

The Aftermath of War: Conflict Exposure and Subnational Recovery

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October 30, 2026

Abstract

How does past exposure to fighting shape a district's economic trajectory after war? This paper studies heterogeneous post-conflict recovery using a panel of more than 5,400 African districts from 1996 to 2020. It introduces a country-level classification of post-conflict periods and a district-level measure of the intensity and timing of wartime exposure based on UCDP conflict events. Using harmonized nighttime lights as a measure of local economic activity, I show that a national peace dividend and local war scarring can coexist within the same country: districts without locally recorded fighting grow faster than districts in peaceful countries, while directly exposed districts face an additional recovery penalty. I then use geocoded World Bank disbursements to examine whether the association between aid and subsequent local growth varies across these recovery environments. The aid-growth association differs across wartime-exposure groups and post-conflict conditions, including the durability of peace. National post-conflict status therefore masks differences in both local recovery and the outcomes associated with spatially targeted development assistance.

Keywords: Post-Conflict, Subnational Conflict Exposure, Foreign Aid, Economic Development, GIS data

JEL: D74, F35, H56, O19, O47

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*The paper has been disseminated under the former title — Growth in the Aftermath of War: Aid Effectiveness in Post-Conflict Locations. Acknowledgements: I want to say thank you to Lisa Chauvet, Mathieu Couttenier, Axel Dreher, Sophie Hatte, Jérémy Lucchetti, Johannes Matzat, Manuel Oechslin, Paula Sanchez Gil, Lukas Schmid and Uwe Sunde for their helpful comments and feedback. Further thanks to comments and feedback from participants of seminars at the University of Lucerne, ENS de Lyon - CERGIC, University of Geneva, the 11th OEEET Workshop 2025, the RES Conference 2024, EPCS Conference 2024, SSES Congress 2024, the Annual Conference on the Political Economy of Aid 2023, ifo Dresden Workshop on Regional Economics 2023, Annual Conference of the DENS 2023, RES PhD Conference 2023 and Max Planck PolEcCon 2023.

1 Introduction

Spatially heterogeneous fighting leaves spatially heterogeneous economic conditions when war ends. Armed conflict is highly uneven within countries with fighting concentrated in particular locations and intensity varying sharply across a country. Even a large interstate war such as Ukraine since 2022 has exposed regions very differently (ACAPS, 2022). Yet the end of war is dated at the national level, and its economic aftermath is commonly studied either in aggregate (Collier, 1999; de Groot et al., 2022; Gates et al., 2012) or within individual conflicts (Abadie & Gardeazabal, 2003; Almer & Hodler, 2015; Miguel & Roland, 2011; Yamada & Yamada, 2021). This leaves limited systematic multi-country evidence on whether locations within the same post-conflict country recover along similar paths. Therefore, this paper asks how a district’s past exposure to fighting shapes its economic trajectory after war.

War reduces investment and destroys physical and human capital (Collier, 1999), but both the incidence of these losses and the adjustments to them are local. Districts spared from fighting may receive displaced households and shifts in demand, while exposed districts lose population and human capital and sustain damage to property and infrastructure. The restoration of national stability may therefore produce a peace dividend in some locations while the effects of local fighting continue to weigh on others.

This possibility connects two competing views of postwar development. The war-ruin hypothesis emphasizes weakened institutions, destroyed capital, and a costly reconstruction process (Nkurunziza, 2019). The phoenix-factor hypothesis predicts recovery as capital is replaced and economic activity reorganizes (Collier, 1999). Evidence from individual wars ranges from persistent local losses to convergence toward the prewar path (Miguel & Roland, 2011; Redding & Sturm, 2024; Yamada & Yamada, 2021). In Normandy, municipalities exposed to longer combat initially lost population, subsequently caught up, and eventually exceeded the trajectory implied by their prewar trend; Chauvet et al. (2026) relate this pattern to the scale and quality of reconstruction. At the subnational level, war-ruin and phoenix-factor dynamics need not be mutually exclusive. They may emerge simultaneously within the same national transition, depending on where and how intensely the preceding war was fought. These divergent recovery conditions may also affect how subnationally targeted development assistance translates into local economic activity.

I study this question using a panel of more than 5,400 districts in 46 African countries from 1996 to 2020. I combine conflict events from the UCDP Georeferenced Event Dataset (Sundberg & Melander, 2013) with harmonized nighttime lights as a measure of local economic activity (Li et al., 2020). I first identify post-conflict periods at the country level. Within post-conflict countries, I distinguish districts with fatalities recorded inside their boundaries during the preceding war from districts without locally recorded fighting. For exposed districts, an intensity index summarizes fatalities over the full war and accounts for both their timing and conflict recurrence. The resulting classification separates districts in peaceful countries, districts exposed to war only through the national environment, and districts directly exposed to different intensities of fighting. I then combine these recovery environments with district-level World Bank disbursements to

examine whether the association between aid and subsequent growth varies across them (Bomprezzi et al., 2024).

To estimate differential recovery, I regress annual changes in nighttime lights on country-level post-conflict status and local wartime exposure. Districts in peaceful countries form the omitted category. The coefficient on country-level post-conflict status measures the differential growth of districts without locally recorded fighting, while the district-exposure coefficients measure the additional differential associated with local fighting. The estimated differential for an exposed district is therefore the sum of the country- and district-level coefficients. Interacting aid with both post-conflict indicators shows whether its association with growth differs between peaceful districts, post-conflict districts without local fighting, and directly exposed districts.

The baseline specification includes district and year fixed effects, district- and country-specific linear trends, time-varying district and country controls, and standard errors clustered by district. Identification comes from changes within districts relative to their own development paths, net of common shocks and country-specific trends. A specification with country-year fixed effects further absorbs all national shocks and compares exposed and non-exposed districts within the same country and year. The main recovery pattern is stable across alternative fixed-effect structures, control sets, and data sources.

The results show that the post-conflict peace dividend depends on local exposure to the preceding war. Districts in post-conflict countries without locally recorded fighting experience significantly higher nighttime-light growth than districts in peaceful countries. Directly exposed districts have an additional negative growth differential that largely offsets this country-wide advantage in the pooled specification. Thus, the same national peace transition is associated with a phoenix-factor pattern in districts spared from fighting and a war-ruin pattern in those directly exposed. The negative local differential remains statistically significant when country-year fixed effects absorb national variation and district-specific trends account for each district’s underlying trajectory.

The average pattern masks substantial heterogeneity. The positive differential for post-conflict districts without local fighting disappears when large economies and North African countries are excluded, and the remaining sample displays more adverse recovery across exposure groups. Recovery also changes with time since the end of war. Very-high-intensity districts initially experience a positive differential but subsequently follow a negative trajectory, while low-intensity districts display the opposite pattern. The durability of peace matters as well. Lasting peace is associated with stronger recovery among districts without local fighting and offsets much of the additional scarring at some levels of local exposure, although the pooled estimate remains more adverse for exposed districts. These patterns show that post-conflict recovery is structured not only by whether fighting occurred locally, but also by the broader national setting, the intensity and timing of exposure, and the durability of peace.

I next use district-level World Bank disbursements from the Geocoded Official Development Assistance Dataset (GODAD) to examine whether the aid-growth relationship varies across these recovery environments

(Bomprezzi et al., 2024). Aid is positively associated with growth in post-conflict districts without locally recorded fighting, while the additional association varies with local exposure; for low-intensity districts, the combined estimate is negative. The relationship also differs across sectors and becomes more positive where peace lasts. Because aid allocation remains endogenous, I interpret these estimates as conditional associations rather than a general causal effect of aid.

The paper contributes to three strands of research. First, it provides systematic multi-country evidence that war-ruin and phoenix-factor dynamics can coexist within the same national post-conflict transition. Existing studies show that destruction can leave persistent local effects, but also that affected locations may return to their previous trajectory or move beyond it (Chauvet et al., 2026; Miguel & Roland, 2011; Redding & Sturm, 2024; Yamada & Yamada, 2021). I show that these patterns need not characterize different wars or countries: within the same post-conflict country, districts spared from local fighting experience a peace dividend, while directly exposed districts continue to bear an additional growth penalty. The paper therefore shifts the question from whether post-conflict economies recover on average to how recovery is distributed across locations within them.

Second, the paper develops a transparent and replicable framework for classifying post-conflict periods and measuring local exposure over the entire preceding war. This sharpens the comparison underlying existing studies (Collier & Hoeffler, 2004; Donaubauer et al., 2019; Fischer, 2022), in which post-conflict periods are often compared with all other country states, thereby pooling substantially different environments in the baseline category. Whereas Bluhm et al. (2021) construct an ordinal measure of conflict escalation and de-escalation, the framework developed here identifies the transition into a post-conflict period and links each post-conflict district to its accumulated exposure over the preceding war. The resulting classification and exposure index provide a reusable framework for comparing post-conflict environments and estimating heterogeneous recovery patterns.

Third, the paper extends national-level work on post-conflict aid and its sectoral allocation by examining variation within post-conflict countries (Collier & Hoeffler, 2004; Donaubauer et al., 2019). National estimates may conceal substantial differences across locations with distinct wartime histories. The relationship between World Bank aid and subsequent local growth varies with district-level wartime exposure, even among districts sharing the same national post-conflict status. This implies that project outcomes should be interpreted in relation to the local recovery conditions under which projects are implemented.

Section 2 develops the post-conflict framework, and Section 3 describes the data, measurement, and empirical approach. Section 4 presents the recovery and aid findings together with the robustness exercises. Section 5 examines how the recovery and aid patterns vary with persistence and timing, spatial spillovers, and sectoral allocation. Section 6 discusses the implications and concludes.

2 The Post-Conflict Framework

Posed by the acute instability, a country's economy contracts during war because of factors such as capital flight, disinvestment in human capital, physical property destruction, displacement and destruction of human capital, dissaving, and shifts in public spending towards increased military funding. These consequences are likely local. Some locations experience an influx of displaced people and their savings are spent on necessities there, other locations experience higher destruction of physical property and infrastructure, increased capital flight and a large outflow of human capital. A location's wartime experience shapes its post-conflict conditions.

The economic impact of conflict is conceptualized by two competing hypotheses. The *war ruin* hypothesis states that economies experience a lasting negative effect after civil wars. Institutions are on average weaker, the economy is destroyed, and post-conflict reconstruction is lengthy and costly. Additionally, rebuilding efforts and investments are hindered by a significant probability of war reoccurrence (Hoeffler, 2012; Hoeffler et al., 2011; Nkurunziza, 2019). These same constraints may also condition the relationship between development assistance and local growth.¹ Any impact of reconstruction efforts might be mitigated by the risk of war reoccurrence.

On the contrary, the *phoenix factor* hypothesis posits that economies destroyed by war are arising from the ashes. During the rebuilding process, outdated institutions are superseded by functional and efficient structures. Devastated economies are replaced by competitive systems. Infrastructure is rebuilt with up-to-date technology. Economies experience increased growth rates compared to peaceful ones - the so-called *peace dividend*. These economies may also have greater absorptive capacity for external assistance (Collier & Hoeffler, 2004), producing a more favorable association between aid and development outcomes (Hoeffler, 2012; Hoeffler et al., 2011; Nkurunziza, 2019).

These hypotheses have been mainly studied at the country level. The following empirical findings further indicate that differential subnational war exposure affects economic outcomes. Redding and Sturm (2024) show that neighborhoods in London that have been more severely bombed and destructed during WWII, have lower property values of 11-18% compared to non-destructed neighborhoods. They further detect a decreased share of high-income residents and significant negative spillover effects to adjacent neighborhoods. The effects are explained by a locations post-conflict attractiveness due to replacing destructed locations with social housing. Findings of Miguel and Roland (2011) indicate that heavy US bombing on Vietnamese districts has no empirically significant long-term impact on infrastructure, local poverty rates and socio-economic outcomes. Yamada and Yamada (2021) show that this result also applies to Southern Laos, however, Northern Laos experienced lasting negative effects. Amodio and Maio (2018) further show that conflict intensity induces a differential impact on firms' accessibility to markets. Firms more exposed to conflict during the Second Intifada shifted from relying on imports to domestically produced materials.

¹Collier and Hoeffler (2004) show that institutional performance conditions the relationship between aid and growth.

de Roux and Martinez (2023) show that municipalities close to markets² and exposed to higher FARC activities during the FARC insurgency in Colombia experience higher long-term investments after a peace agreement is signed. These findings suggest that variation in past conflict exposure can produce different local recovery paths. The same variation may condition the relationship between development assistance and nighttime-light growth. Local wartime exposure is therefore relevant both for analyzing post-conflict development and for interpreting geographically targeted interventions.

3 Data and Methodology

The empirical analysis relates subsequent district growth to country-level post-conflict status and local wartime exposure. The specification also interacts both post-conflict indicators with aid to examine whether the aid-growth relationship varies across the resulting recovery environments. Districts in peaceful countries provide the comparison category. The following subsections describe the conflict classification, exposure intensity, aid, nighttime lights, and time-varying controls used in the specification.

3.1 Identifying post-conflict locations

To compare recovery trajectories after war, I first identify post-conflict periods and distinguish districts by their local wartime exposure. These categories also allow aid disbursements to be compared across districts facing different post-conflict conditions.

For this purpose, the UCDP-GED dataset is employed as it reports events of armed force "used by an organised actor against another organized actor, or against civilians, resulting in at least 1 direct death at a specific location and a specific date" (Högbladh, 2022, p.4). Data with global coverage is available from 1989 to 2023. UCDP is used for this study as it focuses on major wars, whereas the main alternative source, ACLED, the Armed Conflict Location and Event Dataset concentrates on local political violence (Raleigh et al., 2010).³ Additionally, the longer time frame of the UCDP-GED dataset allows the identification of conflict and post-war locations for the whole period that aid data is available.

3.1.1 Categorization

As a next step, countries and their districts need to be categorized to map them from conflict to peace. A precise categorization of a country's conflict-to-peace cycle is integral for accurately measuring post-conflict development.⁴ To classify post-conflict stages with peaceful periods as a counterfactual, this paper

²Markets access is determined as being close to wholesale markets, the department capital or Bogotá (de Roux & Martinez, 2023).

³ACLED data is available for most African countries for the years 1997 to 2025. Comoros and Sao Tome and Principe are not available for that time frame (Raleigh et al., 2010). The ACLED dataset distinguishes conflict events related to political violence including riots and protests that do not necessarily result in battle deaths. As ACLED focuses on different aspects of a conflict e.g. during the time of the Liberian civil war barely any conflict deaths are observed in ACLED whereas UCDP captures the total amount of deaths well on an aggregate level.

⁴If a country is still considered at war by the international community, it likely affects aid disbursement decisions and would add noise to any estimates.

introduces a novel categorization method that captures the evolution at the different stages from peace to post-conflict at both the country and subnational levels.

So far, the standard approach in the literature was that any country with an aggregated sum of 1000 battle deaths or more is considered to be at war, and those with at least 25 to 999 battle deaths are regarded as minor conflict. The period after war was considered as post-conflict whereas the counterfactual is a mix of conflict, minor conflict and peace.⁵ However, conflict is inherently dynamic, continuously evolving over time and space. Bluhm et al. (2021) therefore introduce an ordinal measurement to capture this dynamic nature of conflict escalation and de-escalation. However, this measurement is not applicable for the transition to post-conflict. This suggests the need for a novel methodology which is dynamic and integrates a post-conflict stage.

The new classification introduced in this paper is data-driven and done following the decision tree shown in Appendix A.2. Table A.2 gives an overview for the panel of countries used in this study categorizing them based on this methodology. At an aggregate level, countries are categorized into one of five stages each year following a dummy encoding system. A country can be either in a state of war, post-conflict, minor conflict, post-minor conflict or peace. Based on the aggregate categorization, at the district level, it can be directly affected or not. *Conflict*: Countries are at war at time t if they have 1,000 or more fatalities, or at least 300 battle deaths at time t and an average of at least 500 battle deaths during the current and preceding year. The latter condition applies if the country recorded at least 1,000 fatalities within the preceding ten years. By smoothing around the cut-off levels from the standard approach, common war periods and periods of major instability after war are better reflected at the country level.⁶

Post-Conflict: In the next step, a country is categorized as post-conflict if within the past 1 to 10 years it was in a state of war but by the above definition, there is no active war anymore. A post-conflict period is reclassified if a new war sets on, reverting to an active conflict state.

Peace: Peaceful periods serve as the baseline. A country is considered at peace if it is neither at war nor post-conflict, and has less than 25 battle deaths at time t or a 3-year rolling average of fewer than 25 fatalities. A smoothing average around the minimum cut-off is used to reduce noise. In addition to the major categories of war, post-conflict, and peace, there are also minor conflicts and post-minor conflict periods.

Minor and Post-minor Conflict: Countries experiencing minor conflict are considered post-minor conflict for the two years following the minor conflict.⁷ This middle step ensures that countries at peace serve as

⁵Utilizing this standard approach, post-conflict periods then indicate the first 10 years after the war. Any coefficients resulting from the post-conflict dummy are hard to interpret without a clear counterfactual. Consider the case of the Republic of Congo: In 1999, Congo experienced 948 conflict-related deaths. Using the standard threshold of 1000 battle deaths, the country was no longer "at war". However, given the more than 3,700 fatalities in the previous year and over 10,000 deaths before that, categorizing 1999 as not part of the war would overlook ongoing instability. Thus, to ensure an accurate aggregate picture, the situation in 1999 should still be regarded as a wartime period.

⁶The cut-off levels are chosen driven by the data to accurately reflect the timing of major war periods for most African countries.

⁷Analysis of the conflict data reveals that minor conflicts have some effects lasting approximately 2 years, which informs this set-up.

a counterfactual, avoiding capturing effects in the peace category that may be due to past minor conflicts. This categorization allows for comparisons of the impact of experiencing any kind of conflict state and peace at an aggregate level. In terms of subnational interpretation, it enables the identification of effects, such as whether districts within a conflict country experience different outcomes compared to those in peaceful countries. Once the country-level classification is established, the next step is categorising a district's involvement. The district level is dependent on the country-level classification, indicating passive or active involvement in the country's events.⁸ This means determining whether fighting occurred within a district's territory during the war period. Similarly, for minor conflicts, the question is whether any fatalities were recorded within a district's administrative boundaries during the year of minor conflict. The variation in Figure 8 indicates that fighting does not occur in every district during a country's war and intensity varies substantially across exposed districts.

3.1.2 Intensity Index

Although the categorization identifies a district's stage in the peace-to-post-conflict cycle, it does not capture its intensity of involvement. The intensity index I introduce in this study incorporates the duration of a war as well as its intensity and timing of fighting at the subnational level. As war evolves over time and space, districts are affected by varying intensity at different times. These differences likely shape post-conflict conditions and may also matter for interventions operating within them. Recovery may differ, for example, depending on whether heavy fighting occurred in a district years ago or just before the country entered the post-conflict stage. In the index, I discount heavy fighting at the beginning of the war compared to exposure to violence towards the end of the war. Furthermore, the index takes into account conflict re-occurrences. This ensures that if a country is in a post-conflict state and war reoccurs, fatalities of the past war are still taken into account. After ten years, any post-conflict or past war experiences collapse to peace, in line with the standard definition used by Collier and Hoeffler (2004) and Donaubauer et al. (2019). The following formula quantifies the intensity of past conflict involvement of post-conflict districts:

$$II_{rt} = \omega_{ct} \sum_{i_t=1}^{N_t} \delta_{crt} \frac{i_t}{N_t} + \tau_{ct} \sum_{i_z=1}^{N_z} \delta_{crz} \frac{i_z}{N_z} \left(1 - \frac{\sigma_t}{11}\right) \quad (1)$$

in which $\omega \in \{0, 1\}$ is 1 if a district is part of a post-conflict country. δ accounts for the sum of battle deaths per district at time t and i^{th} year of the war, whereas N accounts for the sum of total years of consecutive war. The first term of the formula measures the intensity of the most recent war period, discounting a district's fatalities according to the length of the war.⁹ In some countries, post-conflict peace might not last and another war sets on. The second term of the formula integrates situations where war resurges, incorporating fatalities from earlier conflicts if they occurred within a 10-year timeframe. $\tau \in \{0, 1\}$ is 1 in

⁸The decision tree in Appendix A.2 shows the respective coding of the binary variables.

⁹The discount factor follows a linear function and thus makes a strong assumption in regards of shorter war periods, as recent fatalities for short periods are more heavily discounted than such of long periods.

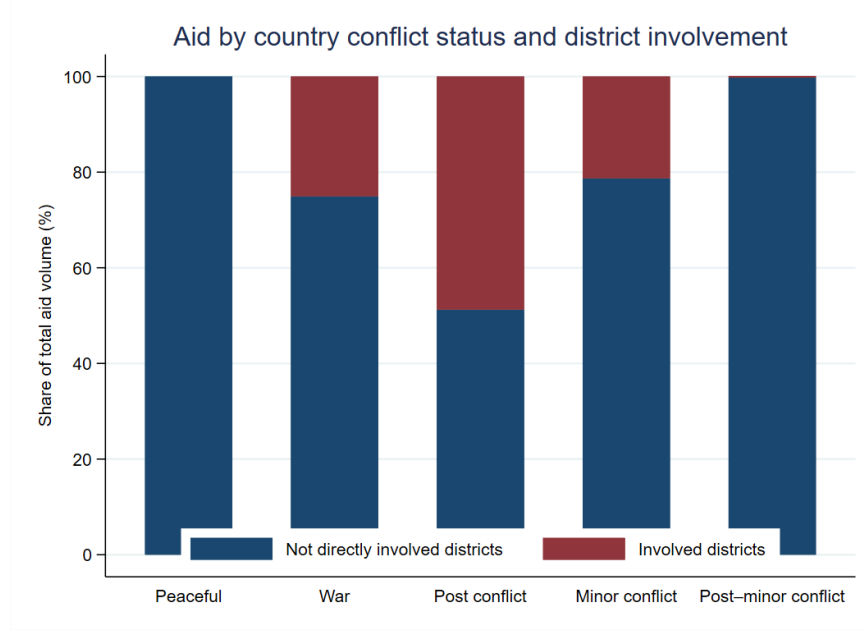


Figure 1: The figure is based on GODAD World Bank aid data with aid flows identified at the subnational level. It reports aid flows aggregated by conflict status and district involvement based on the author’s own classification.

case of war re-occurrence and $\sigma \in [1, \dots, 11]$ denotes the time passed since the first war ended. The subscript z indicates that the timing of the event is in the past, calculated as $z = t - 11 + \sigma_t$.¹⁰

The term $(1 - \frac{\sigma_t}{11})$ implies for events of war re-occurrence, fatalities are excluded from the intensity index after 10 years passed since the last active fighting. This follows partly the definition of Collier and Hoeffler (2004) in which post-conflict countries are only considered post-conflict for 10 years and after that countries are at peace. Similarly, fighting that a district experienced more than 10 years ago does not affect its current intensity value anymore. It further implies that fatalities from the past war are additionally discounted by each year that it lies in the past compared to the current post-conflict experience. Figure 8 in Appendix A indicates the variation of fighting intensity based on the intensity index in the year 2005. It shows considerable variation in the fighting intensity districts experienced during past war periods. Additionally, based on an Angolan district, Figure 10 shows battle deaths and, for comparability, Figure 11 indicates the intensity index and conflict categorization. This example demonstrates the importance of accounting for war reoccurrences in the intensity index to provide an accurate picture of a district’s overall exposure to past fighting.

3.2 Disbursing foreign aid

World Bank projects are implemented in particular locations, while districts under the same national post-conflict label may face different reconstruction constraints and development paths. Geocoded project data

¹⁰Based on the dataset, there are restrictions posed to the starting point of possible combinations of t and σ_t .

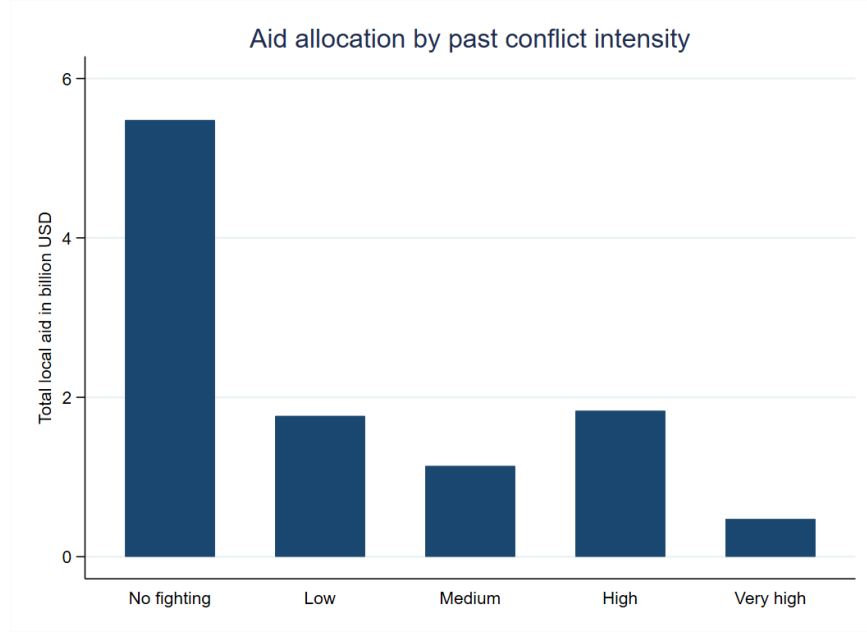


Figure 2: The figure is based on GODAD World Bank aid data with aid flows identified at the subnational level. It reports aid flows aggregated by conflict status and district involvement based on the author’s own categorization and intensity classification.

allows me to examine whether the association between aid and subsequent growth varies with these local conditions. Donor assessments point to the same concern. A SADEV review attributes the failure to reorient funding despite changing conditions partly to “a lack of systematic conflict analysis among donors and international organizations” (SADEV Report 2012). Figure 1 shows that locally disbursed World Bank aid is mainly received by the districts not involved in any fighting.

A look within post-conflict countries gives a similar picture as shown in Figure 2, locations within post-conflict countries that did not experience any involvement in fighting during wartime receive more than 50% of the locally disbursed foreign aid by the World Bank in the period from 1995 to 2020.

Findings by Nunnenkamp (2016) also indicate that at the country-level prior conflict intensity does not seem to drive donors decision of aid allocation. Hoeffler (2012) further show that post-conflict countries receive 20 percent of allocated foreign aid for economic infrastructure, the same as peaceful countries. Similar patterns are found for social infrastructure (36 percent vs. 37 percent). Nonetheless, likely the economic conditions found in post-conflict countries differ in comparison to peaceful economies. Furthermore, if aid is fungible, the government might alter its use of public funds, redirecting them to non-productive sectors - which could not only hinder growth but amplify adverse outcomes.¹¹

¹¹Aid fungibility means that either the incoming aid itself is used for other sectors than originally intended or that the government redirects intended expenditures from sector A toward sector B as incoming aid is covering the planned expenditures in sector A. Werker et al. (2009) for example show that unconditional aid to Muslim countries crowds out domestic savings and imported goods shift from capital to noncapital goods. Hur and Han (2023) indicate a differential effect in post-conflict societies depending on the aid modality. At the country level, technical assistance shows significant negative effects on GDP growth.

Subnational data for aid disbursements is retrieved from GODAD, the Geocoded Official Development Assistance Dataset (Bomprezzi et al., 2024). The data covers official development assistance from the World Bank from 1995 to 2023 and for China from 2000 to 2021. The ADM2 - year level dataset is used in this study providing information on World Bank aid disbursements at the district level including sector information. The GODAD dataset allows to expand the analysis for an additional five years, up to the year 2020 in comparison to the commonly used AidData database. Furthermore, the availability of Chinese aid data for the same timeframe allows controlling for the impact of another major donor. The AidData database (AidData, 2017) for World Bank projects has been widely used in recent aid literature (BenYishay et al., 2022; Bitzer & Gören, 2018; Briggs, 2018; Dreher & Lohmann, 2015; Gehring et al., 2022), thus, it serves as an alternative data source for comparable estimates to other aid literature. AidData information is only available for World Bank projects from 1995 to 2014, limiting the interpretation of the results to this timeframe. It further does not allow the reflection of other donors' impact besides the World Bank.¹²

Aid is considered to be endogenous as it is not randomly disbursed. There is a long-standing debate regarding the appropriate methods to establish a causal link between aid and growth (Bazzi & Clemens, 2013; Clemens et al., 2012). Poor economic growth or conflict may influence a donor's decision to allocate more or less aid to a specific region. However, the disbursement data used in this study indicates that both peaceful and post-conflict countries receive aid. Within post-conflict countries, aid is likewise disbursed to districts with and without local fighting.¹³ The timing of aid presents another point. Aid likely requires time to affect growth, however, lagged aid, as discussed in Clemens et al. (2012) may affect current growth through anticipation effects. It suggests that economic actors respond to expected aid disbursements in advance, creating indirect effects before the actual disbursement.

There are two approaches to address these potential endogeneity concerns: incorporating rigorous fixed effects or employing an instrumental variable. Clemens et al. (2012) suggest a specification that avoids relying on instruments. They propose using a lag-structure for aid to capture the delayed impact of aid on growth and further introducing first-differencing. The specification of this empirical strategy partially follows the structure proposed by Clemens et al. (2012). Further details on the specification are provided in Section 3.5. Appendix B.8 discusses strategies and results of a shift-share instrumental variable approach.

¹²AidData tracks 5,648 World Bank projects from 1995 to 2014, covering disbursements worth 390 billion US\$ and 630 billion US\$ in commitments spread over 61,243 locations. For each project, information about the start and end date, the total amount of disbursements during that time and the different locations that were targeted is provided. As this study concentrates on the district level, only projects that are administered and identified at the ADM2 level are included. As just the start and end date are known, the disbursed amount is evenly split across the years a project took place, thus averaging over those years. A project can target several locations, therefore, the total amount of disbursement is further evenly split across the locations in which a project took place following Fischer (2022) and Gehring et al. (2022). WB aid disbursements targeted to centralized government agencies, other state-level institutions or that can only be identified at the ADM1 level are excluded from the analysis (Dreher & Lohmann, 2015; Fischer, 2022; Strandow et al., 2011). Figure 9 in Appendix A shows the distribution of aid disbursements for African countries at the ADM2 level in the year 2005 using the AidData database.

¹³Figure 1 shows the distribution of aid among different categories.

3.3 Economic activity and nightlight

Subnational measurements of economic activity are often difficult to obtain from official statistics and household surveys. When available, these measurements may lack accuracy, particularly in developing countries and conflict-affected areas (Henderson et al., 2012; Martínez, 2022; Pinkovskiy & Sala-i-Martin, 2016). In such cases, nighttime light intensity captured by satellites can serve as an alternative data source to approximate economic activity. Henderson et al. (2012) were among the first to argue that nighttime light is a valid approximation for GDP growth, especially in developing countries. Changes in nightlight are particularly useful for within-country estimations (Henderson et al., 2012). Bruederle and Hodler (2018) demonstrate that variation in nighttime light reflects changes in human development indicators, while Bluhm and Krause (2018) argue that nightlights are a reliable proxy at the city level.

A potential issue with nighttime light data is its dependency on specific satellite systems. Previously, samples were limited to the periods covered by either the DMSP or VIIRS systems. The DMSP satellite system provides images from 1992 to 2013, while the VIIRS system has been recording data since 2012. Li et al. (2020) addressed inconsistencies between the DMSP and VIIRS satellite systems by calibrating and harmonizing observations from 1992 to 2020 to ensure temporal consistency.¹⁴ First, attributes that could introduce noise — such as clouds, auroras, fires and other temporal disturbances — are removed. In the next step, DMSP and VIIRS observations are integrated to obtain DMSP-like observations for the years that only VIIRS is available. Using the calibrated and harmonized images from Li et al. (2020) allows for the investigation of longer time periods and more recent years.¹⁵

To extract nighttime light intensity, I overlay the images from Li et al. (2020) with geographic boundaries at the second administrative level (ADM2), representing districts or counties (Runfola, 2020). I then extract the mean of the nighttime light within the boundary layer for 5714 districts within 50 African countries with values between 0 and 63.¹⁶ For cells that are partially covered, a weighted value of surrounding polygons is estimated. As mentioned in Martínez (2022) that use the same nightlight data, despite the harmonization process, there is a jump in the level of nightlights in 2014 and 2017 which coincides with the harmonized years. Thus, following Martínez (2022), the changes in nighttime light in 2014 and 2017 are therefore imputed, as described in Section 4.2.

Henderson et al. (2012) exclude some cases of top-coded observations as outliers. In this panel, no country observes a very high percentage of only top-coded observations. Recent critiques (Gibson et al., 2020, 2021) point out potential measurement errors in DMSP observations due to blurriness and a lack of calibration. These issues affect the quality of nighttime light data, particularly when used as a GDP proxy for sparsely populated areas. As a result, they argue for the use of VIIRS data instead. The process used in Li et al.

¹⁴Their paper only mentions the period from 1992 to 2018, however, they also share more recent processed images in their file share up to 2020.

¹⁵In the case of this paper it allows to extend the analysis up to 2020.

¹⁶Sudan and South Sudan excluded due to inconsistencies of their borders throughout the sample period. Furthermore, Sao Tome and Principe are excluded from the analysis as not all control variables are available for this country.

(2020) addresses these concerns as it calibrates the datasets to be consistent over satellite systems as well as from 2012 onward, VIIRS data is used. However, there are still bottom-coded observations. Excluding observations with an extracted value of zero could introduce a sample bias, as these observations indicate very low or nonexistent economic activity. It could mean a district is not populated (as for areas within a desert or mountainous terrain) or the population is too poor to have light emissions during nighttime. This is solved by transforming observations with a constant value.¹⁷ The robustness of this transformation is demonstrated in Section 4.2.

3.4 Controls

Despite using extensive fixed effects, various factors may still influence a district’s economic activity. To account for weather conditions that may affect local agricultural output, mean precipitation and temperature data are used (Harris et al., 2020).¹⁸

Further district-level controls include population density, total population (CIESIN, 2018) and nightlight lagged by two years to control for convergence and the level of economic activity. Peacekeeping troops, often deployed in areas affected by war, can act as potential confounders of development assistance in post-conflict settings. Cil et al. (2020) provide a geocoded dataset offering information about the deployment of peacekeeping troops from 1994 to 2020. Using this dataset, a binary variable detecting peacekeeping deployment in a specific year and district is constructed. However, peacekeeping as a confounder could act as a bad control, and is therefore only used in some specifications. In addition to the district-level controls, variables for population growth and density at the country level are obtained from the World Development Indicators (World Bank, 2022). Table A.3 in Appendix A.4 indicates the summary statistics for the variables used in the main estimation sample, while Table A.4 reports the prevalence of the post-conflict categories and intensity groups.

3.5 Empirical Framework

The empirical model jointly estimates post-conflict recovery and its relationship with aid. Country-level post-conflict status and districts’ local exposure to the preceding war capture recovery differences, while their interactions with aid allow the marginal aid association to vary across the same groups:

¹⁷The growth rate formula follows the standard estimation:

$$\Delta \ln(light) = \ln(light_t) - \ln(light_{t-1}) \quad (2)$$

$\Delta \ln(light)$ indicates the growth (log difference) in annual nighttime light. Values of $light_t$ and $light_{t-1}$ include a value of one, and are therefore equivalent to $\ln(light) \rightarrow \ln(light + 1)$.

¹⁸The geoquery application from AidData (Goodman et al., 2019) is used as a tool to obtain data from multiple sources for some of the controls at the ADM2 level.

$$\begin{aligned}
\Delta \ln(\text{light}_{crt}) = & \alpha + \beta \text{Aid}_{crt-1} + \sigma \text{PCDistrict}_{crt} + \lambda \text{PCCountry}_{ct} + \\
& \gamma \text{Aid}_{crt-1} \times \text{PCDistrict}_{crt} + \rho \text{Aid}_{crt-1} \times \text{PCCountry}_{ct} + \\
& \delta_{ct} + \psi_{rt} + X_{crt} + \kappa_c t + \chi_r t + \eta_r + \tau_t + \epsilon_{crt}
\end{aligned} \tag{3}$$

where $\Delta \ln(\text{light}_{crt})$ represents the change in average annual nighttime-light intensity in country c , district r , at time t . Aid_{crt-1} is the logarithm of World Bank aid disbursed to a district in period $t-1$, allowing for a delayed association with growth.¹⁹ PCCountry_{ct} equals one if the country is classified as post-conflict. Its coefficient, λ , measures the recovery differential for districts in post-conflict countries without locally recorded fighting relative to districts in peaceful countries. PCDistrict_{crt} equals one if a district is located in a post-conflict country and recorded positive conflict exposure (Intensity Index $II_{rt} > 0$) during the previous conflict episode. Its coefficient, σ , is the additional recovery differential associated with direct local exposure. The total recovery differential for an exposed post-conflict district is therefore $\lambda + \sigma$.

The coefficient β measures the marginal association between aid and growth in peaceful districts. The interaction $\text{Aid}_{crt-1} \times \text{PCCountry}_{ct}$ measures the additional aid differential for districts in post-conflict countries without locally recorded fighting, while $\text{Aid}_{crt-1} \times \text{PCDistrict}_{crt}$ measures the further differential associated with direct exposure. The total marginal aid association for an exposed post-conflict district is therefore $\beta + \rho + \gamma$. The vectors δ_{ct} and ψ_{rt} contain the remaining conflict-to-peace category indicators at the country and district levels, with *peace* omitted as the comparison category. Further, X_{crt} is a vector of country- and district-level controls. District fixed effects η_r absorb time-invariant local heterogeneity, while year fixed effects τ_t capture common shocks. Country- and district-specific linear trends, $\kappa_c t$ and $\chi_r t$, account for heterogeneous development paths.

As mentioned in Section 3.2, there is a longstanding debate regarding endogeneity concerns in the relationship between aid and growth. Aid is lagged by one year to allow time for disbursements to affect economic activity.²⁰ District fixed effects are included to control for time-invariant unobservable heterogeneity, such as geographic characteristics, long-term ethnic composition and historic differences.²¹ It ensures that any static differences across countries and districts do not confound the estimates. Additionally, I incorporate district- and country-specific linear time trends to account for divergent development paths as suggested by Gehring et al. (2022). These trends control for systematic variation in development trajectories stemming from historical, ethnic or structural differences. Any results thus focus on deviations from the expected long-term trends. In some stricter specifications, I include country-year fixed effects to control for dynamic factors at the national level, such as technical or monetary assistance to the government, national elections

¹⁹ Aid_{crt-1} includes an added value of 1 to allow for districts that did not receive any aid disbursements. The robustness checks confirm that this approach remains robust when aid is transformed using an inverse hyperbolic sine (ihs) transformation instead of a logarithmic function

²⁰Table A.5 in the Appendix shows outcomes using different lag-structures.

²¹District fixed effects absorb time-invariant district characteristics, including any time-invariant country-level factors.

or macroeconomic shocks. This approach isolates the remaining explanatory variation to within-district changes over time. While this empirical strategy does not strictly establish causality, it mitigates concerns about endogeneity. As a complementary assessment of remaining aid-allocation concerns, I report two shift-share instrumental-variable specifications in Appendix B.8. The appendix discusses their construction, identifying assumptions, first stages, and interpretation.

4 Results

I first report how post-conflict recovery varies with local exposure to past fighting. I then examine whether the association between aid and growth differs across the same groups, before interpreting the recovery and aid coefficients jointly in the full baseline specification.

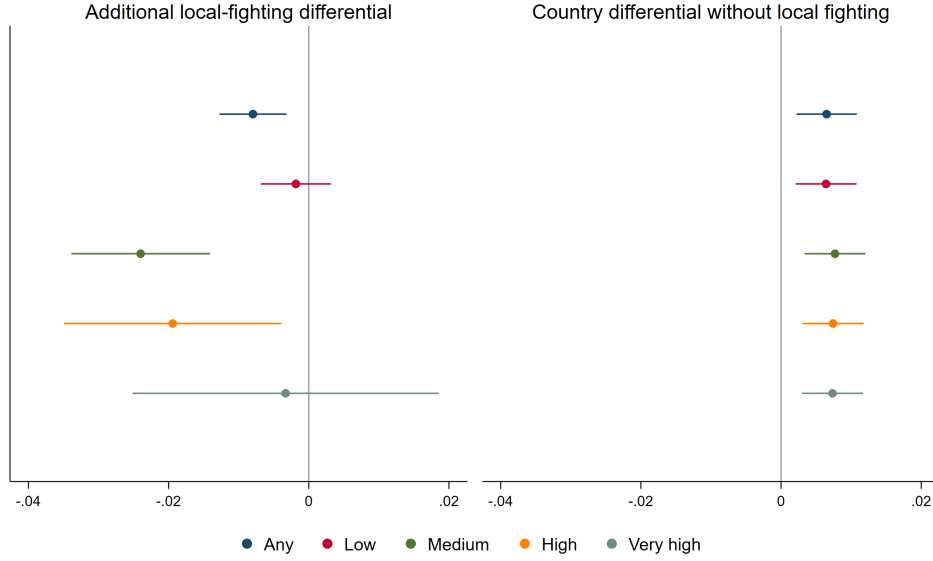
4.1 Heterogeneity of Post-Conflict Recovery

Figure 3 reports the *PCDistrict* coefficients in the left panel and the *PCCountry* coefficient in the right panel. The latter measures the growth differential between districts in post-conflict countries without local fighting and districts in peaceful countries. The former measures the additional differential associated with direct exposure. The *PCCountry* estimate is positive and significant: districts without locally recorded fighting grow on average 0.65 percentage points faster than districts in peaceful countries.

There are several possible explanations for this pattern. Because these districts did not experience direct fighting, they avoided the most severe destruction of physical and human capital. At the same time, they may benefit from reconstruction dynamics and population inflows during and after the conflict, which can increase local economic activity. They may also be better positioned to take advantage of institutional improvements following the end of conflict. The left panel shows the coefficients for the *PCDistrict* variable. The first coefficient, labelled *any*, captures the effect for districts that experienced any fighting during the previous conflict episode. It shows that districts exposed to fighting exhibit significantly lower nighttime light growth in the aftermath of war relative to non-fighting districts in post-conflict countries. The figure further reveals substantial heterogeneity by past fighting intensity. Districts with only mild exposure experience smaller effects, whereas districts with medium and high conflict intensity show pronounced and significant reductions in growth, lagging behind or even shrinking relative to their non-violent counterparts.

These results point to meaningful economic scarring in districts that were directly exposed to fighting. Districts with moderate to high wartime exposure lose between two and six times the annual growth rate of the average African district, indicating a substantial recovery penalty. One possible explanation is a local war-ruin effect: districts that were directly affected by violence suffered more severe destruction of physical and human capital and may also have lower absorptive capacity during the recovery period than districts that stayed out of the fighting. Overall, this first set of results shows clear subnational heterogeneity in post-conflict recovery depending on past exposure to violence. Post-conflict recovery is therefore not a country-

Figure 3: Post-conflict recovery by fighting intensity (relative to peaceful districts)



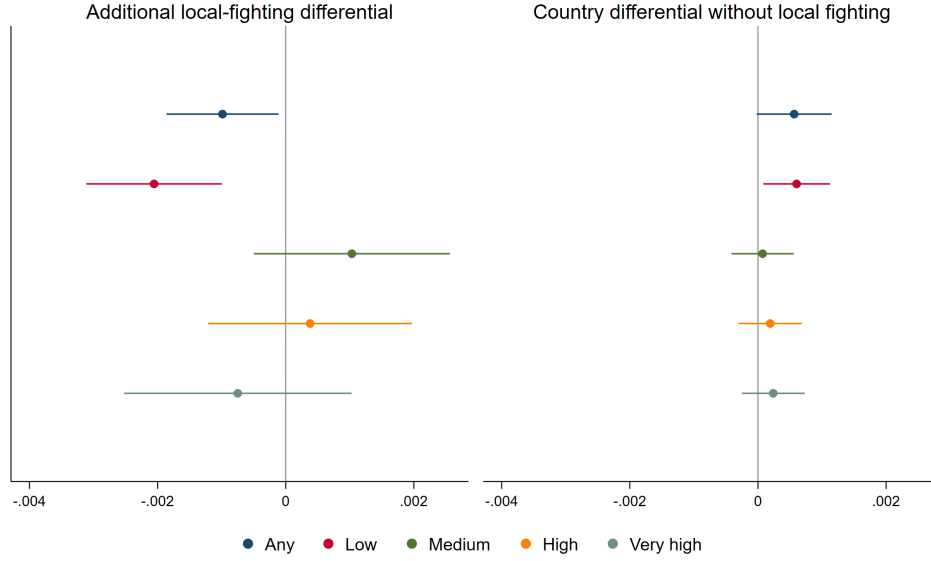
Notes. The figure reports unsummed coefficients and 95% confidence intervals from the baseline specification. The left panel shows the additional recovery differential associated with local fighting, conditional on the intensity category shown in the legend. The right panel shows the post-conflict-country differential for districts without local fighting relative to districts in peaceful countries. The total recovery differential for a district with local fighting is the sum of the corresponding country- and district-level coefficients; that sum is not plotted here. Standard errors are clustered at the district level.

level phenomenon, but a local one: districts that avoid fighting grow faster than peaceful benchmarks, while exposed districts experience persistent economic scarring. The recovery estimates raise a related question: whether the association between aid and growth also differs across these local environments.

Figure 4 reports differences in the link between aid and nighttime-light growth conditioned on past conflict intensity. The right panel shows the additional differential for districts in post-conflict countries without local fighting; the left panel shows the differential linked to direct exposure, conditional on the intensity category. It shows that for districts with low levels of conflict, the aid differential is mitigated compared to the peaceful baseline. While overall districts in a post-conflict country have a positive aid differential, however the total effect largely depends on a districts individual exposure. Overall, the relationship between aid and growth varies with local conflict conditions. The distinct result for low-intensity exposure highlights why local recovery environments matter when geographically targeted assistance is assessed, without implying a common post-conflict aid effect across all exposed districts.

Table 1 reports the full set of results from the baseline specification. Column (1) presents the baseline model, while columns (2) to (5) differentiate post-conflict districts by bins of past conflict intensity. The omitted category in all specifications consists of districts in peaceful countries. The coefficient on *PCCountry* therefore captures the differential for post-conflict districts without local fighting relative to peaceful

Figure 4: Aid-growth associations by fighting intensity (relative to peaceful districts)



Notes. The figure reports unsummed coefficients and 95% confidence intervals from the baseline specification. The left panel shows the additional aid differential associated with local fighting, conditional on the intensity category shown in the legend. The right panel shows the post-conflict-country aid differential for districts without local fighting relative to districts in peaceful countries. The aid association for a district with local fighting combines the main aid coefficient and both post-conflict interaction terms; that total is not plotted here. Standard errors are clustered at the district level.

districts. The *PCDistrict* coefficient captures the additional differential for districts with local fighting.

In columns (2) to (5), the *PCDistrict* indicator is defined separately for each intensity bin. In each column, the dummy equals one only for districts whose past conflict exposure falls within the respective bin, while the remaining bins are included as controls. The *PCDistrict* coefficients can therefore be interpreted as the additional differential associated with a given level of past conflict exposure relative to post-conflict districts without local fighting. The total recovery differential for an exposed district is the sum of *PCCountry* and *PCDistrict*. Across most intensity bins, this sum is substantially smaller—and often negative—indicating persistent local economic scarring. The interaction terms capture how the aid association varies across the same groups. The coefficient on *Aid* is the marginal association between aid and nighttime-light growth in peaceful districts. $Aid \times PCCountry$ captures the additional differential for post-conflict districts without local fighting, while $Aid \times PCDistrict$ captures the further differential associated with local fighting. The total marginal aid association for an exposed post-conflict district is therefore the sum of all three components. Formally, the total recovery differential comprises $\beta_{PCCountry} + \beta_{PCDistrict}$, and the marginal aid association comprises $\beta_{Aid} + \beta_{Aid \times PCCountry} + \beta_{Aid \times PCDistrict}$.

In terms of magnitude, the recovery estimates are economically meaningful. The coefficient on *PCCountry* implies that post-conflict districts without local fighting grow by about 0.65 to 0.77 percentage points

Table 1: Main Results - Post-Conflict Recovery

<i>Dependent variable: $\Delta \ln(light_{crt})$</i>					
Intensity	Any	Low	Medium	High	Very high
$\ln(Aid_{t-1})$	0.0002** (0.0001)	0.0002* (0.0001)	0.0002* (0.0001)	0.0002* (0.0001)	0.0002* (0.0001)
Post-Conflict District	-0.0080*** (0.0024)	-0.0018 (0.0025)	-0.0240*** (0.0050)	-0.0194** (0.0079)	-0.0033 (0.0111)
$\ln(Aid_{t-1}) \times$ Post-Conflict District	-0.0010** (0.0004)	-0.0021*** (0.0005)	0.0010 (0.0008)	0.0004 (0.0008)	-0.0007 (0.0009)
Post-Conflict Country	0.0065*** (0.0022)	0.0064*** (0.0022)	0.0077*** (0.0022)	0.0074*** (0.0022)	0.0073*** (0.0022)
$\ln(Aid_{t-1}) \times$ Post-Conflict Country	0.0006* (0.0003)	0.0006** (0.0003)	0.0001 (0.0002)	0.0002 (0.0003)	0.0002 (0.0002)
Observations	135025	135025	135025	135025	135025
Districts	5401	5401	5401	5401	5401
District FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Country-Year FE					
District-specific Trends	✓	✓	✓	✓	✓
Country-specific Trends	✓	✓	✓	✓	✓
District Controls	✓	✓	✓	✓	✓
Country Controls	✓	✓	✓	✓	✓
Dep. Var. Mean	0.004	0.004	0.004	0.004	0.004
Dep. Var. Std.	0.151	0.151	0.151	0.151	0.151
R2	0.157	0.157	0.157	0.157	0.157

Notes: The standard errors in parentheses are clustered at the district level. Post-Conflict District indicates districts that experienced fighting during the previous conflict episode, while Post-Conflict Country indicates districts located in post-conflict countries. The omitted category consists of districts in peaceful countries. Columns correspond to different bins of past conflict intensity. In each column, the Post-Conflict District dummy equals one only for districts whose past conflict intensity falls within the respective bin, while dummies for the remaining bins are included as controls. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

faster than districts in peaceful countries, compared to an average annual growth rate of about 0.4 percentage points in the sample. By contrast, directly exposed districts face substantial recovery penalties. Relative to post-conflict districts without local fighting, the additional penalty amounts to about 0.8 percentage points for any fighting, and up to around 2.0 to 2.4 percentage points for medium and high intensity conflict. These differentials are large relative to typical annual growth rates, indicating persistent and economically meaningful local scarring. The baseline aid association is smaller in magnitude: a one-unit increase in log aid is associated with approximately 0.02 percentage points higher growth in peaceful districts. This association becomes weaker or even negative for districts with low levels of past conflict exposure. Taken together, these estimates show that post-conflict recovery is highly heterogeneous across districts. Areas directly exposed to fighting experience persistent growth losses, consistent with local scarring effects, while districts without local fighting benefit from a positive peace dividend. The association between aid and growth also varies with past conflict exposure: it is less favorable in districts with low levels of exposure and slightly more positive in districts without local fighting.

Identification. All specifications include district and year fixed effects, as well as district- and country-specific time trends. Additional controls include precipitation, temperature, population levels, population density, and lagged nighttime light at the district level, as well as population growth and density at the country level. Identification relies on within-district variation in nighttime light growth over time, rather than cross-sectional differences in levels. Although economic activity may itself influence conflict, this endogeneity concern is less immediate here because the analysis links subsequent nighttime light growth during the post-conflict decade to exposure during the preceding war, rather than examining the contemporaneous relationship between economic activity and conflict events. District and year fixed effects, heterogeneous trends, and time-varying controls further mitigate concerns that persistent differences in economic conditions or divergent development paths drive the estimated recovery patterns.

A remaining concern is the potential endogeneity of aid allocation. To address this, aid enters the specification with a lag, allowing for delayed effects on growth. In addition, the inclusion of fixed effects and district- and country-specific trends absorbs persistent differences in development levels, conflict exposure, and targeting patterns. As a result, identification comes from deviations in aid disbursements from a district’s expected trajectory, rather than from cross-sectional or long-run differences. Potential bias would therefore require donors to systematically anticipate short-run, district-level growth shocks beyond what is captured by the fixed effects and trends. The limited temporal and spatial precision of aid allocation discussed in Section 3.2, mitigates this concern. Appendix B.8 reports two shift-share instrumental-variable specifications as complementary evidence.²² The results should accordingly be interpreted as suggestive evidence on differential recovery dynamics and conditional relationships between aid and growth across districts, rather than as estimates of causal mechanisms. The next section assesses the robustness of these findings.

4.2 Robustness and Sensitivity

Alternative Specifications. Table 2 represents various alternative specifications. For comparison reasons, any estimates are based on “any” intensity at the district level, and *PCDDistrict* can therefore be interpreted as the additional differential for post-conflict districts with local fighting. Column (1) represents the baseline specification, corresponding to Column (1) of the baseline results. It shows that across specifications with different sets of controls included and different sets of fixed effects, the results are largely stable in magnitude and significance. Column (2) indicates the results without any interaction terms. Estimates are stable in terms of significance, and slightly larger in terms of magnitude for the post-conflict variables. Columns (3) to (5) indicate estimates with different specifications of controls. Column (6) indicates the results using country-year fixed effects. This specification is very demanding as it absorbs any country-year variation as well as district-specific time trends. It therefore relies on within-district variation that is not

²²The instruments impose additional validity conditions and may identify effects for different margins and periods of aid allocation than the baseline specification. I therefore treat the estimates in Table A.20 as supportive evidence rather than as a substitute identification strategy; their construction, and specific limitations are discussed in the appendix.

Table 2: Robustness - Alternative specifications and timeframes

<i>Dependent variable: $\Delta \ln(light_{crt})$</i>								
OLS Specifications	(1)	(2)	(3)	(4)	(5)	(6)	1995–2015	2000–2020
$\ln(Aid_{t-1})$	0.0002** (0.0001)	0.0002** (0.0001)	0.0001 (0.0001)	0.0002** (0.0001)	0.0000 (0.0001)	0.0001 (0.0001)	0.0004*** (0.0001)	0.0003** (0.0001)
Post-Conflict District	-0.0080*** (0.0024)	-0.0103*** (0.0024)	-0.0035* (0.0021)	-0.0057*** (0.0021)	-0.0036* (0.0021)	-0.0052** (0.0023)	0.0035 (0.0026)	-0.0066** (0.0028)
$\ln(Aid_{t-1}) \times$ Post-Conflict District	-0.0010** (0.0004)		-0.0008** (0.0004)	-0.0009** (0.0004)	-0.0008** (0.0004)	0.0007 (0.0004)	-0.0007 (0.0004)	-0.0011** (0.0005)
Post-Conflict Country	0.0065*** (0.0022)	0.0078*** (0.0022)	0.0073*** (0.0018)	0.0045** (0.0018)	0.0075*** (0.0018)		-0.0048** (0.0022)	0.0143*** (0.0030)
$\ln(Aid_{t-1}) \times$ Post-Conflict Country	0.0006* (0.0003)		0.0007*** (0.0003)	0.0005** (0.0003)	0.0007*** (0.0003)	0.0000 (0.0003)	0.0001 (0.0003)	0.0002 (0.0003)
Observations	135025	135025	135100	135025	135100	135025	108020	113421
Districts	5401	5401	5404	5401	5404	5401	5401	5401
District FE	✓	✓	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓		✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Country-Year FE						✓		
District-specific Trends	✓	✓				✓	✓	✓
Country-specific Trends	✓	✓					✓	✓
District Controls	✓	✓		✓		✓	✓	✓
Country Controls	✓	✓			✓		✓	✓
Dep. Var. Mean	0.004	0.004	0.004	0.004	0.004	0.004	0.015	0.003
Dep. Var. Std.	0.151	0.151	0.151	0.151	0.151	0.151	0.114	0.158
R2	0.157	0.157	0.088	0.105	0.089	0.264	0.140	0.171

Notes: The standard errors in parentheses are clustered at the district level. Post-Conflict District indicates districts that experienced fighting during the previous conflict episode, while Post-Conflict Country indicates districts located in post-conflict countries. All columns use the indicator for any direct wartime exposure. Columns vary the controls, fixed effects, trends, and estimation period as indicated. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

explained by a district's general development trend. It shows that despite the demanding specification, districts with local fighting do experience significant scarring in growth rates. This demanding specification does not allow a separate interpretation for post-conflict districts without local fighting, as that variation is captured by the country-year fixed effects. Therefore, it is expected that there is little variation in the corresponding interaction with aid. It also shows that the other estimations involving aid as an explanatory variable lose power. This does not necessarily mean that aid has no impact at the local level, but rather that its variation is largely explained by macro-level shocks, given the specific development paths of districts. If anything, this amplifies the need to consider the local context. Columns (7) and (8) re-estimate the model over 1995–2015 and 2000–2020, respectively. While the earlier sample period coincides with the availability of AidData, the earlier periods make it more likely that there is no detectable nighttime light and that data might be more erroneous in earlier years. Furthermore, as conflict data is only available since from 1989 on, some early post-conflict periods might not yet be captured. These reasons might explain the non-significant coefficient for post-conflict districts in Column (7) and the sign-change for the Post-Conflict Country variable. The later window indicating results in Column (8) is informative because observations with no detectable nighttime light are concentrated in earlier years before 2000 and are rare in later years.

Over 2000–2020, the negative post-conflict differential for directly exposed districts and its interaction with aid remain statistically significant, while the post-conflict coefficient for districts not directly exposed to fighting remains positive. The central local recovery contrast is therefore not driven solely by early-period transitions into or out of zero nighttime light. However, as shown in Column (7), the earlier estimation window captures different variation. The corresponding estimates using the alternative aid and conflict datasets are reported in Table A.6 and Table A.7, respectively.

Confounders. Two concurrent interventions are particularly relevant in post-conflict settings: UN peacekeeping operations and Chinese development finance. Both may respond to local conflict conditions while also affecting subsequent economic activity. This makes them potentially important omitted factors, but also potentially endogenous controls: conditioning on interventions deployed in response to conflict could absorb part of the post-conflict recovery process itself. I therefore exclude them from the baseline specification and introduce them jointly as a robustness check. Using the geocoded deployment data of Cil et al. (2020), I measure whether peacekeeping personnel were present in a district in a given year. I additionally account for local Chinese development finance, which constitutes an important alternative source of external assistance. Existing evidence finds that neither Chinese nor World Bank aid systematically increases local conflict, although World Bank aid is more strongly associated with conflict reductions (Gehring et al., 2022). Chinese development finance has also been linked to positive economic growth (Dreher et al., 2021), while its contrasting national and subnational health effects suggest that resources may be fungible across locations (Cruzatti et al., 2023).

Table A.8 in Appendix B.3 reports the baseline specifications augmented with both measures. Peacekeeping presence is positively and significantly associated with nighttime-light growth across all conflict-intensity definitions. This pattern is consistent with greater local economic activity in districts hosting peacekeeping personnel. The coefficient on Chinese aid is positive but statistically insignificant in every specification. The estimated post-conflict recovery patterns and their interactions with World Bank aid remain very similar in magnitude and statistical significance. The main results therefore do not appear to be driven by the omission of these concurrent international interventions.

Transformations. The transformation of nighttime light determines both the estimand and the interpretation of the coefficients. The baseline outcome is the annual log-difference of nighttime light plus one. Coefficients on the post-conflict indicators therefore measure conditional differences in the annual change of nighttime light, while coefficients on log aid measure how this change varies with proportional differences in aid. This outcome is close to a conventional log growth rate when nighttime light is positive and sufficiently large, but it is not an exact percentage growth rate for observations at or near zero.

Systematically excluding observations with no detectable nighttime light could itself introduce selection into the outcome. Adding one before taking logarithms retains these observations, which are substantively

meaningful in conflict-affected districts. This adjustment does not, however, eliminate the functional-form concern emphasized by Chen and Roth (2024): the log-plus-one transformation combines changes at the extensive and intensive margins. In this application, nighttime light is recorded on a fixed sensor scale from zero to 63 rather than in an arbitrary monetary denomination.²³ Column (2) of Table A.9 replaces the log change with the change in the inverse hyperbolic sine, which behaves approximately linearly near zero and logarithmically at large values. Under this transformation, the coefficient for directly exposed post-conflict districts and its interaction with aid retain their negative signs but are no longer statistically significant; the interaction is similar in magnitude, whereas the district coefficient is smaller. The positive coefficient for other districts in post-conflict countries and its interaction with aid remain statistically significant, although their magnitudes change. The comparison therefore preserves the qualitative peace-dividend pattern outside directly exposed districts but provides weaker evidence for differential scarring and for a differential aid association in directly exposed districts.

The construction of the harmonized nighttime-light series creates a separate measurement concern. Martínez (2022) identifies discontinuities around the harmonization years 2014 and 2017 and recommends imputing the affected observations so that these measurement breaks do not enter measured growth.²⁴ Table A.10 compares this baseline correction with a series that imputes only 2014 and a series that uses neither imputation; all three columns retain the same observations. Without the full correction, the estimates are somewhat different, illustrating that the documented measurement breaks affect inference when they remain in the outcome. The comparison therefore supports the Martínez correction as a consequential measurement adjustment while making transparent which interaction estimates are most sensitive to the untreated discontinuities.

Table A.11 assesses whether the results depend on the transformation of aid by replacing its logarithm with the inverse hyperbolic sine. The signs, magnitudes, and statistical significance of the reported coefficients remain nearly unchanged. Table A.5 then varies the lag of aid from one to four years. The aid coefficient is no longer statistically distinguishable from zero beyond the one-year lag, whereas the negative interaction between aid and direct exposure to fighting remains statistically significant and increases in magnitude as the lag lengthens. One possible interpretation is that longer lags increasingly capture aid disbursed during conflict, particularly for observations in the early post-conflict period: with a four-year lag, the relevant disbursement may predate the end of the conflict. This pattern is therefore consistent with

²³Chen and Roth (2024) emphasize that estimates based on logarithmic transformations with zeros can depend on the units of measurement. Rescaling is particularly informative when units are conventional, such as reporting monetary values in dollars rather than thousands of dollars. Rescaling nighttime light remains a valid diagnostic of scale dependence, but alternative normalizations of its bounded sensor scale are not equally natural reporting conventions because the normalization changes the weight assigned to transitions between no detectable light and positive light. I therefore do not treat arbitrary rescalings as separate robustness outcomes. The 2000–2020 estimates in Column (8) of the alternative-specifications table provide a more substantively relevant check because zero-light observations are rare in this later period. Poisson pseudo-maximum likelihood provides another approach for nonnegative level outcomes when the estimand is a proportional change in the conditional mean, as discussed by Santos Silva and Tenreiro (2006) and Chen and Roth (2024). It is not a like-for-like estimator for the baseline outcome, however, because annual changes in transformed nighttime light can be negative. Applying it would require replacing the growth outcome with nighttime-light levels and would therefore change the estimand.

²⁴Following this procedure, the baseline series replaces the change in nighttime light in each affected year. For 2014, the imputed change averages 2012–2013 and 2015–2016; for 2017, it averages 2015–2016 and 2018–2019.

aid delivered during conflict being potentially less conducive to subsequent recovery in directly exposed districts. Because the specifications with longer-lags also contain fewer observations, differences across columns may partly reflect changes in sample composition.

Alternative Data. As discussed in Section 3.1, UCDP and ACLED differ in their coverage and classification of political violence. These differences affect the reported number and location of fatalities and therefore potentially alter both the assignment of districts to conflict categories and the conflict-intensity index used during the post-conflict period. ACLED coverage for most African countries begins in 1997, so conflicts that ended before then cannot be incorporated into the subsequent post-conflict classification. This is particularly consequential during the early 2000s because the index retains information about conflict exposure for up to ten years. I therefore compare the two conflict datasets over their common estimation period from 2000 to 2020 in Table A.7. The pattern of estimates is largely robust to the alternative conflict data. Across the heterogeneous exposure categories, most coefficients correspond in sign, order of magnitude, and statistical significance. One systematic difference concerns the interaction between aid and the post-conflict-country indicator, which is close to zero in the UCDP specifications but consistently negative and statistically significant with ACLED. A possible contributor to this difference is the comparatively low number of fatalities recorded by ACLED for episodes such as the Liberian civil war, which can shift both post-conflict classification and intensity. The comparison supports the robustness of the heterogeneous local recovery pattern and most of the corresponding aid estimates to the choice of conflict dataset.

As discussed in Section 3.2, the baseline aid measure is drawn from GoDAD, which extends the temporal coverage of geocoded World Bank disbursements beyond that available in AidData. Because AidData has been used extensively in the subnational aid literature, it provides a useful benchmark for assessing whether the results depend on the choice of aid source. Table A.6 compares estimates based on GoDAD and AidData over the common period from 1995 to 2015, holding the estimation sample and specification constant.

The two datasets produce the same positive and statistically significant main aid coefficient, and the post-conflict coefficients correspond closely in sign and magnitude across the heterogeneous exposure categories. The interaction estimates exhibit greater variation. In particular, the interaction between aid and the post-conflict-country indicator is close to zero when aid is measured with GoDAD but negative and statistically significant when AidData is used. Some interactions between aid and direct conflict exposure also differ in magnitude or precision, although these differences do not follow a uniform pattern across the intensity categories. The discrepancies may reflect differences in project coverage, geocoding, or the temporal and spatial allocation of disbursements across multi-year and multi-location projects. GoDAD's more recent construction and district-year structure may reduce some forms of measurement error, but the comparison itself cannot establish which source measures local disbursements more accurately. Overall, the main association between aid and nighttime-light growth and the estimated post-conflict recovery pattern are robust to the choice of aid dataset, whereas the differential association of aid across post-conflict groups

is more sensitive to how local aid flows are measured.

Samples and Sensitivity. Clemens et al. (2012) demonstrate that estimates in the aid literature can be sensitive to the countries and periods included in the analysis (Boone, 1996; Burnside & Dollar, 2000; Rajan & Subramanian, 2008). The subnational panel used here reduces reliance on cross-country variation, but it does not eliminate the possibility that a small number of large aid recipients, distinctive conflict episodes, or correlated unobserved country characteristics exert disproportionate influence. Such sensitivity is substantively informative in this setting because it can reveal whether the balance between postwar scarring and a peace dividend differs across groups of countries. I therefore examine grouped country exclusions, restricted panels, and a leave-one-country-out exercise. The leave-one-country-out estimates retain the signs and broad magnitudes of the baseline coefficients, indicating that no individual country accounts for the overall pattern.

Table A.12 in Appendix B.6 compares the baseline with three grouped country exclusions. Column (2) excludes Egypt, Nigeria, South Africa, Algeria, and Morocco, the five largest African economies by aggregate output.²⁵ This restriction removes roughly one-third of the observations. The post-conflict-country coefficient turns negative, and directly exposed districts face an additional negative differential. The estimates thus no longer exhibit the baseline peace dividend among districts that avoided fighting. Instead, they point toward country-wide postwar scarring combined with an additional local recovery penalty—a pattern more consistent with the war-ruin hypothesis. The main aid coefficient is insignificant, whereas its positive interaction with the post-conflict-country indicator becomes larger. Once all three aid coefficients are combined, the point estimate for directly exposed districts remains positive.

Column (3) excludes Egypt, Morocco, Tunisia, and Algeria, removing approximately one-quarter of the sample and producing a similar recovery pattern. The post-conflict-country coefficient is again negative but insignificant, while direct exposure carries a significant additional penalty. Because this group overlaps substantially with the large-economy exclusion, the two exercises are not independent checks. By contrast, excluding the five countries with the highest GDP per capita in Column (4) preserves the positive post-conflict-country differential, although the negative district differential almost fully offsets it among directly exposed districts. Taken together, the estimates suggest that the baseline peace dividend is disproportionately supported by the largest African economies and North African countries rather than by the richest countries per capita. The relative disadvantage of directly exposed districts is more stable across these samples.

Countries that experienced conflict may also differ systematically from countries that remained peaceful throughout the sample. Panel A of Table A.13 therefore restricts the sample to the 19 countries that are classified as post-conflict at least once during the observation period, accounting for approximately half of all observations. The panel retains all years for these countries, so the relevant comparison is between their post-

²⁵The grouping is based on aggregate purchasing-power-parity-adjusted GDP in 2021 (World Bank, 2022).

conflict years and their own peaceful periods rather than between permanently distinct groups of countries. Within this more comparable set of countries, the positive post-conflict-country coefficient remains similar to the baseline. The phoenix-factor pattern therefore persists for districts that avoided direct fighting. At the same time, the pooled coefficient for any direct exposure is negative and statistically significant, while the additional recovery penalties associated with medium and high conflict exposure remain pronounced. The restricted panel therefore continues to show local scarring within an otherwise faster-growing post-conflict country. The main aid coefficient is somewhat larger and more precisely estimated than in the baseline. This pattern suggests that the general association between aid and growth dominates any differential association during post-conflict periods in this restricted panel.

Panel B in Table A.13 considers a narrower group of seven countries whose first observed World Bank aid year is the same as or later than their first observed post-conflict year.²⁶ By excluding countries with an observed World Bank aid relationship preceding their first post-conflict phase, this timing restriction mitigates concerns that earlier aid drives the transition into the post-conflict period, including concerns related to the potential effects of aid on conflict discussed in the literature (Gehring et al., 2022; Nunn & Qian, 2014). It also provides evidence from countries for which the data record no established World Bank aid relationship before the first observed post-conflict phase.²⁷

The post-conflict-country coefficient is negative across all intensity specifications, indicating a broad scarring pattern even among districts that were not directly exposed to fighting. At low, medium, and high intensities, the district-level differentials are too small to offset this country-wide penalty. Only the very-high-intensity differential is sufficiently positive for the combined point estimate to imply higher post-conflict growth. Within this restricted sample, the war-ruin pattern therefore dominates, while a phoenix-factor pattern appears only among the most intensely exposed districts. Sample composition may help explain this result. Liberia and Rwanda experienced highly concentrated episodes of severe violence over substantial parts of their territories, followed by comparatively long periods without renewed war, and are often regarded as prominent cases of postwar recovery. Their trajectories may therefore contribute disproportionately to the positive differential at very high intensity. Districts at all other intensity levels, as well as districts not directly exposed to fighting, retain the strong country-wide scarring pattern. Neither the main aid coefficient nor its interaction with the post-conflict-country indicator is statistically distinguishable from zero. The district-level aid interactions are also generally imprecise, although the medium-intensity interaction is positive and the very-high-intensity interaction negative at the ten-percent level. Overall, the panel reinforces the paper’s central distinction between country-wide post-conflict conditions and local exposure to fighting and shows that this heterogeneity persists in a sample that mitigates concerns about

²⁶The restriction is defined within the available aid data, which begin in 1995. The seven countries are Angola, Ethiopia, Liberia, Mozambique, Rwanda, Somalia, and South Africa.

²⁷Because the aid data begin only in 1995, the absence of an earlier recorded relationship does not establish the absence of earlier donor structures. Nevertheless, the restriction isolates countries that may only be beginning to establish large-scale donor relationships. It therefore mitigates concerns about aid endogeneity and the possible effects of earlier assistance on conflict, while providing suggestive evidence for a small and substantively distinctive group of countries.

the timing of observed aid.

5 Mechanisms and Heterogeneity

In the following section, I further explore different mechanisms and heterogeneity that might affect recovery outcomes during the post-conflict phase.

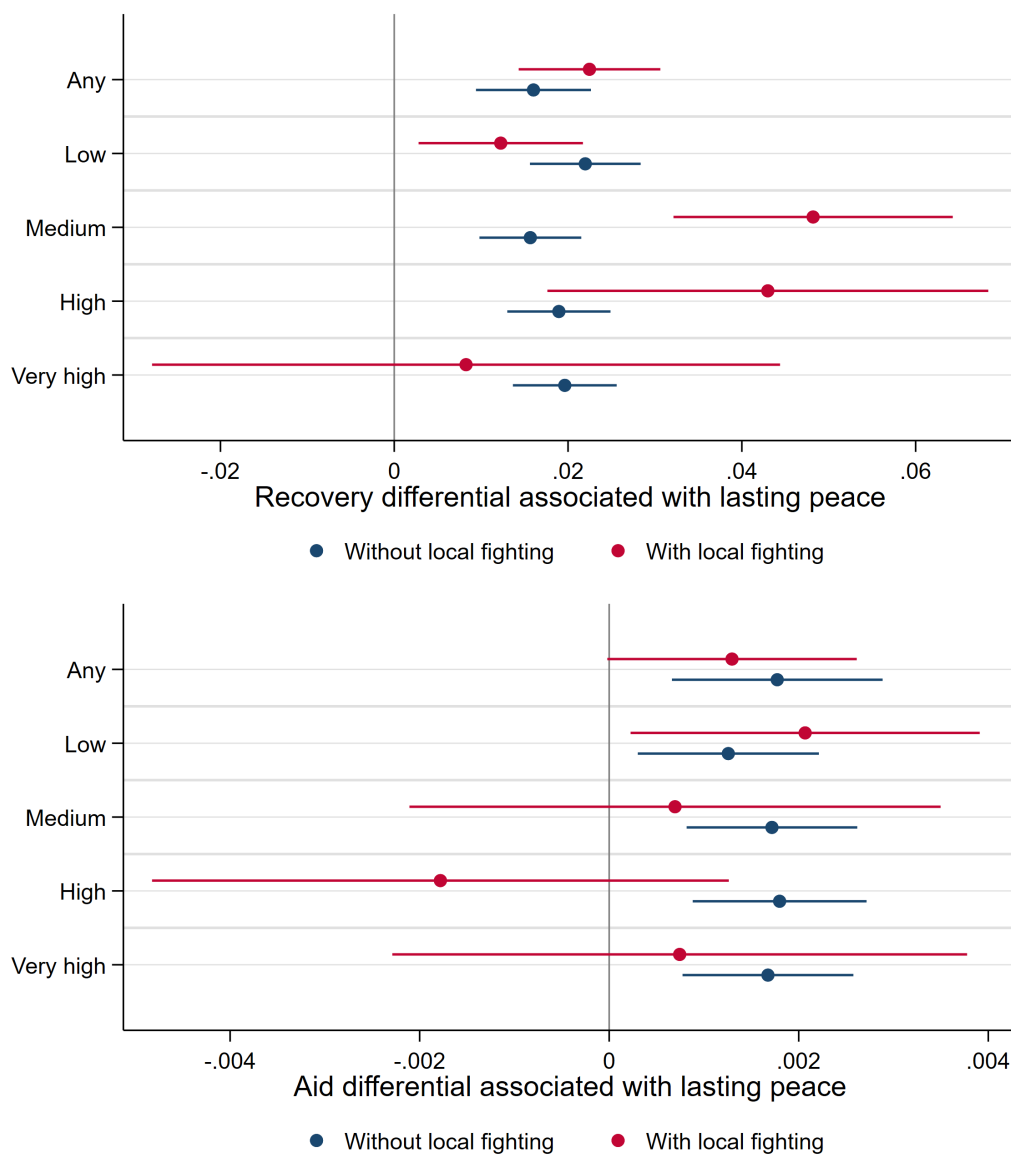
5.1 Persistence and Recurrence of Conflict

Lasting Peace. Whether peace endures or a country returns to violence is likely to shape the trajectory of post-conflict recovery. Investors may be more willing to commit capital when they expect peace to persist, while displaced people are more likely to return when renewed conflict appears less likely. Return migration can restore human capital, labor supply, and local demand. Durable peace may likewise encourage larger and more sustained reconstruction efforts, reinforcing local growth. Lasting peace should therefore be associated with stronger recovery dynamics.

Figure 5 summarizes these patterns in the sample of countries that experience at least one post-conflict period; Table A.14 reports the full estimates. The lasting-peace indicator equals one throughout a post-conflict period if the next observed non-post-conflict state is peace. It therefore distinguishes post-conflict periods that culminate in peace from those followed by renewed war or minor conflict. The model examines how the lasting-peace differential varies with past conflict intensity and aid. The positive lasting-peace coefficient more than offsets the negative post-conflict-country coefficient, implying a positive combined recovery differential for districts that were not directly exposed to fighting. Among directly exposed districts, lasting peace offsets most of the additional penalties associated with medium and high conflict intensity, leaving the combined point estimates close to zero. The pooled estimate, however, shows clearer direct scarring and provides less evidence that lasting peace offsets it. The estimates thus suggest that durable peace mitigates both country-wide and local postwar scarring, but that this differential mitigation is concentrated in particular intensity groups.

Aid is also more positively associated with nighttime-light growth during post-conflict periods that culminate in peace, irrespective of past conflict intensity. The additional differential for directly exposed districts is generally not statistically distinguishable from zero, although it is negative and statistically significant for high-intensity exposure. Because the lasting-peace classification depends on how the post-conflict period ultimately ends, this characteristic would generally not have been observable when aid was initially disbursed. The pattern therefore suggests that countries and affected districts which subsequently sustain peace not only experience stronger recovery but also provide conditions under which aid is more supportive of local growth.

Figure 5: Lasting Peace and Conflict Recovery



Notes. The upper panel reports the recovery differential associated with lasting peace, and the lower panel reports the corresponding aid differential. For post-conflict districts without local fighting, each point is the relevant country-level lasting-peace coefficient. For districts with local fighting, each point is the sum of the country- and district-level lasting-peace coefficients. The 95% confidence intervals for these sums are calculated from the full district-clustered variance-covariance matrix.

Timing. Figure 6 examines when post-conflict districts experience scarring or a peace dividend and whether this timing depends on the intensity of their wartime exposure; Table A.15 reports the full estimates. These temporal differences provide suggestive evidence on the reconstruction and institutional channels underlying recovery. To examine them, I condition the post-conflict indicators on three stages—

the early (years 1–3), middle (years 4–7), and late (years 8–10) post-conflict periods—while controlling for the remaining observations. The estimates show how the recovery and aid differentials associated with direct exposure vary over the post-conflict decade.

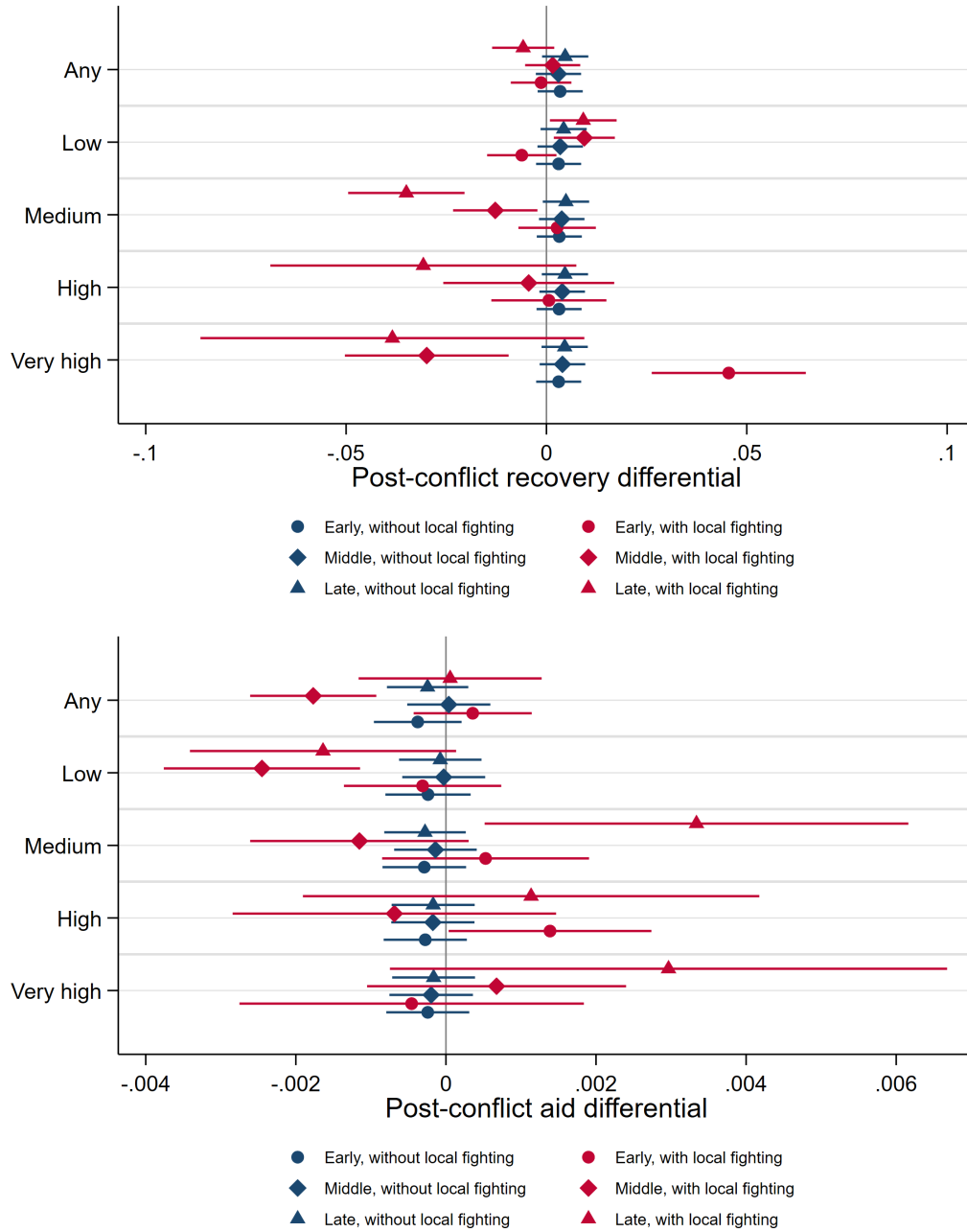
During the first three years, there is no significant peace dividend for districts not directly exposed to fighting. Instead, early recovery is uneven: low-intensity districts remain locally scarred, the differentials at medium and high intensity are insignificant, and only very-high-intensity districts exhibit a positive growth differential. This suggests that the benefits of peace do not spread evenly within countries. Reconstruction may first concentrate in the most heavily affected areas, allowing very-high-intensity districts to rebound while less exposed districts remain scarred. Aid does not yet provide broad relief. Among the intensity-specific estimates, its interaction is positive and significant only at high intensity; the pooled interaction for any direct exposure is also positive and significant at the ten-percent level. This accords with Collier and Hoeffler (2004)’s finding that, conditional on policy and institutions, post-conflict countries have greater absorptive capacity and aid is more productive. Although their country-level analysis locates exceptionally high absorption primarily in years 4–7, some heavily affected districts may benefit earlier.

During the middle post-conflict years—years 4–7—the pattern shifts rather than converges. Low-intensity districts move from early scarring to a positive growth differential. Yet their aid interaction becomes negative and significant, suggesting that recovery occurs despite, rather than through, a more favorable association with aid. By contrast, the combined point estimates imply lower nighttime-light growth than in peaceful districts at medium, high, and very high intensity. The district differentials are significant at medium and very high intensity but insignificant at high intensity, where the positive early aid differential also disappears. Thus, even when cross-country evidence points to greater absorptive capacity, local recovery and the aid-growth association remain conditioned by prior conflict intensity.

The late estimates may rely on less identifying variation because fewer post-conflict periods reach years 8–10. Nevertheless, the post-conflict-country coefficients are positive but generally not significant. The district differentials are negative from medium through very high intensity, although not strictly monotonic, suggesting persistent local scarring among more intensely exposed districts. Conversely, the aid interactions are positive at medium, high, and very high intensity but negative at low intensity.

Overall, both recovery and the association between aid and growth change over the post-conflict decade. Very-high-intensity districts experience their largest positive growth differential early, while the early positive aid differential appears at high intensity. Low-intensity districts move from initial scarring to a positive growth differential from the fourth year onward, despite a negative aid differential during the middle stage. Medium- and high-intensity districts perform worse primarily during the middle and late stages, while the aid association becomes more favorable later. These patterns underscore the importance of considering conflict intensity and time since conflict jointly when assessing local recovery and targeted interventions.

Figure 6: Timing of Post-Conflict Recovery



Notes. The upper panel reports post-conflict recovery differentials, and the lower panel reports the corresponding aid differentials, separately for the early (years 1–3), middle (years 4–7), and late (years 8–10) post-conflict periods. For districts without local fighting, each point is the post-conflict-country coefficient from the corresponding period-specific regression; the country indicator itself spans the full post-conflict period. For districts with local fighting, each point adds the period- and intensity-specific district coefficient to that country-level coefficient. The 95% confidence intervals for these sums are calculated from the full district-clustered variance-covariance matrix.

5.2 Spatial Spillovers and Reallocation

Spatial Spills of Conflict. The baseline framework distinguishes between country-wide post-conflict conditions and the additional differential associated with a district’s own exposure to fighting. This decomposition is also intended to capture spatial consequences within countries. Broad spillovers shared across districts should be absorbed largely by the country-wide post-conflict differential, while the remaining heterogeneity should primarily reflect differences in districts’ own fighting intensity. Nevertheless, district boundaries need not contain the consequences of war. Districts not directly exposed may still suffer from disrupted markets, damaged regional infrastructure, or displacement from nearby areas. Conversely, an inflow of people and economic activity may support their subsequent recovery. At the country level, de Groot et al. (2022) show that conflicts affect growth in neighboring peaceful countries, with the direction depending on the type of conflict. I therefore examine whether proximity to more intensely exposed districts generates additional variation in local post-conflict recovery.

To capture this channel, I construct spatial lags of neighboring districts’ post-conflict intensity. Neighbor relationships are defined by spherical distance between district centroids and do not depend on whether districts share a country. I consider four distance bands: 0–20, 20–50, 50–150, and 150–250 kilometers. Within each band, neighboring districts receive inverse-distance weights, so closer districts contribute more strongly. The weights are standardized within each district, which also gives each neighbor greater weight in areas with fewer neighbors. The resulting spatial lag is the weighted average of the post-conflict intensities experienced by all neighbors in a given band. I add this measure to the baseline specification and interact it with local aid to assess whether the association between aid and growth also varies with surrounding conflict intensity. Table A.16 reports the results.

The spillover-intensity coefficients are economically negligible across all four distance bands and generally display a negative tendency. The estimate is positive and insignificant between 50 and 150 kilometers, while the remaining estimates are negative; despite being statistically significant, their magnitudes remain extremely small. This does not imply that the consequences of conflict stop at district boundaries. By construction, the post-conflict-country coefficient already captures consequences common to districts within the same country, including broad within-country spillovers. The spatial measure instead tests whether the intensity of nearby fighting explains additional differences after accounting for these common conditions and a district’s own exposure. Its limited magnitude therefore supports the baseline decomposition: broad spatial consequences appear to be absorbed largely by the country-wide post-conflict differential, while the remaining heterogeneity primarily reflects differences in districts’ own fighting intensity. The interaction between aid and neighboring intensity is likewise negligible at short distances and small and negative in the two outer bands. Hence, surrounding conflict intensity does not appear to enhance the association between local aid and recovery.

Spatial Spills of Aid. Having found little additional role for neighboring conflict intensity, I next

examine whether aid disbursed in one district affects growth elsewhere. Such externalities can operate in either direction. Donor financing for infrastructure may improve regional market access or release domestic public funds for investment in neighboring districts. Conversely, better infrastructure and services in one location may draw workers and human capital away from surrounding districts, weakening their recovery. I examine these possibilities in the full sample by constructing inverse-distance-weighted spatial lags of aid disbursed in neighboring districts, using the same four distance bands, and interacting them with the post-conflict-country and directly exposed district indicators.

Table A.17 shows a positive association between neighboring aid exposure and growth in peaceful districts across all distance bands. For post-conflict districts without local fighting, the combined point estimates are also nonnegative, with a significant additional country differential only between 50 and 150 kilometers. The pattern changes among directly exposed post-conflict districts. Their additional spillover-aid differential is negative in every distance band and significant in all except the 20–50 kilometer band; the combined point estimates are also negative throughout. The results are therefore consistent with positive externalities in peaceful districts and post-conflict districts without local fighting but adverse spatial reallocation among districts carrying a direct legacy of conflict. Moreover, the magnitudes tend to increase rather than attenuate with distance. This pattern is more consistent with broader regional reallocation than with a purely local diffusion process, although the estimates do not identify the underlying mechanism. Overall, the results reinforce the argument that local post-conflict conditions shape not only recovery itself but also how the benefits of geographically targeted interventions extend across space.

5.3 Sectoral Allocation

The preceding results show that districts recover differently from their violent past depending on whether fighting occurred locally. This heterogeneity likely also conditions how well different types of projects support local economic recovery. Sectoral allocation and conflict exposure are, however, potentially endogenous to one another: if donors tailor aid to local needs, projects should be selected to match the damage and recovery constraints districts face. The aggregate descriptive evidence discussed above suggests that such tailoring cannot be assumed. I therefore examine whether aid directed to different sectors is differentially associated with recovery in post-conflict districts with and without local fighting.

Aid takes various forms and is distributed across different sectors. Projects in the GODAD database are classified into 11 sectoral categories, which can be aggregated into production, social infrastructure, and economic infrastructure. Donaubauer et al. (2019) examine whether sector-specific post-conflict aid affects some dimensions of infrastructure more than others and find that, across countries, post-conflict aid improves social infrastructure. Bitzer and Gören (2018) similarly estimate heterogeneous associations between aid sectors and nighttime-light growth at the grid-cell level, but do not cover all sectors or distinguish post-conflict locations. This study reverses the question by asking whether aid directed to particular sectors is differentially associated with nighttime-light growth across districts with different local exposure to fighting

within post-conflict countries.

Table A.18 reports these sectoral differentials. Among districts in post-conflict countries that were not directly exposed to fighting, the presence of economic-infrastructure projects is associated with particularly strong growth. By contrast, the interaction with direct conflict exposure is negative and statistically significant for each aggregate sector, indicating that the same sectoral project categories are less favorably associated with recovery where fighting occurred locally. This pattern places the aid results within the broader post-conflict interpretation: the performance of sectoral interventions appears to depend not only on the country-wide transition to peace, but also on persistent local legacies of direct exposure.

Figure 7 disaggregates these categories further; Table A.19 reports the full estimates. In post-conflict districts without local fighting, projects targeting health, government, energy, agriculture, industry, and communications have positive associations with nighttime-light growth once the sector main effect and the post-conflict-country sector differential are combined. For districts with local fighting, adding the district-level sector differential substantially reduces several of these associations and can turn the corresponding point estimates negative. The underlying district-level sector interactions are negative and statistically significant for education, health, government, agriculture, and industry. Thus, sectors associated with stronger recovery where the post-conflict peace dividend is not accompanied by direct local scarring do not necessarily perform similarly in districts that experienced fighting.

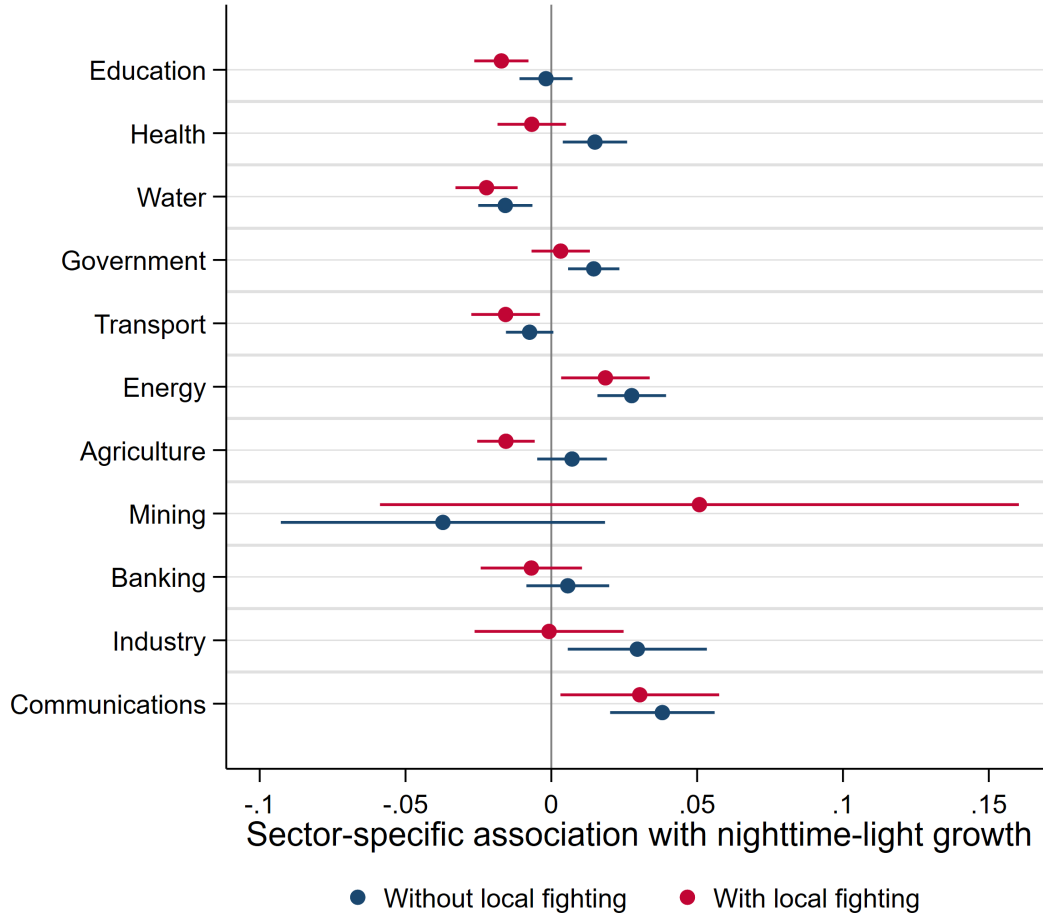
These associations should be interpreted cautiously because sectoral allocation may respond to local recovery conditions and some sector-specific post-conflict treatment cells may be sparse. Nevertheless, the results provide evidence of sector-specific heterogeneity at the subnational level that warrants further research. Hoeffler (2012) shows that post-conflict and peaceful countries receive very similar sectoral aid packages. Together with Donaubauer et al. (2019), the results here indicate that sectoral priorities should be differentiated not only between peaceful and post-conflict countries, but also within post-conflict countries, to account for local recovery constraints and avoid potentially adverse outcomes.

6 Conclusion

Recovery begins under a common national transition to peace, but it need not follow a common path within the country. This paper studies how local exposure to the preceding war is associated with subsequent district growth. It develops a country-level classification of post-conflict periods and a district-level intensity index that records whether fighting occurred locally and summarizes its intensity and timing.

The results show that a national peace dividend and local war scarring can coexist. Districts without locally recorded fighting exhibit higher nighttime-light growth than districts in peaceful countries, whereas directly exposed districts experience an additional recovery penalty that varies with fighting intensity. The positive country-wide differential is disproportionately supported by the largest African economies and North African countries. It remains, however, when the sample is restricted to countries that experience

Figure 7: Aid Subsectors and Post-Conflict Recovery



Notes. The figure reports summed sector-specific associations with nighttime-light growth. For post-conflict districts without local fighting, each point sums the sector main effect and the interaction between that sector and the post-conflict-country indicator. For districts with local fighting, each point additionally includes the interaction between the sector and the district-level fighting indicator. The 95% confidence intervals for both sums are calculated from the full district-clustered variance-covariance matrix.

a post-conflict period and their post-conflict years are compared with their own peaceful periods. Sample composition therefore affects the average country-wide pattern without removing the within-country difference associated with local exposure.

Recovery also varies over the post-conflict period and with the durability of peace. Very-high-intensity districts exhibit their strongest positive differential early but follow a more adverse trajectory later, while low-intensity districts display the opposite pattern. Post-conflict periods that culminate in durable peace are associated with stronger recovery among districts without local fighting and with less scarring at some exposure levels. Because lasting peace is observed only as the post-conflict period unfolds, these estimates do not identify the conditions that sustain peace or the mechanisms through which they affect recovery.

The geocoded World Bank data show that aid is positively associated with growth in peaceful districts

and post-conflict districts without locally recorded fighting, while the combined estimate is negative for low-intensity districts. The aid association also changes over the post-conflict decade and is more positive in periods that ultimately culminate in peace. Project categories associated with stronger growth in districts spared from fighting are less favorably associated with growth in directly exposed districts, and neighboring aid is associated with different spillover patterns across recovery environments. Because aid and its sectoral allocation are endogenous, I interpret these patterns as conditional associations. They nevertheless show why national post-conflict status alone is insufficient for interpreting geographically targeted aid.

Local wartime exposure should therefore be considered when post-conflict trajectories are analyzed and when interventions are planned, implemented, and assessed. For randomized evaluations spanning differently exposed locations, predetermined exposure measures can inform stratification, balance assessment, and pre-specified heterogeneity analysis without changing the estimand identified by randomization. For larger aid projects, the same information can clarify the recovery conditions under which a project operates and how its outcomes should be interpreted. National peace does not create a single post-conflict economy: local wartime histories continue to shape recovery and the conditions under which development assistance operates.

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A Appendix Graphs and Figures

A.1 Descriptive Graphs

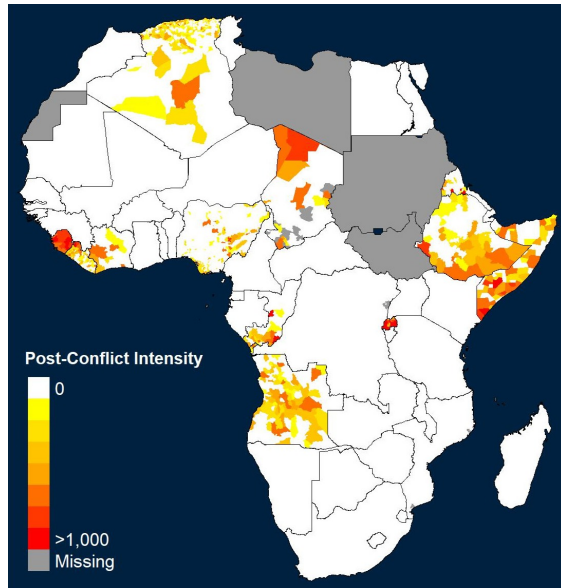


Figure 8: Post-Conflict intensity exposure in 2005 based on author's intensity index. Source: UCDP/Author's estimations.

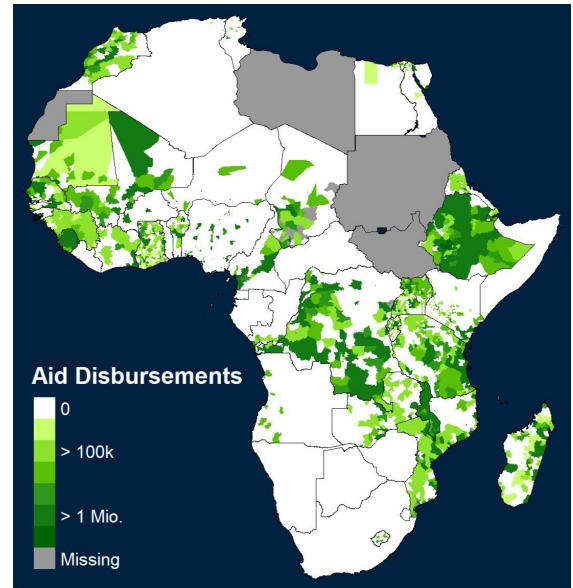


Figure 9: World Bank aid disbursements at ADM2 level in 2005. Source: AidData/Author's estimation.

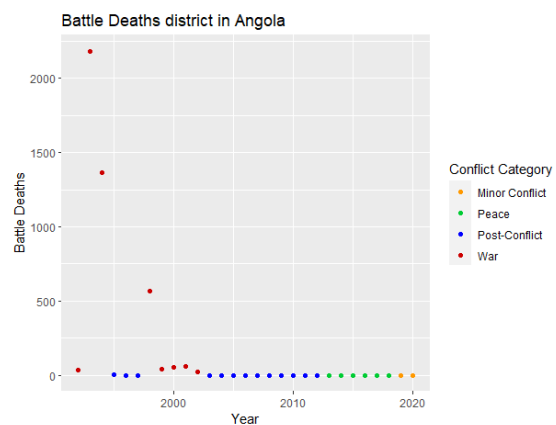


Figure 10: Battle deaths and conflict categorization based on an Angolan district. Source: UCDP/Author's categorization.

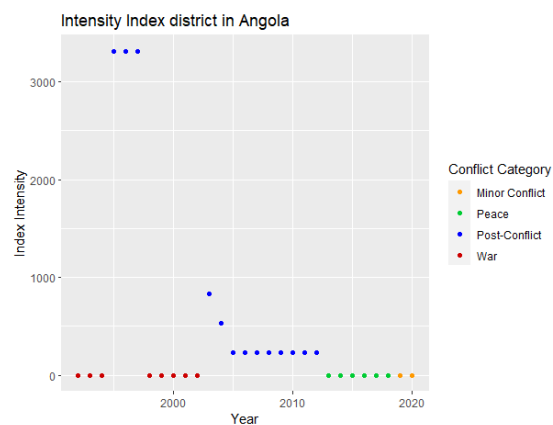
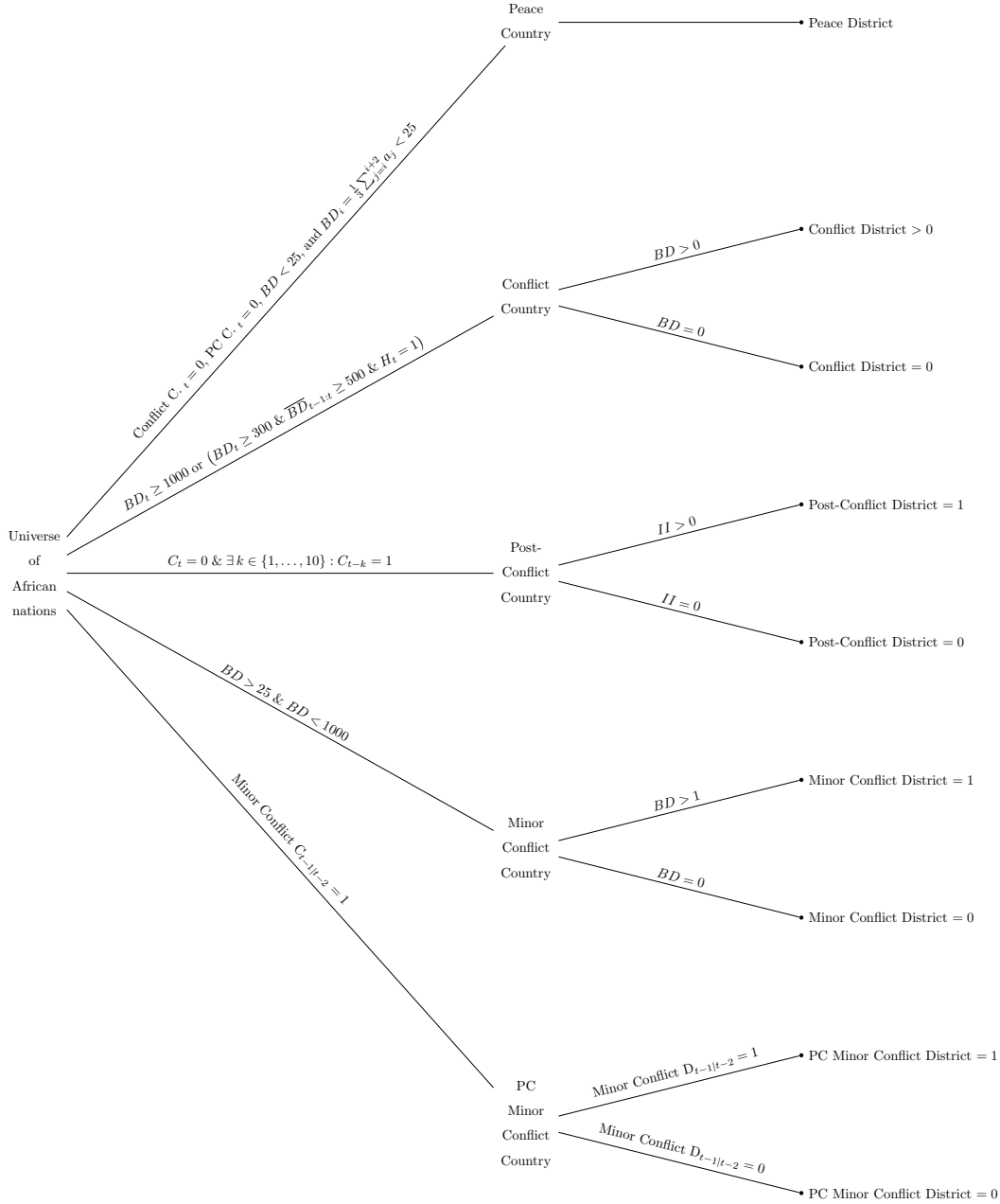


Figure 11: Intensity Index and conflict categorization based on an Angolan district. Source: UCDP/Author's estimation.

A.2 Categorization of Conflict

Figure A.2 describes a decision tree for coding the different stages between conflict and peace at the country and district levels. It shows that each district-level coding depends first on the country level at the respective time t . Dummy encoding is used, meaning if a country is at war at time t , the binary dummy for Conflict Country is 1, and 0 for each other country-stage. The same is true for the district level. In the regressions, the binary variables for peace at the country and district level are excluded as they indicate the baseline. In order for the system to be estimated correctly, every other stage must be included in the regression estimation.



Note: $\overline{BD}_{t-1:t} = \text{round}\left(\frac{BD_{t-1} + BD_t}{2}\right)$, and $H_t = 1$ if the country recorded at least 1,000 fatalities in any of the preceding ten years.

A.3 Data

Table A.1: UN Security Council Membership and Presidency

Country	Year	Presidency
Algeria	2004	1
	2005	0
Angola	2003	1
	2004	0
	2015	0
Benin	2004	0
	2005	1
Botswana	1995	1
	1996	1
Burkina Faso	2008	1
	2009	1
Cameroon	2002	1
	2003	0
Chad	2014	1
	2015	0
Congo	2006	1
	2007	1
Egypt	1996	1
	1997	0
Gabon	1998	0
	1999	1
	2010	1
	2011	1
Gambia	1998	1
	1999	1
Ghana	2006	1
	2007	1
Guinea	2002	0
	2003	1
Guinea-Bissau	1996	1
	1997	0

Country	Year	Presidency
Kenya	1997	1
	1998	1
Morocco	2012	1
	2013	0
Namibia	1999	1
	2000	1
Nigeria	1995	1
	2010	1
	2011	1
	2014	1
	2015	1
Rwanda	1995	0
	2013	1
	2014	1
South Africa	2007	1
	2008	1
	2011	0
	2012	1
Tanzania	2005	0
	2006	1
Togo	2012	1
	2013	1
Tunisia	2000	0
	2001	1
Uganda	2009	1
	2010	1

Table A.2: Conflict-/Post-Conflict Country Periods

Country	Conflict-/Post-Conflict Periods				
Angola	War 1992-1994 Minor Conflict 2019-2020	Post-Conflict 1995-1997	War 1998-2002	Post-Conflict 2003-2012	Peace 2013-2018
Algeria	Minor Conflict 1992-1993	War 1994-2004	Post-Conflict 2005-2014	Minor Conflict 2015-2020	
Burkina Faso	Peace 1992-2016	Minor Conflict 2017-2018	War 2018-2020		
Burundi	Minor Conflict 1992	War 1993-2005	Post-Conflict 2006-2015	Minor Conflict 2016-2020	
Cameroon	Minor Conflict 1992-1994 Post Minor Conflict 1999-2000	Post Minor Conflict 1995 Peace 2001-2013	Minor Conflict 1996 War 2014-2016	Post Minor Conflict 1997 Post-Conflict 2017	Minor Conflict 1998 War 2018-2020
Central African Republic	Peace 1992-2000 Post Minor Conflict 2008	Minor Conflict 2001-2002 Minor Conflict 2009-2012	Post Minor Conflict 2003-2004 War 2013-2018	Peace 2005 Post-Conflict 2019-2020	Minor Conflict 2006-2007
Chad	War 1992-1993 Post-Conflict 2009-2018	Post-Conflict 1994-1999 Minor Conflict 2019-2020	War 2000	Post-Conflict 2001-2005	War 2006-2008
Congo	Peace 1992 Post-Conflict 2000-2009	Minor Conflict 1993 Peace 2010-2017	Post Minor Conflict 1994-1995 Minor Conflict 2018	Peace 1996 Post Minor Conflict 2019-2020	War 1997-1999
Côte d'Ivoire	Peace 1992-1999 War 2011	Minor Conflict 2000 Post-Conflict 2012-2020	Post Minor Conflict 2001	War 2002-2003	Post-Conflict 2004-2010
DR Congo	Minor Conflict 1992 War 2007-2020	War 1993-1994	Post-Conflict 1995	War 1996-2005	Post-Conflict 2006
Eritrea	Post-Conflict 1992-1998	War 1999-2000	Post-Conflict 2001-2010	Peace 2011-2020	
Ethiopia	War 1992 Post-Conflict 2001	Post-Conflict 1994-1994 War 2002-2004	War 1995 Post-Conflict 2005-2014	Post-Conflict 1996-1997 Minor Conflict 2015-2019	War 1998-2000 War 2020
Ghana	Minor Conflict 1992	Post Minor Conflict 1993	War 1994	Post-Conflict 1995-2004	Peace 2005-2020
Liberia	War 1992-1996	Post-Conflict 1997-2001	War 2002-2003	Post-Conflict 2004-2013	Peace 2014-2020
Mali	Minor Conflict 1992 Minor Conflict 1999 Minor Conflict 2012-2017	Post Minor Conflict 1993 Post Minor Conflict 2000-2001 War 2018-2020	Minor Conflict 1994 Peace 2002-2007	Post Minor conflict 1995-1996 Minor Conflict 2008-2009	Peace 1997-1998 Post Minor Conflict 2010-2011
Mozambique	War 1992 Minor Conflict 2016	Post-Conflict 1993-2002 Post Minor Conflict 2017	Peace 2003-2013 Minor Conflict 2018-2019	Minor Conflict 2014 War 2020	Post Minor Conflict 2015
Nigeria	Minor Conflict 1992 War 2010-2020	War 1993	Post Conflict 1994-1998	War 1999-2004	Post-Conflict 2005-2009
Rwanda	War 1992-1998	Post-Conflict 1999-2000	War 2001	Post-Conflict 2002-2011	Peace 2012-2020
Sierra Leone	War 1992-2000	Post-Conflict 2001-2010	Peace 2011-2020		
Somalia	War 1992-1996	Post-Conflict 1997-2001	War 2002	Post-Conflict 2003-2005	War 2006-2020
South Africa	War 1992-1994	Post-Conflict 1995-2004	Peace 2005-2020		
Uganda	Post-Conflict 1992-1995 Peace 2017-2020	War 1996-2000	Post-Conflict 2001	War 2002-2006	Post-Conflict 2007-2016

A.4 Summary and Treatment Statistics

Table A.3: Summary Statistics for the Main Estimation Sample

Variable	N	Mean	St. Dev.	Min	Max
$\Delta \ln(light_{crt})$	135,025	0.004	0.15	-2.59	2.08
$\ln(Aid_{t-1})$ (GoDAD)	135,025	3.62	6.01	0.00	21.18
$\ln(Aid_{t-1})$ (AidData)	108,020	3.03	5.41	0.00	17.73
$\ln(light_{t-2})$	135,025	1.02	1.28	0.00	4.16
Precipitation	135,025	81.01	52.76	0.03	305.89
Temperature	135,025	23.36	4.02	9.21	31.03
District population density	135,025	1,010	4,539	0.00	95,668
District population	135,025	177,809	291,230	0.00	6,094,831
Country population density	135,025	77.54	76.24	2.00	529.62
Country population growth	135,025	2.45	0.91	-0.92	16.75
Peacekeeping presence	135,025	0.02	0.13	0	1
Chinese aid-flow count	135,025	0.01	0.08	0	3
Post-conflict intensity index (UCDP)	135,025	24.44	638.01	0.00	74,791

Notes. The sample covers 1996–2020 and is restricted to observations with complete data for the variables defining the main estimation sample. The smaller number of observations for AidData reflects its shorter temporal coverage.

Table A.4: Treatment Counts for Post-Conflict Variables, 1996–2020

Category	Non-treated	Treated	Treated share
Post-conflict district (any fighting)*	126,488	8,612	0.064
Post-conflict district (low intensity)*	129,914	5,186	0.038
Post-conflict district (medium intensity)*	132,747	2,353	0.017
Post-conflict district (high intensity)*	134,427	673	0.005
Post-conflict district (very high intensity)*	134,700	400	0.003
Post-conflict country	112,341	22,759	0.168
War country	107,811	27,289	0.202
Minor-conflict country	111,447	23,653	0.175
Post-minor-conflict country	127,931	7,169	0.053
Peaceful country	80,870	54,230	0.401
Post-conflict district, early period*	131,649	3,451	0.026
Post-conflict district, middle period*	131,769	3,331	0.025
Post-conflict district, late period*	133,270	1,830	0.014
Post-conflict district with lasting peace*	127,886	7,214	0.053

*Based on the post-conflict intensity index.

B Appendix Further Regression Results

B.1 Aid Timing

Table A.5: Alternative Lags of Aid Disbursements

	<i>Dependent variable: $\Delta \ln(light_{crt})$</i>			
	Lag 1	Lag 2	Lag 3	Lag 4
$\ln(Aid_{t-k})$	0.0002** (0.0001)	-0.0001 (0.0001)	-0.0000 (0.0001)	0.0000 (0.0001)
Post-Conflict District	-0.0080*** (0.0024)	-0.0091*** (0.0025)	-0.0036 (0.0026)	-0.0030 (0.0027)
$\ln(Aid_{t-k}) \times$ Post-Conflict District	-0.0010** (0.0004)	-0.0013*** (0.0005)	-0.0017*** (0.0005)	-0.0019*** (0.0005)
Post-Conflict Country	0.0065*** (0.0022)	0.0061** (0.0024)	0.0033 (0.0026)	0.0090*** (0.0029)
$\ln(Aid_{t-k}) \times$ Post-Conflict Country	0.0006* (0.0003)	0.0009*** (0.0003)	0.0010*** (0.0003)	0.0007* (0.0004)
Observations	135025	129624	124223	118822
Districts	5401	5401	5401	5401
Country FE	✓	✓	✓	✓
District FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Country-Year FE				
District-specific Trends	✓	✓	✓	✓
Country-specific Trends	✓	✓	✓	✓
District Controls	✓	✓	✓	✓
Country Controls	✓	✓	✓	✓
Dep. Var. Mean	0.004	0.004	0.004	0.004
Dep. Var. Std.	0.151	0.153	0.154	0.156
R2	0.157	0.159	0.163	0.168

B.2 Alternative Aid and Conflict Data

Table A.6: Robustness - GODAD vs. AidData

<i>Dependent variable: $\Delta \ln(light_{crt})$</i>					
Panel A: GoDAD Aid, 1995–2015					
Intensity	Any	Low	Medium	High	Very high
$\ln(Aid_{t-1})$	0.0004*** (0.0001)	0.0004*** (0.0001)	0.0004*** (0.0001)	0.0004*** (0.0001)	0.0004*** (0.0001)
Post-Conflict District	0.0035 (0.0026)	0.0029 (0.0026)	0.0021 (0.0063)	0.0056 (0.0055)	0.0209** (0.0090)
$\ln(Aid_{t-1}) \times$ Post-Conflict District	-0.0007 (0.0004)	-0.0013* (0.0007)	-0.0004 (0.0008)	0.0010 (0.0007)	-0.0007 (0.0009)
Post-Conflict Country	-0.0048** (0.0022)	-0.0048** (0.0022)	-0.0044** (0.0022)	-0.0043* (0.0022)	-0.0043** (0.0022)
$\ln(Aid_{t-1}) \times$ Post-Conflict Country	0.0001 (0.0003)	-0.0000 (0.0002)	-0.0002 (0.0003)	-0.0003 (0.0003)	-0.0002 (0.0003)
Observations	108020	108020	108020	108020	108020
Districts	5401	5401	5401	5401	5401
Dep. Var. Mean	0.015	0.015	0.015	0.015	0.015
Dep. Var. Std.	0.114	0.114	0.114	0.114	0.114
R2	0.140	0.140	0.140	0.140	0.140
Panel B: AidData Aid, 1995–2015					
$\ln(Aid_{t-1})$	0.0004*** (0.0001)	0.0004*** (0.0001)	0.0004*** (0.0001)	0.0004*** (0.0001)	0.0004*** (0.0001)
Post-Conflict District	0.0017 (0.0027)	0.0011 (0.0027)	0.0006 (0.0066)	0.0060 (0.0062)	0.0273*** (0.0098)
$\ln(Aid_{t-1}) \times$ Post-Conflict District	0.0003 (0.0004)	-0.0001 (0.0006)	0.0004 (0.0007)	0.0011* (0.0006)	-0.0013 (0.0009)
Post-Conflict Country	-0.0029 (0.0022)	-0.0032 (0.0022)	-0.0031 (0.0022)	-0.0031 (0.0022)	-0.0032 (0.0022)
$\ln(Aid_{t-1}) \times$ Post-Conflict Country	-0.0007** (0.0003)	-0.0006** (0.0003)	-0.0007*** (0.0002)	-0.0007*** (0.0003)	-0.0006** (0.0002)
Observations	108020	108020	108020	108020	108020
Districts	5401	5401	5401	5401	5401
District FE	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
District-specific Trends	✓	✓	✓	✓	✓
Country-specific Trends	✓	✓	✓	✓	✓
District Controls	✓	✓	✓	✓	✓
Country Controls	✓	✓	✓	✓	✓
Dep. Var. Mean	0.015	0.015	0.015	0.015	0.015
Dep. Var. Std.	0.114	0.114	0.114	0.114	0.114
R2	0.140	0.140	0.140	0.140	0.140

Standard errors in the parentheses are clustered at the district level. All specifications include district and year-fixed effects. All country and district-level controls are used. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.7: Robustness - UCDP vs. ACLED

	<i>Dependent variable: $\Delta \ln(light_{crt})$</i>				
Panel A: UCDP Conflict Data, 2000–2020					
Intensity	Any	Low	Medium	High	Very high
$\ln(Aid_{t-1})$	0.0003** (0.0001)	0.0003** (0.0001)	0.0003** (0.0001)	0.0003** (0.0001)	0.0003** (0.0001)
Post-Conflict District	-0.0066** (0.0028)	0.0020 (0.0030)	-0.0298*** (0.0058)	-0.0241*** (0.0090)	0.0181 (0.0162)
$\ln(Aid_{t-1}) \times$ Post-Conflict District	-0.0011** (0.0005)	-0.0025*** (0.0006)	0.0016** (0.0008)	0.0006 (0.0009)	-0.0019* (0.0011)
Post-Conflict Country	0.0143*** (0.0030)	0.0141*** (0.0031)	0.0161*** (0.0031)	0.0156*** (0.0031)	0.0154*** (0.0031)
$\ln(Aid_{t-1}) \times$ Post-Conflict Country	0.0002 (0.0003)	0.0003 (0.0003)	-0.0004* (0.0003)	-0.0002 (0.0003)	-0.0002 (0.0003)
Observations	113421	113421	113421	113421	113421
Districts	5401	5401	5401	5401	5401
Dep. Var. Mean	0.003	0.003	0.003	0.003	0.003
Dep. Var. Std.	0.158	0.158	0.158	0.158	0.158
R2	0.171	0.171	0.171	0.171	0.171
Panel B: ACLED Conflict Data, 2000–2020					
$\ln(Aid_{t-1})$	0.0005*** (0.0001)	0.0005*** (0.0001)	0.0005*** (0.0001)	0.0005*** (0.0001)	0.0005*** (0.0001)
Post-Conflict District	-0.0035 (0.0035)	0.0014 (0.0040)	-0.0147** (0.0064)	-0.0226** (0.0101)	0.0023 (0.0115)
$\ln(Aid_{t-1}) \times$ Post-Conflict District	-0.0001 (0.0005)	-0.0014** (0.0007)	0.0015** (0.0008)	0.0025*** (0.0009)	0.0009 (0.0007)
Post-Conflict Country	0.0306*** (0.0028)	0.0298*** (0.0028)	0.0316*** (0.0028)	0.0312*** (0.0028)	0.0309*** (0.0028)
$\ln(Aid_{t-1}) \times$ Post-Conflict Country	-0.0016*** (0.0003)	-0.0013*** (0.0003)	-0.0019*** (0.0003)	-0.0018*** (0.0003)	-0.0017*** (0.0003)
Observations	113282	113282	113282	113282	113282
Districts	5401	5401	5401	5401	5401
District FE	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
District-specific Trends	✓	✓	✓	✓	✓
Country-specific Trends	✓	✓	✓	✓	✓
District Controls	✓	✓	✓	✓	✓
Country Controls	✓	✓	✓	✓	✓
Dep. Var. Mean	0.003	0.003	0.003	0.003	0.003
Dep. Var. Std.	0.158	0.158	0.158	0.158	0.158
R2	0.172	0.172	0.172	0.172	0.172

Standard errors in the parentheses are clustered at the district level. All specifications include district and year-fixed effects. All country and district-level controls are used. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B.3 Concurrent International Interventions

Table A.8: Robustness to Concurrent International Interventions

Intensity	<i>Dependent variable: $\Delta \ln(light_{crt})$</i>				
	Any	Low	Medium	High	Very high
$\ln(Aid_{t-1})$	0.0002* (0.0001)	0.0002* (0.0001)	0.0002* (0.0001)	0.0002* (0.0001)	0.0002* (0.0001)
Post-Conflict District	-0.0080*** (0.0024)	-0.0018 (0.0025)	-0.0241*** (0.0050)	-0.0200** (0.0079)	-0.0039 (0.0111)
$\ln(Aid_{t-1}) \times$ Post-Conflict District	-0.0010** (0.0004)	-0.0021*** (0.0005)	0.0010 (0.0008)	0.0004 (0.0008)	-0.0008 (0.0009)
Post-Conflict Country	0.0064*** (0.0022)	0.0063*** (0.0022)	0.0076*** (0.0022)	0.0073*** (0.0022)	0.0072*** (0.0022)
$\ln(Aid_{t-1}) \times$ Post-Conflict Country	0.0006* (0.0003)	0.0006** (0.0003)	0.0001 (0.0002)	0.0002 (0.0003)	0.0002 (0.0002)
Peacekeeping	0.0105** (0.0046)	0.0107** (0.0046)	0.0104** (0.0046)	0.0106** (0.0046)	0.0107** (0.0046)
Chinese Aid	0.0018 (0.0039)	0.0018 (0.0039)	0.0017 (0.0039)	0.0017 (0.0039)	0.0017 (0.0039)
Observations	135025	135025	135025	135025	135025
Districts	5401	5401	5401	5401	5401
District FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Country-Year FE					
District-specific Trends	✓	✓	✓	✓	✓
Country-specific Trends	✓	✓	✓	✓	✓
District Controls	✓	✓	✓	✓	✓
Country Controls	✓	✓	✓	✓	✓
Dep. Var. Mean	0.004	0.004	0.004	0.004	0.004
Dep. Var. Std.	0.151	0.151	0.151	0.151	0.151
R2	0.157	0.157	0.157	0.157	0.157

Notes. The table augments the baseline specification with measures of local peacekeeping presence and Chinese development finance. Standard errors are clustered at the district level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.4 Outcome Transformations and Harmonization

Table A.9: Nighttime-Light Functional Forms

<i>Nighttime-light outcome</i>				
	Growth: $\Delta \ln(light+1)$	Growth: $\Delta IHS(light)$	Level: $\ln(light+1)$	Level: $IHS(light)$
$\ln(Aid_{t-1})$	0.0002** (0.0001)	0.0003** (0.0002)	0.0004 (0.0002)	0.0006** (0.0003)
Post-Conflict District	-0.0080*** (0.0024)	-0.0029 (0.0035)	-0.0214*** (0.0055)	-0.0247*** (0.0073)
$\ln(Aid_{t-1}) \times$ Post-Conflict District	-0.0010** (0.0004)	-0.0008 (0.0006)	-0.0009 (0.0008)	-0.0014 (0.0011)
Post-Conflict Country	0.0065*** (0.0022)	0.0131*** (0.0034)	0.0186*** (0.0053)	0.0357*** (0.0069)
$\ln(Aid_{t-1}) \times$ Post-Conflict Country	0.0006* (0.0003)	0.0008** (0.0004)	-0.0005 (0.0005)	-0.0013* (0.0007)
Observations	135025	135025	135025	135025
Districts	5401	5401	5401	5401
Country FE	✓	✓	✓	✓
District FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Country-Year FE				
District-specific Trends	✓	✓	✓	✓
Country-specific Trends	✓	✓	✓	✓
District Controls	✓	✓	✓	✓
Country Controls	✓	✓	✓	✓
Dep. Var. Mean	0.004	0.047	1.104	1.357
Dep. Var. Std.	0.151	0.232	1.290	1.547
R2	0.157	0.369	0.983	0.980

Notes. Columns (1) and (2) use annual changes in transformed nighttime light; Columns (3) and (4) use transformed nighttime-light levels. Coefficients are not directly comparable across transformations. Standard errors in parentheses are clustered at the district level. All specifications include the fixed effects, heterogeneous trends, and controls used in the baseline. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Columns (3) and (4) instead use transformed nighttime-light levels. These specifications answer a different question: whether post-conflict status and aid are associated with a shift in the conditional level of nighttime light, rather than with its annual growth. A temporary acceleration in growth can create a persistent level difference, so a level estimate is not evidence of continued growth and need not indicate that recovery is overstated. The signs of the post-conflict coefficients remain stable in the level specifications, but the aid interactions do not: the interaction for directly exposed districts is statistically insignificant, while the interaction for post-conflict districts without local fighting becomes negative. These columns should therefore be read as complementary level estimates rather than robustness checks for the same estimand.

Table A.10: Nighttime-Light Harmonization and Imputation

	<i>Dependent variable: $\Delta \ln(light_{crt})$</i>		
	Baseline Imputation	DMSP Imputation	No Imputation
$\ln(Aid_{t-1})$	0.0002** (0.0001)	0.0003*** (0.0001)	0.0004*** (0.0001)
Post-Conflict District	-0.0080*** (0.0024)	0.0014 (0.0021)	-0.0002 (0.0028)
$\ln(Aid_{t-1}) \times$ Post-Conflict District	-0.0010** (0.0004)	-0.0005 (0.0004)	-0.0007 (0.0005)
Post-Conflict Country	0.0065*** (0.0022)	0.0053*** (0.0019)	0.0137*** (0.0026)
$\ln(Aid_{t-1}) \times$ Post-Conflict Country	0.0006* (0.0003)	0.0005* (0.0003)	0.0004 (0.0003)
Observations	135025	135025	135025
Districts	5401	5401	5401
Country FE	✓	✓	✓
District FE	✓	✓	✓
Year FE	✓	✓	✓
Country-Year FE			
District-specific Trends	✓	✓	✓
Country-specific Trends	✓	✓	✓
District Controls	✓	✓	✓
Country Controls	✓	✓	✓
Dep. Var. Mean	0.004	0.015	0.037
Dep. Var. Std.	0.151	0.165	0.225
R2	0.157	0.173	0.371

Notes. Column (1) uses the baseline series, in which the changes in nighttime light in 2014 and 2017 are imputed. Column (2) imputes only 2014, and Column (3) uses neither imputation. All columns retain the same observations. Standard errors in parentheses are clustered at the district level. All specifications include the fixed effects, heterogeneous trends, and controls used in the baseline. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B.5 Aid Transformation

Table A.11: Robustness - Aid functional forms

	<i>Dependent variable: $\Delta \ln(light_{crt})$</i>	
	$\ln(Aid_{t-1})$	IHS(Aid_{t-1})
$\ln(Aid_{t-1})$	0.0002** (0.0001)	
Post-Conflict District	-0.0080*** (0.0024)	-0.0080*** (0.0024)
$\ln(Aid_{t-1}) \times$ Post-Conflict District	-0.0010** (0.0004)	
Post-Conflict Country	0.0065*** (0.0022)	0.0065*** (0.0022)
$\ln(Aid_{t-1}) \times$ Post-Conflict Country	0.0006* (0.0003)	
IHS(Aid_{t-1})		0.0002** (0.0001)
IHS(Aid_{t-1}) $\times$ Post-Conflict District		-0.0009** (0.0004)
IHS(Aid_{t-1}) $\times$ Post-Conflict Country		0.0005* (0.0003)
Observations	135025	135025
Districts	5401	5401
Country FE	✓	✓
District FE	✓	✓
Year FE	✓	✓
Country-Year FE		
District-specific Trends	✓	✓
Country-specific Trends	✓	✓
District Controls	✓	✓
Country Controls	✓	✓
Dep. Var. Mean	0.004	0.004
Dep. Var. Std.	0.151	0.151
R2	0.157	0.157

Standard errors in the parentheses are clustered at the district level. All specifications include district and year-fixed effects. All country and district-level controls are used. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B.6 Sample Composition and Restricted Panels

Table A.12: Sample Sensitivity: Excluding Large Economies and North African Countries

	<i>Dependent variable: $\Delta \ln(light_{crt})$</i>			
	Baseline	Excl. Top Five GDP	Excl. North Africa	Excl. Top Five GDP p.c.
$\ln(Aid_{t-1})$	0.0002** (0.0001)	-0.0000 (0.0001)	0.0001 (0.0001)	0.0003** (0.0001)
Post-Conflict District	-0.0080*** (0.0024)	-0.0157*** (0.0036)	-0.0107*** (0.0032)	-0.0082*** (0.0025)
$\ln(Aid_{t-1}) \times$ Post-Conflict District	-0.0010** (0.0004)	-0.0008* (0.0005)	-0.0007 (0.0005)	-0.0010** (0.0004)
Post-Conflict Country	0.0065*** (0.0022)	-0.0056* (0.0031)	-0.0033 (0.0027)	0.0085*** (0.0024)
$\ln(Aid_{t-1}) \times$ Post-Conflict Country	0.0006* (0.0003)	0.0015*** (0.0003)	0.0013*** (0.0003)	0.0005* (0.0003)
Observations	135025	89825	103850	122175
Districts	5401	3593	4154	4887
Country FE	✓	✓	✓	✓
District FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Country-Year FE				
District-specific Trends	✓	✓	✓	✓
Country-specific Trends	✓	✓	✓	✓
District Controls	✓	✓	✓	✓
Country Controls	✓	✓	✓	✓
Dep. Var. Mean	0.004	0.002	0.001	0.004
Dep. Var. Std.	0.151	0.154	0.161	0.156
R2	0.157	0.151	0.145	0.155

Notes. Column (1) reports the baseline. Column (2) excludes Egypt, Nigeria, South Africa, Algeria, and Morocco, the five largest African economies by aggregate GDP. Column (3) excludes Egypt, Morocco, Tunisia, and Algeria. Column (4) excludes the five African countries with the highest GDP per capita. Standard errors in parentheses are clustered at the district level. All specifications include the fixed effects, heterogeneous trends, and controls used in the baseline. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.13: Restricted Panels of Conflict-Experienced Countries

Dependent variable: $\Delta \ln(light_{crt})$					
Panel A: Countries with a Post-Conflict Period					
Intensity	Any	Low	Medium	High	Very high
$\ln(Aid_{t-1})$	0.0007*** (0.0002)	0.0007*** (0.0002)	0.0007*** (0.0002)	0.0007*** (0.0002)	0.0007*** (0.0002)
Post-Conflict District	-0.0094*** (0.0025)	-0.0035 (0.0026)	-0.0255*** (0.0052)	-0.0202** (0.0081)	0.0036 (0.0119)
$\ln(Aid_{t-1}) \times$ Post-Conflict District	-0.0009** (0.0005)	-0.0020*** (0.0006)	0.0011 (0.0008)	0.0004 (0.0008)	-0.0008 (0.0009)
Post-Conflict Country	0.0059* (0.0030)	0.0059* (0.0030)	0.0074** (0.0031)	0.0070** (0.0030)	0.0069** (0.0030)
$\ln(Aid_{t-1}) \times$ Post-Conflict Country	0.0002 (0.0003)	0.0003 (0.0003)	-0.0003 (0.0003)	-0.0001 (0.0003)	-0.0001 (0.0003)
Observations	69900	69900	69900	69900	69900
Districts	2796	2796	2796	2796	2796
Dep. Var. Mean	0.004	0.004	0.004	0.004	0.004
Dep. Var. Std.	0.167	0.167	0.167	0.167	0.167
R2	0.158	0.158	0.158	0.158	0.158
Panel B: First Observed Aid Does Not Precede Post-Conflict					
$\ln(Aid_{t-1})$	0.0001 (0.0004)	0.0001 (0.0004)	0.0001 (0.0004)	0.0001 (0.0004)	0.0001 (0.0004)
Post-Conflict District	0.0027 (0.0035)	0.0005 (0.0037)	0.0026 (0.0061)	0.0019 (0.0069)	0.0318*** (0.0109)
$\ln(Aid_{t-1}) \times$ Post-Conflict District	0.0004 (0.0005)	0.0008 (0.0008)	0.0010* (0.0005)	-0.0006 (0.0014)	-0.0013* (0.0008)
Post-Conflict Country	-0.0126*** (0.0044)	-0.0128*** (0.0044)	-0.0128*** (0.0044)	-0.0130*** (0.0044)	-0.0131*** (0.0044)
$\ln(Aid_{t-1}) \times$ Post-Conflict Country	-0.0001 (0.0005)	-0.0000 (0.0004)	-0.0000 (0.0004)	0.0002 (0.0004)	0.0003 (0.0004)
Observations	17100	17100	17100	17100	17100
Districts	684	684	684	684	684
District FE	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
District-specific Trends	✓	✓	✓	✓	✓
Country-specific Trends	✓	✓	✓	✓	✓
District Controls	✓	✓	✓	✓	✓
Country Controls	✓	✓	✓	✓	✓
Dep. Var. Mean	-0.005	-0.005	-0.005	-0.005	-0.005
Dep. Var. Std.	0.158	0.158	0.158	0.158	0.158
R2	0.213	0.213	0.213	0.213	0.213

Notes. Panel A restricts the sample to countries classified as post-conflict at least once during the observation period. Panel B, “Countries Where First Observed Aid Does Not Precede Post-Conflict,” restricts the sample to countries whose first observed World Bank aid year is the same as or later than their first observed post-conflict year. Standard errors in parentheses are clustered at the district level. All specifications include the fixed effects, heterogeneous trends, and controls used in the baseline. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B.7 Additional Mechanism Results

Table A.14: Lasting Peace and Post-Conflict Recovery

	<i>Dependent variable: $\Delta \ln(light_{crt})$</i>				
	Any	Low	Medium	High	Very high
Lasting Peace					
Intensity					
$\ln(Aid_{t-1})$	0.0008*** (0.0002)	0.0007*** (0.0002)	0.0008*** (0.0002)	0.0008*** (0.0002)	0.0008*** (0.0002)
Post-Conflict District	-0.0119*** (0.0029)	-0.0014 (0.0030)	-0.0388*** (0.0065)	-0.0302*** (0.0106)	0.0061 (0.0195)
Lasting Peace	0.0160*** (0.0034)	0.0220*** (0.0033)	0.0157*** (0.0030)	0.0189*** (0.0030)	0.0196*** (0.0030)
$\ln(Aid_{t-1}) \times$ Post-Conflict District	-0.0006 (0.0006)	-0.0020*** (0.0007)	0.0016 (0.0010)	0.0019 (0.0012)	-0.0003 (0.0013)
$\ln(Aid_{t-1}) \times$ Lasting Peace	0.0018*** (0.0006)	0.0013*** (0.0005)	0.0017*** (0.0005)	0.0018*** (0.0005)	0.0017*** (0.0005)
Lasting Peace $\times$ Post-Conflict District	0.0064 (0.0046)	-0.0097* (0.0051)	0.0325*** (0.0082)	0.0240* (0.0131)	-0.0114 (0.0186)
$\ln(Aid_{t-1}) \times$ Lasting Peace $\times$ Post-Conflict District	-0.0005 (0.0009)	0.0008 (0.0010)	-0.0010 (0.0015)	-0.0036** (0.0016)	-0.0009 (0.0016)
Post-Conflict Country	-0.0105*** (0.0038)	-0.0124*** (0.0039)	-0.0094** (0.0038)	-0.0109*** (0.0039)	-0.0112*** (0.0039)
$\ln(Aid_{t-1}) \times$ Post-Conflict Country	-0.0010** (0.0004)	-0.0007* (0.0004)	-0.0013*** (0.0004)	-0.0012*** (0.0004)	-0.0011*** (0.0004)
Observations	69900	69900	69900	69900	69900
Districts	2796	2796	2796	2796	2796
District FE	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
District-specific Trends	✓	✓	✓	✓	✓
Country-specific Trends	✓	✓	✓	✓	✓
District Controls	✓	✓	✓	✓	✓
Country Controls	✓	✓	✓	✓	✓
Dep. Var. Mean	0.004	0.004	0.004	0.004	0.004
Dep. Var. Std.	0.167	0.167	0.167	0.167	0.167
R2	0.158	0.158	0.158	0.158	0.158

Notes. The sample is restricted to countries that experience at least one post-conflict period. The lasting-peace indicator equals one throughout a post-conflict period if the next observed non-post-conflict state is peace. Standard errors in parentheses are clustered at the district level. All specifications include the fixed effects, heterogeneous trends, and controls used in the baseline. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.15: Timing of Post-Conflict Recovery

	Dependent variable: $\Delta \ln(light_{crt})$				
Panel A: Early Post-Conflict Period					
Intensity	Any	Low	Medium	High	Very high
$\ln(Aid_{t-1})$	0.0007*** (0.0002)	0.0007*** (0.0002)	0.0007*** (0.0002)	0.0007*** (0.0002)	0.0007*** (0.0002)
Post-Conflict District	-0.0048* (0.0027)	-0.0092*** (0.0035)	-0.0005 (0.0040)	-0.0025 (0.0068)	0.0424*** (0.0094)
$\ln(Aid_{t-1}) \times$ Post-Conflict District	0.0007* (0.0004)	-0.0001 (0.0005)	0.0008 (0.0007)	0.0017** (0.0007)	-0.0002 (0.0012)
Post-Conflict Country	0.0034 (0.0029)	0.0030 (0.0029)	0.0032 (0.0029)	0.0032 (0.0029)	0.0031 (0.0029)
$\ln(Aid_{t-1}) \times$ Post-Conflict Country	-0.0004 (0.0003)	-0.0002 (0.0003)	-0.0003 (0.0003)	-0.0003 (0.0003)	-0.0002 (0.0003)
Observations	69900	69900	69900	69900	69900
Panel B: Middle Post-Conflict Period					
$\ln(Aid_{t-1})$	0.0007*** (0.0002)	0.0007*** (0.0002)	0.0007*** (0.0002)	0.0007*** (0.0002)	0.0007*** (0.0002)
Post-Conflict District	-0.0014 (0.0024)	0.0060** (0.0030)	-0.0166*** (0.0045)	-0.0083 (0.0106)	-0.0339*** (0.0102)
$\ln(Aid_{t-1}) \times$ Post-Conflict District	-0.0018*** (0.0004)	-0.0024*** (0.0006)	-0.0010 (0.0007)	-0.0005 (0.0011)	0.0009 (0.0009)
Post-Conflict Country	0.0030 (0.0029)	0.0034 (0.0029)	0.0038 (0.0029)	0.0039 (0.0029)	0.0040 (0.0029)
$\ln(Aid_{t-1}) \times$ Post-Conflict Country	0.0000 (0.0003)	-0.0000 (0.0003)	-0.0001 (0.0003)	-0.0002 (0.0003)	-0.0002 (0.0003)
Observations	69900	69900	69900	69900	69900
Panel C: Late Post-Conflict Period					
$\ln(Aid_{t-1})$	0.0007*** (0.0002)	0.0006*** (0.0002)	0.0006*** (0.0002)	0.0006*** (0.0002)	0.0006*** (0.0002)
Post-Conflict District	-0.0105*** (0.0034)	0.0049 (0.0036)	-0.0398*** (0.0071)	-0.0353* (0.0195)	-0.0430* (0.0243)
$\ln(Aid_{t-1}) \times$ Post-Conflict District	0.0003 (0.0006)	-0.0016* (0.0009)	0.0036** (0.0014)	0.0013 (0.0015)	0.0031* (0.0019)
Post-Conflict Country	0.0047 (0.0029)	0.0043 (0.0029)	0.0049* (0.0030)	0.0046 (0.0029)	0.0046 (0.0029)
$\ln(Aid_{t-1}) \times$ Post-Conflict Country	-0.0002 (0.0003)	-0.0001 (0.0003)	-0.0003 (0.0003)	-0.0002 (0.0003)	-0.0002 (0.0003)
Observations	69900	69900	69900	69900	69900
Districts	2796	2796	2796	2796	2796
District FE	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
District-specific Trends	✓	✓	✓	✓	✓
Country-specific Trends	✓	✓	✓	✓	✓
District Controls	✓	✓	✓	✓	✓
Country Controls	✓	✓	✓	✓	✓
Dep. Var. Mean	0.004	0.004	0.004	0.004	0.004
Dep. Var. Std.	0.167	0.167	0.167	0.167	0.167
R2	0.158	0.158	0.158	0.158	0.158

Notes. The early, middle, and late post-conflict periods cover years 1–3, 4–7, and 8–10, respectively. The period-specific post-conflict indicators are included while controlling for the remaining observations. Standard errors in parentheses are clustered at the district level. All specifications include the fixed effects, heterogeneous trends, and controls used in the baseline. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.16: Spatial Spillovers of Post-Conflict Intensity

	<i>Dependent variable: $\Delta \ln(light_{ct})$</i>			
	0–20 km	20–50 km	50–150 km	150–250 km
$\ln(Aid_{t-1})$	0.000221* (0.0001)	0.000235** (0.0001)	0.000231* (0.0001)	0.000136 (0.0001)
Post-Conflict District	-0.007904*** (0.0024)	-0.007460*** (0.0024)	-0.007166*** (0.0026)	-0.009332*** (0.0027)
$\ln(Aid_{t-1}) \times$ Post-Conflict District	-0.000989** (0.0004)	-0.000974** (0.0005)	-0.000714 (0.0005)	-0.000902* (0.0005)
Post-Conflict Country	0.006524*** (0.0022)	0.006484*** (0.0022)	0.007542*** (0.0023)	0.009255*** (0.0024)
$\ln(Aid_{t-1}) \times$ Post-Conflict Country	0.000571* (0.0003)	0.000546* (0.0003)	0.000691** (0.0003)	0.000500 (0.0003)
Conflict Spillover Intensity	-0.000001* (0.0000)	-0.000002 (0.0000)	0.000001 (0.0000)	-0.000008*** (0.0000)
$\ln(Aid_{t-1}) \times$ Conflict Spillover Intensity	-0.000000 (0.0000)	-0.000000 (0.0000)	-0.000002*** (0.0000)	-0.000000* (0.0000)
Observations	134975	134650	120375	111950
Districts	5399	5386	4815	4478
Country FE	✓	✓	✓	✓
District FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Country-Year FE				
District-specific Trends	✓	✓	✓	✓
Country-specific Trends	✓	✓	✓	✓
District Controls	✓	✓	✓	✓
Country Controls	✓	✓	✓	✓
Dep. Var. Mean	0.004	0.004	0.004	0.005
Dep. Var. Std.	0.151	0.151	0.151	0.150
R2	0.157	0.157	0.155	0.158

Table A.17: Spatial Spillovers of Aid Exposure

	<i>Dependent variable: $\Delta \ln(light_{crt})$</i>			
	0–20 km	20–50 km	50–150 km	150–250 km
$\ln(Aid_{t-1})$	0.0002* (0.0001)	0.0002 (0.0001)	0.0001 (0.0001)	0.0000 (0.0001)
Post-Conflict District	-0.0062** (0.0024)	-0.0059** (0.0025)	0.0024 (0.0028)	0.0008 (0.0033)
$\ln(Aid_{t-1}) \times$ Post-Conflict District	-0.0008* (0.0004)	-0.0008* (0.0004)	-0.0000 (0.0005)	-0.0004 (0.0005)
Post-Conflict Country	0.0071*** (0.0023)	0.0073*** (0.0023)	0.0056** (0.0026)	0.0088*** (0.0030)
$\ln(Aid_{t-1}) \times$ Post-Conflict Country	0.0006** (0.0003)	0.0005* (0.0003)	0.0005 (0.0003)	0.0004 (0.0003)
Spillover Aid Exposure	0.0004* (0.0002)	0.0009*** (0.0002)	0.0009** (0.0004)	0.0011*** (0.0004)
Spillover Aid Exposure $\times$ Post-Conflict District	-0.0022*** (0.0008)	-0.0012 (0.0008)	-0.0044*** (0.0009)	-0.0040*** (0.0011)
Spillover Aid Exposure $\times$ Post-Conflict Country	-0.0004 (0.0004)	-0.0001 (0.0004)	0.0011* (0.0006)	0.0005 (0.0006)
Observations	134975	134650	120375	111950
Districts	5399	5386	4815	4478
Country FE	✓	✓	✓	✓
District FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Country-Year FE				
District-specific Trends	✓	✓	✓	✓
Country-specific Trends	✓	✓	✓	✓
District Controls	✓	✓	✓	✓
Country Controls	✓	✓	✓	✓
Dep. Var. Mean	0.004	0.004	0.004	0.005
Dep. Var. Std.	0.151	0.151	0.151	0.150
R2	0.157	0.157	0.155	0.158

Table A.18: Main Aid Sectors and Post-Conflict Recovery

Sector	<i>Dependent variable: $\Delta \ln(light_{crt})$</i>		
	Social Infrastructure	Economic Infrastructure	Production
$\ln(Aid_{t-1})$	-0.0003* (0.0002)	0.0003** (0.0001)	0.0005*** (0.0001)
Sector	0.0105*** (0.0029)	-0.0018 (0.0021)	-0.0076*** (0.0022)
Post-Conflict District	-0.0078*** (0.0024)	-0.0080*** (0.0024)	-0.0087*** (0.0025)
Sector $\times$ Post-Conflict District	-0.0145** (0.0062)	-0.0187*** (0.0071)	-0.0216*** (0.0075)
Post-Conflict Country	0.0074*** (0.0022)	0.0063*** (0.0022)	0.0072*** (0.0023)
Sector $\times$ Post-Conflict Country	0.0052 (0.0040)	0.0118** (0.0048)	0.0087 (0.0058)
Observations	135025	135025	135025
Districts	5401	5401	5401
District FE	✓	✓	✓
Country FE	✓	✓	✓
Year FE	✓	✓	✓
District-specific Trends	✓	✓	✓
Country-specific Trends	✓	✓	✓
District Controls	✓	✓	✓
Country Controls	✓	✓	✓
Dep. Var. Mean	0.004	0.004	0.004
Dep. Var. Std.	0.151	0.151	0.151
R2	0.157	0.157	0.157

Table A.19: Aid Subsectors and Post-Conflict Recovery

<i>Dependent variable: $\Delta \ln(light_{crt})$</i>						
Panel A: Subsectors (I)						
	Education	Health	Water	Government	Transport	Energy
$\ln(Aid_{t-1})$	0.0003*** (0.0001)	0.0002 (0.0001)	0.0004*** (0.0001)	-0.0001 (0.0002)	0.0003** (0.0001)	0.0001 (0.0001)
Post-Conflict District	-0.0090*** (0.0025)	-0.0087*** (0.0024)	-0.0085*** (0.0024)	-0.0087*** (0.0025)	-0.0089*** (0.0025)	-0.0095*** (0.0024)
Sector	-0.0017 (0.0022)	0.0041* (0.0023)	-0.0017 (0.0023)	0.0075*** (0.0025)	-0.0008 (0.0021)	0.0065** (0.0030)
Sector $\times$ Post-Conflict District	-0.0153** (0.0065)	-0.0217*** (0.0079)	-0.0064 (0.0070)	-0.0113* (0.0060)	-0.0082 (0.0070)	-0.0090 (0.0097)
Post-Conflict Country	0.0079*** (0.0023)	0.0071*** (0.0023)	0.0087*** (0.0023)	0.0071*** (0.0022)	0.0082*** (0.0023)	0.0054** (0.0022)
Sector $\times$ Post-Conflict Country	-0.0001 (0.0049)	0.0108* (0.0057)	-0.0141*** (0.0049)	0.0070* (0.0041)	-0.0067 (0.0041)	0.0211*** (0.0066)
Observations	135025	135025	135025	135025	135025	135025
Panel B: Subsectors (II)						
	Agriculture	Mining	Banking	Industry	Communications	
$\ln(Aid_{t-1})$	0.0004*** (0.0001)	0.0003** (0.0001)	0.0003** (0.0001)	0.0002* (0.0001)	0.0002* (0.0001)	
Post-Conflict District	-0.0089*** (0.0025)	-0.0106*** (0.0024)	-0.0101*** (0.0024)	-0.0097*** (0.0024)	-0.0098*** (0.0024)	
Sector	-0.0053** (0.0023)	-0.0076 (0.0052)	-0.0023 (0.0030)	0.0077** (0.0036)	0.0017 (0.0043)	
Sector $\times$ Post-Conflict District	-0.0227*** (0.0077)	0.0880 (0.0628)	-0.0125 (0.0114)	-0.0302* (0.0176)	-0.0077 (0.0164)	
Post-Conflict Country	0.0071*** (0.0023)	0.0078*** (0.0022)	0.0077*** (0.0023)	0.0075*** (0.0022)	0.0054** (0.0021)	
Sector $\times$ Post-Conflict Country	0.0124** (0.0063)	-0.0296 (0.0286)	0.0080 (0.0077)	0.0218* (0.0121)	0.0364*** (0.0101)	
Observations	135025	135025	135025	135025	135025	
Districts	5401	5401	5401	5401	5401	
District FE	✓	✓	✓	✓	✓	
Country FE	✓	✓	✓	✓	✓	
Year FE	✓	✓	✓	✓	✓	
District-specific Trends	✓	✓	✓	✓	✓	
Country-specific Trends	✓	✓	✓	✓	✓	
District Controls	✓	✓	✓	✓	✓	
Country Controls	✓	✓	✓	✓	✓	
Dep. Var. Mean	0.004	0.004	0.004	0.004	0.004	
Dep. Var. Std.	0.151	0.151	0.151	0.151	0.151	
R2	0.157	0.157	0.157	0.157	0.157	

Notes. Each column reports a separate sector specification. The sector coefficient measures the association for districts in peaceful countries. The sector interaction with the post-conflict-country indicator is the additional differential for post-conflict districts without local fighting, and the sector interaction with the district-level indicator is the further differential associated with local fighting. Standard errors in parentheses are clustered at the district level. All specifications include the fixed effects, heterogeneous trends, and controls used in the baseline. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B.8 SSIV Approach

Table A.20: Shift-Share Instrumental Variables

	<i>Dependent variable: $\Delta \ln(light_{crt})$</i>					
	OLS	GoDAD IV-IDA	IV-UNSC	OLS	AidData IV-IDA	IV-UNSC
$\ln(Aid_{t-1})$ (GoDAD)	0.0003*** (0.0001)	-0.0000 (0.0002)	0.0299** (0.0120)			
$\ln(Aid_{t-1})$ (GoDAD) $\times$ Post-Conflict District	-0.0006** (0.0002)	-0.0011*** (0.0003)	0.0045 (0.0045)			
$\ln(Aid_{t-1})$ (AidData)				0.0002** (0.0001)	0.0003 (0.0003)	0.0260*** (0.0082)
$\ln(Aid_{t-1})$ (AidData) $\times$ Post-Conflict District				-0.0005** (0.0002)	-0.0009*** (0.0002)	-0.0022 (0.0022)
Observations	108020	102619	108020	108020	102619	108020
Districts	5401	5401	5401	5401	5401	5401
District FE	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Country-Year FE						
District-specific Trends						
Country-specific Trends						
District Controls	✓	✓	✓	✓	✓	✓
Country Controls	✓	✓	✓	✓	✓	✓
Dep. Var. Mean	0.015	0.013	0.015	0.015	0.013	0.015
Dep. Var. Std.	0.114	0.106	0.114	0.114	0.106	0.114
Kleibergen-Paap Wald F stat		587.168	4.901		196.062	7.426

Standard errors in the parentheses are clustered at the district level. All specifications include country, district and year-fixed effects. All country and district-level controls are used. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

There is a rigorous debate on the difficulty of finding valid instruments for determinants of growth. For an instrument to be valid, it is not allowed to "materially affect growth through channels other than the variable of interest" and it needs to be strong in the sense that "instruments correlate well with the variable of interest." (Bazzi & Clemens, 2013, p.152) In the case of this study, the instrument needs to provide exogenous variation of multilateral aid, reflect the variation of aid disbursement on a local level and is not allowed to be correlated with the error term. It has been shown that results based on supply-side instruments that mostly take their strength from its correlation with population size do not capture the variance of aid flows enough to correct the bias generated in an OLS estimation (Clemens et al., 2012). A recent strand of literature relies on instruments using an interaction between the exogenous variation of the donor's aid budget and the strength of the relationship with the recipient country or a country's probability to receive aid. For example, Nunn and Qian (2014) rely on the time variation of the US's wheat production changes and a recipient country's likelihood to receive food aid. Chauvet and Ehrhart (2018) introduce exogenous variation using the total amounts of tax revenues of donor countries in interaction with colonial ties between the donor and the recipient country to account for the strength of the bilateral

relationship. Dreher and Langlotz (2020) employ an instrument that uses the government fractionalization of donor countries interacted with a country’s probability to receive aid. These instruments work well for bilateral aid but are not useful for exploring exogenous variation in multilateral aid.

Instruments used in a multilateral donor settings are employed by Dreher and Lohmann (2015), Dreher et al. (2021), and Galiani et al. (2016). Galiani et al. (2016) present the crossing of the IDA threshold as an exogenous shock for countries to receive less aid and Dreher and Lohmann (2015) combine this with a country’s probability to receive aid which is then used as an instrument for WB aid disbursements. This would be a possible instrument, however, it limits the sample size of countries that can be used for the analysis as they must have a period in which they crossed the IDA threshold. Dreher et al. (2021) introduced an instrument that can be used at a subnational level, the WB’s total budget proxied by the IBRD equity-to-loan ratio and IDA funding position in combination with the country’s probability to receive aid.²⁸ This instrument is used as a robustness check and to ensure comparability of the results to other studies in the aid literature, however, it is not used as the main strategy due to concerns in regard to issues highlighted by Christian and Barrett (2017).

This paper exploits a new version of an instrument that has been well studied regarding its validity (de Mesquita & Smith, 2010; Fischer, 2022) and strength (Dreher et al., 2018; Fischer, 2022) in recent aid literature.²⁹ It uses the quasi-random allocation of holding the United Nations Security Council (UNSC) presidency lagged by three years interacted with the initial number of projects to gain variation at the district level. Research has shown that donors³⁰ increase their aid funds towards temporary members of the United Nations Security Council (UNSC) which receive increased amounts of aid during their tenure. For African countries this additional aid inflow is quasi-random as a strict rotational rule to participate in the UNSC is enforced (Dreher et al., 2008, 2009).³¹ Own estimations indicate that the increase in aid is mainly predicted by holding the presidency of the UNSC in a certain year and less so by the membership itself. Table A.21 reports the corresponding results. The presidency introduces further exogeneity as it rotates every month and changes based on the English names of the members following the alphabet (United Nations, 2022).³² As an increase in aid is likely not an immediate response but delayed, the UNSC presidency dummy is lagged by three years ($t-3$) to instrument aid at time $t-1$ and causing growth at time t , following

²⁸Data for this instrument is used from Dreher et al. (2021).

²⁹Fischer (2022) employs a seemingly similar instrument which uses a binary variable for holding the presidency of the UNSC interacted with the number of projects a country received. As this is used for all developing countries and not exclusively for Africa, the exclusion restriction is likely violated. Moreover, the delay between UNSC membership to receiving aid disbursements is disregarded and as the projects were counted on a country level, there is no variation on the district level. Although, based on this idea, the instrument used in this paper is further refined to be strong and valid in this specific context.

³⁰Kuziemko and Werker (2006) show this in the case of the US, Dreher et al. (2015) provide similar evidence for Germany and Dreher et al. (2009) for Japan and multilateral organizations. Dreher et al. (2018) give an overview.

³¹There are 5 permanent members (China, France, Russia, the UK and the US) and 10 temporary members which are voted by the UN General Assembly and which hold their seat for 2 years. The membership is not immediately renewable. The 10 temporary seats are distributed between the world regions. Latin America and Asia hold competitive elections which is usually won by the regional power, Western Europe mixes elections with rotation and Eastern Europe has no systematic patterns (Dreher et al., 2018; Malone, 2000)

³²The presidency instigates additional power as the country holding the presidency sets the agenda. At the same time, it is quasi-random if a country holds the presidency during its term at the UNSC or not.

a modified³³ version of the proposed timeline in Dreher et al. (2018).

The additional influx of aid for recipient countries is likely motivated by geopolitical interests. Dreher et al. (2008) demonstrate that the US buys voter compliance in the UN General Assembly through aid, while Dreher et al. (2018) indicate that politically motivated aid distributed while a recipient country was a temporary member of the UNSC is less effective. Any results should therefore be interpreted as a local average treatment effect (LATE) of politically motivated aid. Another concern targeting the exclusion restriction is that UNSC membership itself has an impact on growth. Due to the territorial scope, this issue is partly addressed and additionally findings by de Mesquita and Smith (2010) indicate that election to the UNSC is unrelated to development concerns within the member country.

Table A.21: UNSC Membership and Presidency Relevance

Panel A: GoDAD				
	Membership	Presidency	Both	Presidency $\times$ initial projects
UNSC membership $t - 3$	0.6497*** (0.0625)		-0.0899 (0.0911)	
UNSC presidency $t - 3$		0.8269*** (0.0652)	0.9160*** (0.0882)	
Presidency $t - 3 \times q_{d,1995}$				-1.1063*** (0.2789)
Observations	108020	108020	108020	108020
Districts	5401	5401	5401	5401
Adjusted R-squared	0.5412	0.5415	0.5415	0.5405
Panel B: AidData				
	Membership	Presidency	Both	Presidency $\times$ initial projects
UNSC membership $t - 3$	0.3604*** (0.0583)		-0.0752 (0.0899)	
UNSC presidency $t - 3$		0.4649*** (0.0591)	0.5395*** (0.0818)	
Presidency $t - 3 \times q_{d,1995}$				-0.9407*** (0.2532)
Observations	108020	108020	108020	108020
Districts	5401	5401	5401	5401
Adjusted R-squared	0.5530	0.5531	0.5531	0.5528

Standard errors in the parentheses are clustered at the district level. The dependent variable is $\log(1 + \text{lagged aid})$. The sample covers 1996–2015. All specifications include direct post-conflict exposure, the baseline controls, and district, country, and year fixed effects. Panel A uses GoDAD aid data and Panel B uses AidData. The final column interacts UNSC presidency at $t-3$ with the source-specific number of district projects in 1995. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.20 compares the aid coefficients obtained with the two instruments. The budget-based instrument retains a negative interaction between aid and direct post-conflict exposure, while its main aid coefficient is not statistically distinguishable from zero. The UNSC-based instrument produces a positive main aid coefficient, but the interaction estimates are imprecise. Its first stage is also weaker than that of the budget-based instrument. The SSIV estimates therefore provide complementary, but mixed evidence on the aid coefficients and do not constitute a direct replication of the baseline specification reflecting the

³³Dreher et al. (2018) use 4-year averages with shares of UNSC membership within those 4 years, which is then followed by a 4-year average for the disbursement.

mentioned potential issues with an SSIV approach to identify subnational variation. Furthermore, the results are only obtained for a different time period than for the baseline specification as instruments are only available up to 2014 for the IDA/IBRD instrument and further instruments seem to not be valid anymore for a longer timeframe after 2015 as the initial variation in project counts does not provide enough identifying variation more than 25 years later.

Table A.22: First-Stage Results

Panel A: GoDAD				
	IDA: Aid	IDA: Aid $\times$ PC	UNSC: Aid	UNSC: Aid $\times$ PC
IBRD ratio $\times p_d$	0.0114 (0.0169)	-0.0379*** (0.0038)		
IDA position $\times p_d$	-0.3299*** (0.0072)	-0.0179*** (0.0017)		
IBRD instrument $\times PC_d$	0.0745** (0.0360)	0.5905*** (0.0284)		
IDA instrument $\times PC_d$	-0.0270*** (0.0101)	-0.0551*** (0.0090)		
Presidency $t - 3 \times q_{d,1995}$			-0.8562*** (0.3027)	-0.3435*** (0.1103)
Presidency instrument $\times PC_d$			-0.3754 (0.3528)	3.7430*** (0.5624)
Observations	102619	102619	108020	108020
Districts	5401	5401	5401	5401
Partial R-squared	0.1109	0.4470	0.0001	0.0014
Excluded-instrument F	587.643	668.590	23.911	24.051
SW conditional F	831.081	1075.778	38.243	51.773
Panel B: AidData				
	IDA: Aid	IDA: Aid $\times$ PC	UNSC: Aid	UNSC: Aid $\times$ PC
IBRD ratio $\times p_d$	0.2220*** (0.0152)	-0.0138*** (0.0029)		
IDA position $\times p_d$	-0.1560*** (0.0079)	-0.0031** (0.0014)		
IBRD instrument $\times PC_d$	0.1395*** (0.0425)	0.6310*** (0.0331)		
IDA instrument $\times PC_d$	-0.0363*** (0.0135)	-0.0451*** (0.0111)		
Presidency $t - 3 \times q_{d,1995}$			-0.9742*** (0.2571)	-0.1284*** (0.0467)
Presidency instrument $\times PC_d$			0.3834 (0.2945)	3.1786*** (0.5065)
Observations	102619	102619	108020	108020
Districts	5401	5401	5401	5401
Partial R-squared	0.0583	0.5487	0.0002	0.0011
Excluded-instrument F	194.749	889.019	14.783	22.241
SW conditional F	266.917	2237.975	32.546	42.271

Standard errors in the parentheses are clustered at the district level. All specifications include country, district and year-fixed effects and the baseline controls. Aid is $\log(1+\text{lagged aid})$; PC denotes direct local post-conflict exposure. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.21 reports the separate relevance regressions for UNSC membership, the presidency, both indicators jointly, and the presidency interacted with initial district project counts. Table A.22 reports the corresponding first-stage regressions for both aid measures.

B.8.1 Instrument Validity

One of the main concerns when instrumenting determinants of growth is that the employed instrument violates the exclusion restriction (Bazzi & Clemens, 2013; Clemens et al., 2012). Clemens et al. (2012) showed for example for the studies of Boone (1996) and Rajan and Subramanian (2008) that when including a control for population, their instruments collapse. Therefore, Table A.20 already indicates the results including controls for total population at the district and country level, showing that the instrument used for aid in this study is robust to including such population controls.

One specific concern to my setting is the deployment of peacekeeping in post-conflict districts. Peacekeeping deployment has the potential to affect growth in post-conflict districts as well as it could affect the instrument. UNSC presidency might not solely cause growth through increased aid, but also growth through increased peacekeeping deployment. As the research question targets aid to post-conflict locations, UNSC membership could make the deployment of peacekeeping troops more likely. To address this concern, peacekeeping deployment at the district level is included as a control.

Table A.23 in Appendix B.8.1 shows estimation results including different peacekeeping specifications. The results indicate peacekeeping does not invalidate the instrument as the coefficient for aid in the main specification shown in column (2) of Table A.23 is not altered in its magnitude or strength. If peacekeeping distorts the instrument, this would be particularly relevant for the aid-growth relationship in post-conflict districts. As the coefficient of the interaction term merely changes, the exclusion restriction holds in terms of peacekeeping deployment. The Kleibergen-Paap tests for underidentification and weak identification demonstrate as well the robustness of the instrument against the inclusion of a dummy for peacekeeping deployment. To further strengthen the validity of the baseline results, I estimate a specification that includes the peacekeeping variable and, additionally, an interaction term of peacekeeping and initial aid projects, which can be interpreted as districts with more aid projects in 1995 receiving differential peacekeeping deployment. This interaction term is insignificant for Panel B and only significant at the 10 percent level in Panel A and despite its inclusion, magnitude and significance of the main coefficients are not altered compared to the baseline SSIV shown in Table A.20.

A further concern regarding a potential violation of the exclusion restriction is omitted growth determinants as stated by Bazzi and Clemens (2013). I, therefore, include potential determinants of growth on the country level like FDI flows, trade and, likely important in the case of developing and especially post-conflict countries, migration. Table A.24 in Appendix B.8.1 shows the results when including those variables. The results indicate that the inclusion of those controls makes results more align with the baseline fixed effects specifications with the budget-based instrument indicating positive significant coefficients for aid for both datasets. In terms of the 2SLS estimations, the Kleibergen-Paap tests show stronger results for the UNSC-instrument, thus the instrument and results prove to be robust to other growth determinants. In light of the discussion in regards of shift-share instruments (Borusyak et al., 2021; Christian & Barrett,

Table A.23: Peacekeeping Controls: OLS and Shift-Share IV

Panel A: GoDAD			
	OLS	IV-IDA/IBRD	IV-UNSC
$\ln(1 + Aid_{t-1})$	0.0002*** (0.0001)	-0.0000 (0.0002)	0.0298** (0.0119)
$\ln(1 + Aid_{t-1}) \times PC_d$	-0.0006*** (0.0002)	-0.0011*** (0.0003)	0.0045 (0.0045)
Peacekeeping deployment	0.0179*** (0.0043)	0.0130*** (0.0041)	-0.0310 (0.0230)
Peacekeeping $\times q_{d,1995}$	-0.0135* (0.0071)	-0.0109* (0.0063)	0.0749* (0.0439)
Observations	108020	102619	108020
Districts	5401	5401	5401
District FE	✓	✓	✓
Country FE	✓	✓	✓
Year FE	✓	✓	✓
Heterogeneous trends			
Baseline controls	✓	✓	✓
Mean growth	0.015	0.013	0.015
KP Wald F		583.674	4.944
Underidentification p		0.000	0.003
Panel B: AidData			
	OLS	IV-IDA/IBRD	IV-UNSC
$\ln(1 + Aid_{t-1})$	0.0002** (0.0001)	0.0003 (0.0003)	0.0260*** (0.0082)
$\ln(1 + Aid_{t-1}) \times PC_d$	-0.0005** (0.0002)	-0.0010*** (0.0002)	-0.0022 (0.0022)
Peacekeeping deployment	0.0176*** (0.0043)	0.0117*** (0.0041)	-0.0136 (0.0133)
Peacekeeping $\times q_{d,1995}$	-0.0068 (0.0078)	-0.0004 (0.0075)	0.0411 (0.0260)
Observations	108020	102619	108020
Districts	5401	5401	5401
District FE	✓	✓	✓
Country FE	✓	✓	✓
Year FE	✓	✓	✓
Heterogeneous trends			
Baseline controls	✓	✓	✓
Mean growth	0.015	0.013	0.015
KP Wald F		195.187	7.431
Underidentification p		0.000	0.000

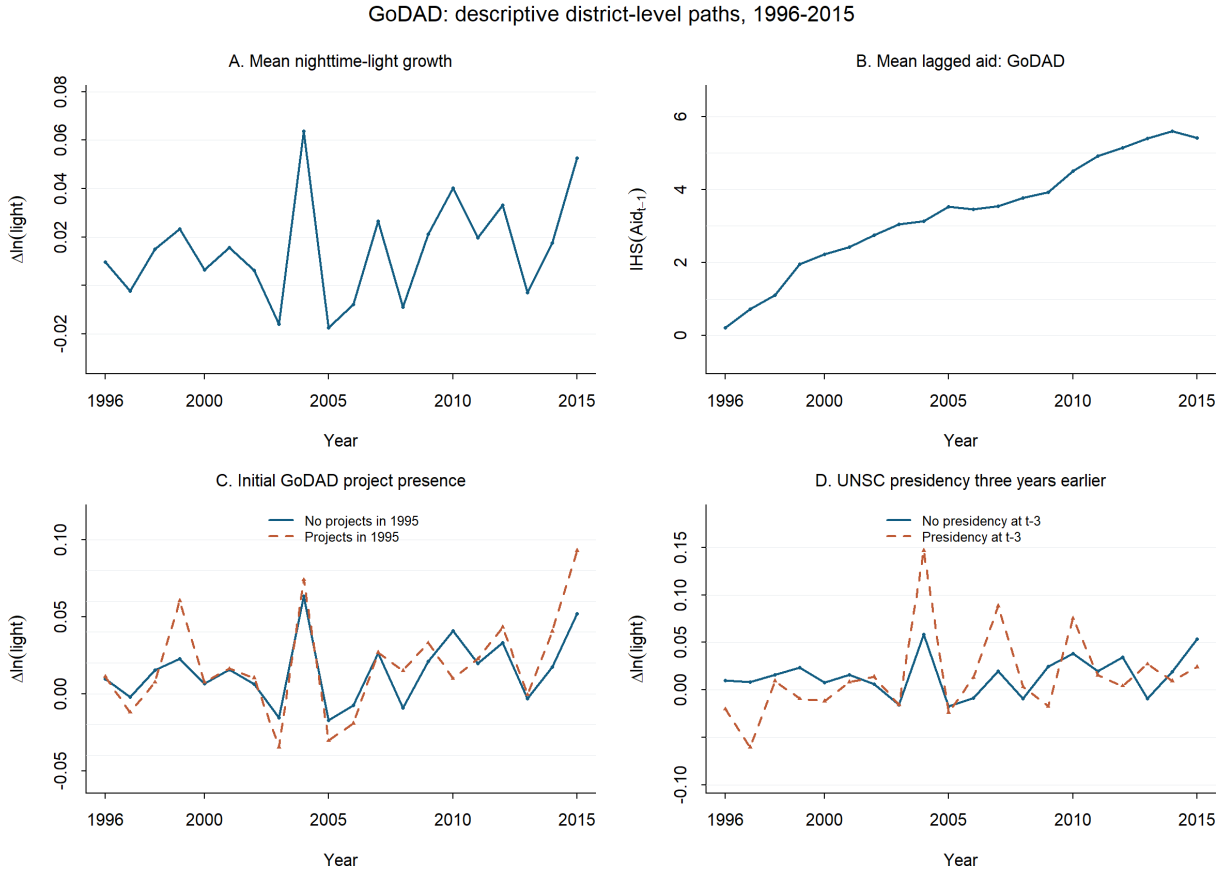
Standard errors in the parentheses are clustered at the district level. All specifications include country, district and year-fixed effects and the baseline controls. Panel A uses GoDAD aid data and Panel B uses AidData. Each specification includes peacekeeping and its interaction with the source-specific district project count in 1995. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

2017; Goldsmith-Pinkham et al., 2020), Figures 12 and 13 show trends in nighttime-light growth by initial aid-project presence in 1995 and UNSC presidency three years earlier, separately for GoDAD and AidData.

Overall, as this SSIV-approach mainly identifies the effect of politically motivated aid and concerns

based on Borusyak et al. (2021), Christian and Barrett (2017), and Goldsmith-Pinkham et al. (2020) might still apply, the main empirical strategy of this study uses an OLS estimator with rigorous fixed effects and incorporating controls for heterogeneous trends of districts and countries to target any remaining endogeneity. A similar approach between these two empirical methodologies has been used in Gehring et al. (2022), employing heterogeneous trends and OLS estimations in their preferred strategy and showing the robustness of their results via the aforementioned IV strategy using the IDA financial position in combination with a district's probability to receive aid.

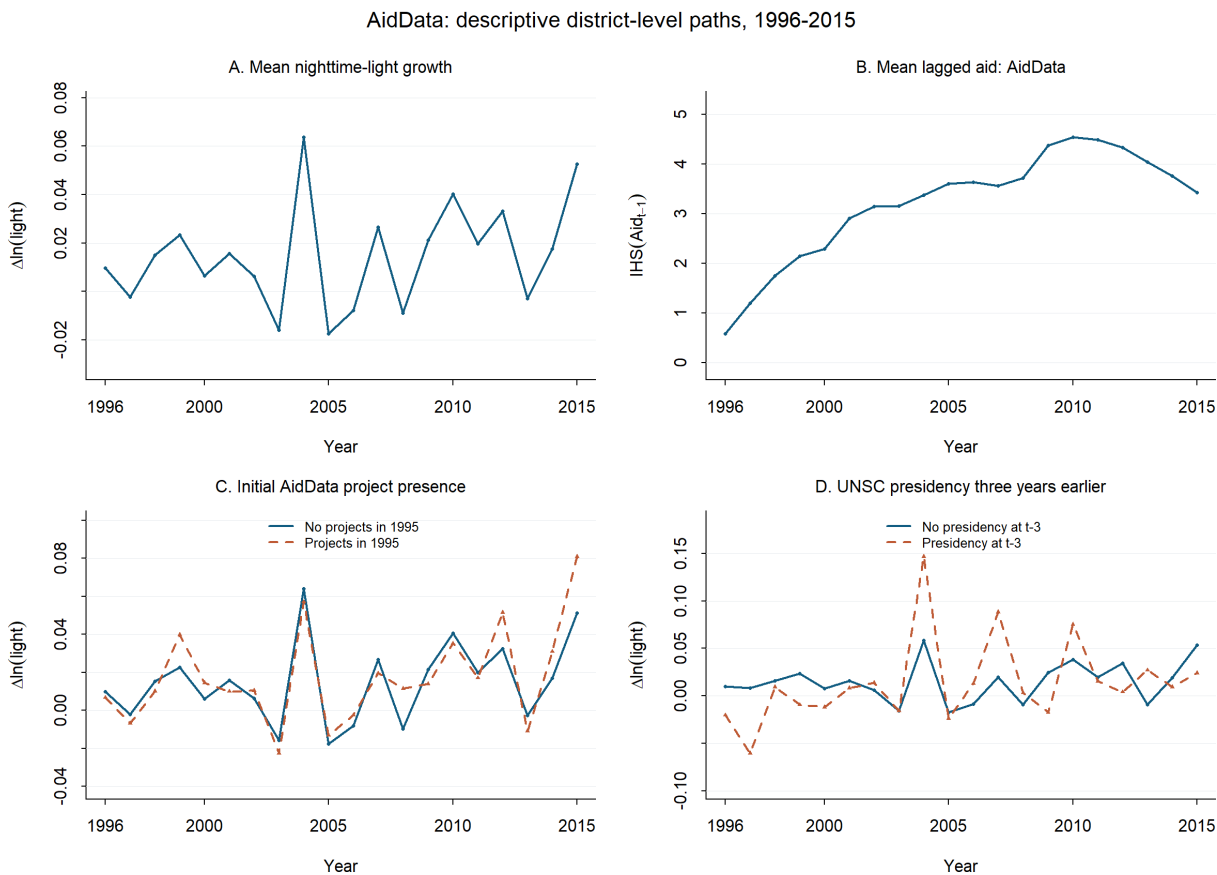
Figure 12: District Growth and Aid Paths: GoDAD



Common baseline sample: 108,020 district-years; 5401 districts. Unweighted annual means.
Initial projects refer to 1995. The presidency split uses the instrument's t-3 timing.

Notes. The figure reports unweighted annual district means for 1996–2015 in the common complete-case baseline sample of 108,020 district-year observations and 5,401 districts. Panel A shows mean nighttime-light growth, and Panel B shows mean lagged GoDAD disbursements transformed using the inverse hyperbolic sine. Panel C compares nighttime-light growth between districts with and without GoDAD projects in 1995. Panel D compares districts whose countries held the UNSC presidency three years earlier with districts whose countries did not. The panels describe annual paths in the baseline sample before imposing instrument-specific data restrictions.

Figure 13: District Growth and Aid Paths: AidData



Common baseline sample: 108,020 district-years; 5401 districts. Unweighted annual means.
Initial projects refer to 1995. The presidency split uses the instrument's t-3 timing.

Notes. The figure reports unweighted annual district means for 1996–2015 in the common complete-case baseline sample of 108,020 district-year observations and 5,401 districts. Panel A shows mean nighttime-light growth, and Panel B shows mean lagged AidData disbursements transformed using the inverse hyperbolic sine. Panel C compares nighttime-light growth between districts with and without AidData projects in 1995. Panel D compares districts whose countries held the UNSC presidency three years earlier with districts whose countries did not. The panels describe annual paths in the baseline sample before imposing instrument-specific data restrictions.

Table A.24: Country-Level Growth Controls: OLS and Shift-Share IV

Panel A: GoDAD			
	OLS	IV-IDA/IBRD	IV-UNSC
$\ln(1 + Aid_{t-1})$	0.0001 (0.0001)	0.0005** (0.0002)	0.0208*** (0.0065)
$\ln(1 + Aid_{t-1}) \times PC_d$	-0.0010*** (0.0003)	-0.0018*** (0.0003)	0.0056 (0.0043)
FDI inflows (USD billions)	-0.0000 (0.0003)	0.0001 (0.0002)	0.0017** (0.0007)
Trade (percent of GDP)	-0.0000 (0.0000)	-0.0000 (0.0000)	-0.0003*** (0.0001)
Net migration (thousands)	0.0000** (0.0000)	-0.0000 (0.0000)	0.0000** (0.0000)
Observations	82991	78585	82991
Districts	4464	4464	4464
District FE	✓	✓	✓
Country FE	✓	✓	✓
Year FE	✓	✓	✓
Heterogeneous trends			
Baseline controls	✓	✓	✓
Mean growth	0.016	0.014	0.016
KP Wald F		434.821	10.171
Underidentification p		0.000	0.000
Panel B: AidData			
	OLS	IV-IDA/IBRD	IV-UNSC
$\ln(1 + Aid_{t-1})$	0.0001 (0.0001)	0.0011*** (0.0003)	0.0194*** (0.0054)
$\ln(1 + Aid_{t-1}) \times PC_d$	-0.0008*** (0.0003)	-0.0017*** (0.0003)	-0.0039* (0.0021)
FDI inflows (USD billions)	-0.0000 (0.0003)	0.0001 (0.0002)	-0.0004 (0.0004)
Trade (percent of GDP)	-0.0000 (0.0000)	-0.0000 (0.0000)	-0.0000 (0.0001)
Net migration (thousands)	0.0000** (0.0000)	-0.0000 (0.0000)	0.0000 (0.0000)
Observations	82991	78585	82991
Districts	4464	4464	4464
District FE	✓	✓	✓
Country FE	✓	✓	✓
Year FE	✓	✓	✓
Heterogeneous trends			
Baseline controls	✓	✓	✓
Mean growth	0.016	0.014	0.016
KP Wald F		163.395	11.774
Underidentification p		0.000	0.000

Standard errors in the parentheses are clustered at the district level. All specifications include country, district and year-fixed effects and the baseline controls. Panel A uses GoDAD aid data and Panel B uses AidData. FDI is measured in current USD billions, trade as a percentage of GDP, and net migration in thousands. The sample is the common complete-case sample for the three country-level controls. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$