0.008	0.008	0.008

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table A13: Regressions with Different Lags of Covariates and Refugee Policy

A.11 Addressing Spatial Interdependence

Table A14 estimates models with a spatial lag of conflict (Columns 1–3) and Spatial Durbin models with spatial lags of both aid and conflict (Columns 4–6). The results remain broadly consistent with the main finding.

	SLY			SDM		
	OLS	IV	IV	OLS	IV	IV
Aid	0.000 (0.001)	−0.003 (0.003)	−0.002 (0.003)	0.000 (0.001)	−0.002 (0.004)	−0.001 (0.003)
Aid x Policy	−0.002 (0.002)	0.013 (0.013)	0.010 (0.012)	−0.003 (0.002)	0.007 (0.013)	0.004 (0.012)
Aid x Refugee	0.005** (0.002)	0.012 (0.007)	0.010 (0.007)	0.001 (0.002)	0.016 (0.011)	0.013 (0.010)
Aid x Refugee x Policy	−0.013** (0.006)	−0.073** (0.028)	−0.064** (0.027)	−0.005 (0.006)	−0.109* (0.055)	−0.098* (0.050)
Refugee	−0.027 (0.033)	−0.108 (0.083)	−0.092 (0.081)	−0.063 (0.044)	−0.113 (0.140)	−0.107 (0.135)
Refugee x Policy	0.112 (0.092)	0.813** (0.330)	0.719** (0.310)	0.200 (0.128)	0.863 (0.566)	0.799 (0.534)
Conflict (Spatial Lag)	0.074*** (0.016)	0.074*** (0.016)	0.071*** (0.016)	0.074*** (0.016)	0.073*** (0.016)	0.071*** (0.016)
Aid (Spatial Lag)				−0.001 (0.002)	−0.013 (0.013)	−0.012 (0.012)
Aid (Spatial Lag) x Policy				0.002 (0.004)	0.056 (0.049)	0.053 (0.044)
Aid (Spatial Lag) x Refugee				0.007 (0.005)	−0.005 (0.018)	−0.003 (0.018)
Aid (Spatial Lag) x Refugee x Policy				−0.018 (0.013)	0.036 (0.072)	0.031 (0.069)
Conflict (Temporal Lag)			0.081*** (0.016)			0.081*** (0.015)
Full Control	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓	✓
Lagged DV			✓			✓
Num. obs.	47795	47795	47795	47795	47795	47795
R ² (full model)	0.562	0.562	0.565	0.563	0.562	0.565
R ² (within)	0.009	0.009	0.016	0.009	0.009	0.016

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table A14: Robustness to Spatial Interdependence

A.12 Matching Analysis Results

	Matched on Refugee Presence			Matched on Policy		
	(1)	(2)	(3)	(4)	(5)	(6)
Aid	−0.000 (0.001)	−0.000 (0.001)	−0.000 (0.001)	−0.001 (0.001)	−0.001 (0.001)	−0.001 (0.001)
Aid x Policy	−0.001 (0.002)	−0.001 (0.002)	−0.001 (0.001)	−0.001 (0.004)	−0.001 (0.004)	−0.001 (0.004)
Aid x Refugee	0.004* (0.002)	0.004* (0.002)	0.004* (0.002)	0.009** (0.003)	0.008*** (0.003)	0.008*** (0.003)
Aid x Refugee x Policy	−0.012* (0.006)	−0.012* (0.006)	−0.011* (0.006)	−0.041*** (0.016)	−0.041*** (0.015)	−0.040*** (0.015)
Refugee	−0.000 (0.038)	0.008 (0.041)	0.005 (0.040)	−0.104** (0.051)	−0.111** (0.049)	−0.107** (0.048)
Refugee x Policy	0.083 (0.117)	0.067 (0.122)	0.066 (0.120)	0.541** (0.240)	0.557** (0.232)	0.542** (0.229)
Conflict			0.073*** (0.024)		0.027 (0.024)	
Full Control	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Country-by-Year FE	✓	✓	✓	✓	✓	✓
Num. obs.	31260	31260	31260	13136	13136	13136
R ² (full model)	0.536	0.537	0.540	0.352	0.352	0.353
R ² (within)	0.002	0.003	0.011	0.005	0.006	0.006

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table A15: CEM on Refugee Presence and Policy Liberality

Variable	Means (With Refugees)	Means (Without Refugees)	Var. Ratio
IDP	0.00	0.00	
Refugee in Other Provinces	0.91	0.91	
Population	0.23	0.22	1.01
GDP per capita	0.14	0.12	1.03
Returnee	0.00	0.00	
Aid	0.16	0.16	1.01
Lagged Civil Conflict	0.05	0.05	1.00
Excluded Ethnic Groups	0.60	0.60	

Table A16: Balance Table of the Matched Data: Refugee Presence

Variable	Means (Incl. Policy)	Means (Restr. Policy)	Var. Ratio
Elite Kinship Ties	−0.36	−0.36	0.97
Total Aid	−0.39	−0.45	1.27
Neighboring Conflict	0.56	0.56	
Civil Conflict	0.06	0.06	
Country Refugee Stock	−0.29	−0.30	1.20
Country Returnee Stock	−0.21	−0.20	0.85
Country IDP Stock	−0.21	−0.21	1.25
Electoral Democracy (V-Dem)	0.24	0.25	1.03
Population	−0.29	−0.32	1.10
GDP	−0.22	−0.24	0.45
Liberal Democracy (V-Dem)	0.18	0.18	1.04

Table A17: Balance Table of the Matched Data: Economic Rights

A.13 Regression Tables with Full Controls and All Available Years

	Economic Rights				Non-Economic Rights			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Aid	-0.001 (0.000)	0.000 (0.001)	0.000 (0.001)	0.001 (0.001)	-0.002 (0.005)	-0.001 (0.005)	-0.001 (0.001)	-0.003 (0.003)
Aid x Refugee	0.001 (0.001)	0.006** (0.003)	0.006** (0.003)	0.006** (0.003)	0.011 (0.007)	0.010 (0.007)	0.005* (0.003)	0.005 (0.007)
Aid x Policy		-0.003 (0.003)	-0.004 (0.003)	-0.003 (0.003)	0.004 (0.013)	0.001 (0.012)	0.003 (0.004)	0.012 (0.018)
Aid x Refugee x Policy		-0.015** (0.007)	-0.016** (0.007)	-0.015** (0.007)	-0.054** (0.026)	-0.049** (0.025)	-0.013 (0.009)	-0.036 (0.023)
Refugee	0.005 (0.013)	-0.067 (0.045)	-0.070 (0.046)	-0.071 (0.044)	-0.129 (0.102)	-0.120 (0.098)	-0.049 (0.042)	-0.067 (0.088)
Refugee x Policy		0.218** (0.107)	0.226** (0.109)	0.224** (0.104)	0.682** (0.329)	0.640** (0.316)	0.199 (0.139)	0.525 (0.324)
Conflict				0.069*** (0.011)		0.069*** (0.011)	0.069*** (0.011)	0.069*** (0.011)
IDP			0.054 (0.074)	0.053 (0.069)	0.059 (0.076)	0.058 (0.071)	0.052 (0.069)	0.055 (0.071)
Returnees			0.005 (0.051)	0.004 (0.047)	0.009 (0.053)	0.007 (0.049)	0.003 (0.047)	0.004 (0.049)
Population			-0.004 (0.042)	-0.006 (0.040)	-0.007 (0.042)	-0.009 (0.039)	-0.006 (0.040)	-0.008 (0.040)
GDP per Capita			-0.003 (0.026)	-0.002 (0.024)	-0.003 (0.026)	-0.003 (0.024)	-0.002 (0.024)	-0.002 (0.025)
Excluded Ethnic Groups			0.026 (0.019)	0.025 (0.017)	0.026 (0.018)	0.025 (0.017)	0.025 (0.017)	0.024 (0.017)
Full Control								
Province FE	✓	✓	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Model	OLS	OLS	OLS	OLS	IV	IV	OLS	IV
Num. obs.	47795	47795	47795	47795	47795	47795	47795	47795
R ² (full model)	0.594	0.594	0.594	0.596	0.594	0.596	0.596	0.596
R ² (within)	0.000	0.001	0.001	0.006	0.001	0.006	0.006	0.005

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

	Economic Rights				Non-Economic Rights			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Aid	-0.001 (0.000)	-0.000 (0.001)	-0.000 (0.001)	-0.000 (0.001)	-0.001 (0.003)	-0.000 (0.003)	-0.001 (0.000)	-0.002 (0.002)
Aid x Policy		-0.001 (0.002)	-0.001 (0.002)	-0.001 (0.002)	0.004 (0.010)	-0.000 (0.008)	0.001 (0.002)	0.011 (0.010)
Aid x Refugee	0.001 (0.001)	0.005** (0.002)	0.005** (0.002)	0.004** (0.002)	0.012 (0.007)	0.010 (0.007)	0.002 (0.002)	0.003 (0.005)
Aid x Refugee x Policy		-0.012** (0.005)	-0.013** (0.005)	-0.012** (0.005)	-0.068** (0.027)	-0.058** (0.024)	-0.006 (0.005)	-0.028 (0.023)
Refugee	-0.002 (0.011)	-0.030 (0.030)	-0.032 (0.031)	-0.034 (0.029)	-0.115 (0.093)	-0.098 (0.085)	-0.023 (0.023)	-0.034 (0.074)
Refugee x Policy		0.095 (0.087)	0.097 (0.088)	0.105 (0.084)	0.759** (0.320)	0.656** (0.286)	0.083 (0.080)	0.379 (0.314)
Conflict				0.132*** (0.018)		0.132*** (0.019)	0.132*** (0.019)	0.132*** (0.018)
Full Control			✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Model	OLS	OLS	OLS	OLS	IV	IV	OLS	IV
Num. obs.	62159	62159	62159	62159	62159	62159	62133	62133
R ² (full model)	0.553	0.553	0.554	0.561	0.553	0.561	0.561	0.561
R ² (within)	0.000	0.000	0.002	0.020	0.002	0.019	0.019	0.019

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table A19: Foreign Aid, Refugee Policy and Armed Conflict (1997–2022)

Table A18: Foreign Aid, Refugee Policy and Armed Conflict (Full Controls)