Foreign Aid and Targeted Political Violence*

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Abstract: Elections in fragile democracies are not merely contests over policy but battles for control over state resources, including foreign aid. Aid provides local governments with substantial discretionary funds, creating strong incentives for rent-seeking political actors to capture political office. To win elections, political actors, both in government and opposition, try to reduce electoral competition through targeted political violence, especially in weakly institutionalized settings, where the economic stakes from gaining (or losing) office are higher and the potential costs of using targeted violence are limited. We empirically test this argument using novel geo-located data on aid disbursements from 18 European donors and the United States, covering the period from 1990 to 2020. Applying an instrumental variables (IV) approach, we find that foreign aid is associated with higher levels of targeted political violence against local authorities and politicians, in particular during elections and in contexts with weak institutions and strong informal politics. These findings highlight the unintended consequences of foreign aid, showing how it can lead to targeted political violence by increasing the stakes of political competition.

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1 Introduction

> "Why will we kill her, assassinate her, when she is not even an official? And we know very well that she is not going to win. Maybe if she is 'winnable."' (Baybay City Mayor Carmen Loreto-Cari denying involvement in an attempted assassination of her political rival Marilou Galenzoga in 2013; quoted in Rappler 2013)[1]

Economic expectations and the provision of public goods are central to political survival in democracies and autocracies alike.[2] Voters reward or punish politicians based on their ability to deliver essential services, infrastructure, and economic stability. In developing countries, where domestic revenue is often low, foreign aid becomes a crucial supplement to government funds, making it a powerful political resource. Given the fiscal significance of foreign aid in developing countries, a large body of research has examined how foreign aid can be misappropriated for personal and political gain.[3] Most of this work emphasizes how incumbents use foreign aid to gain and maintain power by reinforcing local clientelist networks or enhancing their electoral legitimacy. Our paper highlights a more coercive and underexplored dynamic: the idea that the value of aid itself, as a discretionary and potentially embezzlable resource, can incentivize targeted violence by the government and opposition actors against political rivals. Rather than relying solely on patronage or persuasion, political actors may turn to violence as a strategy to reduce electoral competition, secure victory at the polls, and gain control over lucrative aid resources.

We argue that when foreign aid provides discretionary resources to local governments, both incumbents and opposition groups have strong incentives to employ targeted political violence as a strategy to improve their chances of getting elected, and with it, access to valuable aid resources. Targeted political violence entails intimidating or attacking political rivals, deterring potential candidates from running for office, coercing voters into supporting specific candidates, and terrorizing populations to manipulate voter turnout in favor of a particular faction. In weakly institutionalized settings, where the political and economic stakes of power are high, and political and legal accountability is low, eliminating rivals through violence can become a rational electoral strategy. The combination of ineffective law enforcement, impunity for political elites, and the absence of strong institutions amplifies both the incentives for violence and the lack of deterrence. These conditions make targeted violence not only feasible, but also a pragmatic approach

[1] https://www.rappler.com/philippines/elections/28541-we-may-kill-her-if-she-were-winnable/, last accessed: April 2, 2025.

[2] See, for example, Lewis-Beck (1980, 2006), Duch and Stevenson (2008, 2010), and Knutsen (2014).

[3] On the use of aid for personal gain see Svensson (2000), Knack (2001), Alesina and Weder (2002) and Djankov et al. (2008). On the use of foreign aid for political gain, see Kono and Montinola (2009), Licht (2010), Ahmed (2012), Rota-Graziosi et al. (2012), Guiteras and Mobarak (2015), Cruz and Schneider (2017); oftentimes politicians do this by redirecting aid to strengthen their political base (Jablonski 2014, Hodler and Raschky 2014, Briggs 2017, Dreher et al. 2019, Anaxagorou et al. 2020, Bomprezzi et al. 2025a) or to increase government legitimacy (Dietrich and Winters 2015, Dietrich et al. 2018, Baldwin and Winters 2020, Blair and Roessler 2021, O'Brien-Udry 2025).

to secure electoral victory.

To empirically evaluate this argument, we leverage novel geo-coded data on foreign aid disbursements from 18 European donors and the United States to sub-national jurisdictions in up to 121 countries between 1990 and 2020 (Bomprezzi et al. 2025a). We combine the data on aid with data on sub-national targeted political violence using multiple sources (Daxecker et al. 2019, START 2022, Fjelde and Höglund 2022, Raleigh et al. 2023) and data on the timing of elections (Beck et al. 2001). Using both ordinary least squares (OLS) and an instrumental variables (IV) approach that addresses endogeneity concerns, we find robust evidence in support of our theoretical expectations. Specifically, leveraging exogenous variation in donor political fragmentation and sub-national aid patterns, our IV estimates suggest that foreign aid can exacerbate cycles of electorally motivated targeted violence. Rather than promoting stability, aid inflows may incentivize political actors to resort to coercive strategies in order to eliminate rivals, secure electoral victory, and gain control over discretionary and embezzlable aid resources. In line with our argument, the results also show that these incentives are particularly strong prior to elections and in weakly institutionalized systems. Finally, we demonstrate that the results are distinct from broader trends in political violence and civil conflict.

Our findings highlight that foreign aid does not only provide governments, especially at the local level, with discretionary resources that they can allocate to increase public support (Ahmed 2012, Jablonski 2014, Guiteras and Mobarak 2015, Cruz and Schneider 2017, Baldwin and Winters 2020, Shea et al. 2025), but it also generates strong incentives for political actors to capture office by coercing and murdering political competition, thus increasing their own electoral prospects to gain access to foreign aid resources. Although existing research has examined extensively how incumbents use aid for clientelism and political survival, we provide the first systematic argument that aid also fuels targeted electoral violence by making political power a more valuable and contested prize, incentivizing political groups to resort to targeted assassinations and coercion as a means of winning elections.

Our findings relate to the literature on foreign aid and conflict, in particular scholarship showing that conflict is the consequence of rival groups fighting over resources (Grossman 1992, Arcand and Chauvet 2001, Besley and Persson 2011, Berman et al. 2012, Dube and Vargas 2013, Mihalache-O'Keef 2018, Ahmed et al. 2021, How Choon et al. 2024), especially natural resources (Collier 2004, Regan and Norton 2005, Dreher and Kreibaum 2016, Hunziker and Cederman 2017, Berman et al. 2017).[4] Although our argument captures important aspects of conflict and violence, we do not focus on wide-

[4]An interesting counterpoint is the argument that aid increases the fiscal capacity of the government, thereby discouraging the opposition to use violence (Collier and Hoeffler 2002, Collier 2010, Ahmed and Werker 2015, Lyall 2019). A related literature has focused on the effect of aid on terror, at the level of countries (Young and Findley 2011, Bandyopadhyay et al. 2014, Savun and Tirone 2018, Boutton 2019, Kim and Sandler 2021, Dimant et al. 2024).

scale civil conflict or clear acts of terrorism, but highlight a different form of targeted political violence whereby the political opposition and the government use violence as a means to increase their chances to get elected or stay in power, mainly by conducting targeted killings of politicians or candidates, deterring other politicians to run for office, and coercing voters into voting for preferred candidates. Indeed, our results demonstrate that targeted political violence operates as a distinct phenomenon, separate from the broader civil conflicts that have been the primary focus of the foreign aid literature. As such, our argument builds on existing research about political violence and politically-motivated assassinations (Jones and Olken 2009, Robinson and Torvik 2009, Collier and Vicente 2012, Acemoglu et al. 2013, Hafner-Burton et al. 2014, 2018, Staniland 2014, Daniele and Geys 2015, Daniele and Dipoppa 2017, Ley 2018, Alesina et al. 2019, Bapat 2019, Rauschenbach and Paula 2019, Fjelde 2020, Birch 2020, Magaloni et al. 2020, Wahman and Goldring 2020, Trejo and Ley 2021, Carvalho Barbosa 2022).[5] This literature has focused on a range of societal, institutional, and international factors,[6] but it has not considered the effect of foreign aid inflows. By analyzing the effect of foreign aid flows, we are also able to present a direct empirical test of rent-seeking incentives and election violence.[7] Unlike natural resource revenues such as oil, which are often associated with rent-seeking and violent competition, foreign aid is typically viewed as a governance-enhancing resource. As Bermeo (2016) argues, "aid is not oil": it is more likely to be allocated to democratic states, comes with oversight mechanisms, and reflects donor priorities for transparency, stability, and development. While not all aid is disbursed with governance in mind—geopolitical and strategic considerations often shape where and how donors allocate funds, sometimes diluting conditionalities or accountability—these features should, in principle, reduce the incentives for violence. Donors may also withdraw aid or impose penalties if they detect widespread misuse or political instability, which ought to further constrain its abuse. From this perspective, one might expect foreign aid to generate fewer incentives for politically motivated violence. Yet our findings suggest that even foreign aid—despite its conditionalities and monitoring—can function as a politically valuable rent. In this way, our analysis extends the rent-seeking literature by showing that not only extractive rents, but even ostensibly developmental transfers like aid, can fuel coercive political competition under the right conditions.

In addition to presenting first evidence that foreign aid increases targeted political

[5]Political violence broadly includes any use of force, intimidation, or coercion for political ends, ranging from protests and insurgencies to state repression, while targeted political violence specifically refers to the strategic alliance between political actors and armed groups to influence electoral outcomes and maintain patronage networks.

[6]Birch et al. (2020) provide an excellent summary of the literature.

[7]Testing the rent-seeking argument is notoriously difficult and scholars have tended to discuss rent-seeking incentives more implicitly (Bratton 1989)—for example by measuring the presence of corruption and patronage (Birch 2011, Acemoglu and Robinson 2012, Berenschot 2020, Birch 2020)—or they have focused on the potential access to natural resources or drug trafficking routes (Acemoglu et al. 2013, Carreri and Dube 2017, von Borzyskowski 2019, Gutiérrez-Romero and Iturbe 2024).

violence against incumbent and opposition actors, our study makes several empirical contributions. We leverage a novel combination of geo-coded aid disbursement data and subnational records of targeted political violence in up to 121 countries from 1990 to 2020. This allows us to systematically analyze how foreign aid influences the use of electoral violence as a strategy for political competition. By incorporating multiple donors and examining aid allocation at the subnational level, our approach extends previous research on aid and conflict, which either focuses on the level of countries or, when sub-national regions are analyzed, are limited to just one recipient country or single donors.[8] The focus on sub-national regions is particularly important for our argument, as many foreign aid projects are implemented locally, thereby generating geographically targeted incentives that shape both government strategies to defend aid resources and opposition groups' motivations to capture them through coercive means.

2 The Argument

We now present a theory explaining how foreign aid influences electorally motivated assassinations of politicians. In a nutshell, we argue that expectations about access to aid resources provide a crucial incentive for political actors, both in government and in the opposition, to engage in targeted political violence. By eliminating political competitors, these political actors hope to increase their likelihood of winning or maintaining power and securing control over aid-financed state resources. The effect should be stronger in weakly institutionalized contexts where aid resources are more easily captured and accountability mechanisms are weaker.

In many developing countries, foreign aid plays a critical role in government finances, often exceeding domestically generated revenue. Between 2010 and 2020, net Official Development Assistance (ODA) accounted for an average of 27% of total government expenditures across aid-recipient nations, with some countries receiving aid surpassing their entire national budgets. For instance, in 2020 the Central African Republic received aid equivalent to 250% of its government spending, while the Democratic Republic of Congo and Ethiopia received 76% and 58%, respectively (OECD Aid Statistics).[9] While some of this aid is distributed as general budget support to national governments, a substantial share is implemented through locally targeted development projects. International donors frequently rely on local governments—particularly at the municipal and district levels—to identify priorities, allocate funds, and oversee project implementation in key sectors like infrastructure, education, and health (Cruz and

[8]Examples are Savun and Tirone (2018), Bluhm et al. (2021), Kim and Sandler (2021), Gehring et al. (2022), Loewenthal et al. (2023), Aja-Eke and Brazys (2024), Mueller (2025) and Bomprezzi et al. (2025b). Hoeffler (2025) provides a recent survey. We also contribute to research on electoral violence and conflict more broadly (Wilkinson 2006, von Borzyskowski and Kuhn 2020).

[9]`http://www.oecd.org/dac/stats/idsonline`, last accessed: April 2, 2025.

Schneider 2017, Ijaz 2023). Local officials often co-finance projects, manage procurement processes, and serve as the primary authorities for monitoring and service delivery. In bilateral donor programs such as Germany's GIZ-supported decentralization initiatives or the United States' USAID-funded community-driven development projects in sub-Saharan Africa, mayors and municipal councils have exercised substantial discretion over how aid is deployed within their jurisdictions. The fiscal importance of foreign aid at the subnational level can be even greater than at the national level. In one striking case, USAID allocated US$ 17 billion for reconstruction in Afghanistan in 2017, with much of it directed to local projects. A single Afghan district—comparable in size to a U.S. county—was projected to receive approximately US$ 3 million per day (Whitlock 2021, 159). This decentralized structure not only positions local governments as critical implementation partners, but also as direct beneficiaries of aid flows, creating strong incentives for both effective use and potential misuse of these politically and economically valuable resources.

Local control over aid disbursement creates strong electoral incentives at the subnational level. Winning local office often provides direct access to lucrative municipal contracts, infrastructure tenders, and development funds. Aid-related resources are not only financially significant relative to local budgets, but also highly visible to constituents, making them politically valuable for signaling competence and responsiveness. What is more, because voters are more likely to attribute service delivery, or its absence, to local officials, the political stakes of managing and controlling foreign-funded projects are especially high. Although our focus is on the subnational dynamics of electoral violence, these contests are not insulated from broader political competition. Political actors often seek to influence national legislative elections to expand their networks and consolidate influence across levels of government.[10]

The primary objectives of misappropriating aid are self-enrichment, bolstering the politician's local political standing through patronage networks, or a combination of both. Political elites frequently exploit foreign aid through rent-seeking and personal gain (Svensson 2000, Knack 2001, Alesina and Weder 2002, Djankov et al. 2008). They siphon off aid funds using inflated contracts, kickbacks, and direct embezzlement, with development projects serving as convenient vehicles for financial gain. Empirical evidence indicates that substantial portions of aid never reach intended beneficiaries, instead vanishing into elite-controlled networks. For example, Andersen et al. (2022) show that a significant share of foreign aid ends up in offshore accounts rather than being invested in public services. Similarly, Nikolova and Marinov (2017) demonstrate how local municipalities misused significant amounts of aid from the European Union that

[10]While our empirical analysis focuses on the violent consequences of the geographic distribution of aid within countries, our theoretical argument should also apply to national-level politicians seeking access to centrally distributed aid, such as budget support.

had been given to local governments to implement disaster relief effects.[11] The scale of misuse can be staggering. Estimates suggest that at least 25% of World Bank development funds have been misappropriated in recipient countries (Moyo 2010, 52). And much of the vast sums allocated to Afghanistan through USAID's reconstruction program in districts across the country ultimately ended up in the hands of overpriced contractors and corrupt Afghan officials (Whitlock 2021, 159). Even when aid is formally earmarked for specific programs, its allocation is rarely neutral. Incumbents often manipulate aid distribution to favor family members, politically strategic constituencies, and loyal supporters, thereby reinforcing their patronage networks (Jablonski 2014, Hodler and Raschky 2014, Briggs 2017, Dreher et al. 2019, Mares and Young 2019, Scheiring 2020, 2021, Bommer et al. 2022, Berlin et al. 2023, Bomprezzi et al. 2025a). To the extent that aid is fungible, it also enables political actors to redirect existing budget resources towards other personal priorities (Pettersson 2007, Marć 2017, Cruzatti et al. 2023).

Foreign aid can therefore also be strategically (mis)used to influence electoral competition in favor of incumbent officeholders who control its distribution. Political survival in these settings is closely tied to economic performance and the provision of public goods, as voters reward governments that expand infrastructure, improve economic conditions, and deliver essential services (Lewis-Beck 1980, 2006, Duch and Stevenson 2008, 2010). This creates a powerful incentive for incumbents to strategically allocate resources to maximize electoral support, particularly before elections (Alesina and Roubini 1992, Alesina et al. 1993, 1997, Drazen 2000, Franzese 2002, Alt and Lassen 2006, Kayser 2009, Kersting and Kilby 2016). These incentives are particularly strong in developing countries (Shi and Svensson 2006), where clientelism and patronage dominate electoral politics, and governments do not simply use aid to fund public goods, but also to redirect resources to sustain political loyalty (Keefer 2007, Keefer and Vlaicu 2007, Kitschelt and Wilkinson 2007, Hicken 2011, Labonne 2013, Guiteras and Mobarak 2015, Anaxagorou et al. 2020). Aid's fungibility further enhances its political utility (Pettersson 2007). When donors finance essential public services—such as healthcare, education, or infrastructure—governments free up their own budgetary resources for explicitly political purposes, including campaign spending, vote-buying, and patronage distribution. Even when governments don't have direct access to aid resources, they can try and claim credit for the inflow of foreign aid projects. Cruz and Schneider (2017) demonstrate how Filipino mayors have been able to increase electoral support for World Bank projects even though they did not have any control over how those resources were spent. Whether they control foreign aid or not, research strongly indicates that foreign aid inflows allow ruling elites to maintain the appearance of effective governance while simultaneously channeling resources toward ensuring electoral dominance.

[11] Kelemen (2017) further documents how Hungary's prime minister used European Union aid to enrich political allies and family members.

The personal and political value of aid is not confined to its present distribution: many aid projects unfold over several years, often lasting a full electoral cycle or longer. Political actors therefore seek not only immediate access to aid but also future control over its implementation; who gets hired, who benefits, and how resources are allocated. Once a school or health clinic is built, for example, the local government can influence employment decisions, access to services, and procurement processes. These extended benefits amplify the rewards of winning office and create expectations of continued patronage opportunities. Even in settings where future aid allocations are uncertain, politicians may extrapolate from current trends and campaign on promises to deliver aid, reinforcing their incentive to secure electoral victory at all costs.

The ability to control aid resources therefore significantly raises the stakes of elections, making access to foreign aid a key driver of political competition. This increases incentives for political actors to use any means necessary, including the use of targeted violence to suppress electoral competition, in order to win elections and gain access to the aid resources. We identify three primary strategies through which targeted political violence manifests.[12] One of the most direct ways to reduce electoral competition is through targeted attacks on incumbent politicians or opposition challengers. These attacks frequently lead to the death (or near death) of politicians such as the fatal attack on the former Pakistan Prime Minister, Benazir Bhutto, while she was on the campaign trail, or the assassination of two presidential candidates during the Haitian election campaign in 1987 (Birch 2020, 13). The report, *Freedom in the World 2025: The Uphill Battle to Safeguard Rights*, found that of the 66 countries and territories that hosted national elections in 2024, some 40 percent featured election-related violence. Candidates were attacked in at least 20 countries. Violence occurs in both national and local elections, but it is particularly prevalent in local elections. In South Africa, for example, political violence targeting political officials has escalated, with 186 killed between 2000 and 2023; of those, 105 attacks targeted local politicians (Matamba and Thobela 2024). In the 2010 Philippine elections, a clan sent private militias to kill 57 campaigners for the rival clan as well as journalists in the Maguindanao province (White 2015, 33). It is common to see about 100-200 political deaths during Philippine elections, on average. By eliminating key political rivals, political actors can create power vacuums that they or their allies can exploit. Empirical evidence from Colombia shows that assassinations are focused on targeting politicians seen as obstacles to elite-controlled electoral victories (Acemoglu et al. 2013).

Beyond direct targeted attacks that cause bodily harm or death, political actors can also deter potential challengers from running for office through threats and coercion. A

[12]Our mechanisms borrow from commonly accepted dimensions of political violence (Birch 2020). However, concepts of political violence are usually very broad and include acts that are not targeted. Our particular focus is on dimensions of political violence that directly capture political actors' violent strategies to get into positions of political power and capture aid resources for political purposes.

recent example of these tactics could be experienced in the Indian state of Gujarat during the 2024 elections where the candidate of the ruling Bharatiya Janatra Party (BJP) won the seat by default after every other candidate was either disqualified or dropped out of the race. In Gandhinagar constituency, all 15 opposition candidates dropped out, alleging that the ruling party was using party workers and the police to intimidate and put pressure on opposing candidates to withdraw, sometimes with explicit threats of violence or direct harassment of their families. Candidates and their friends and families were stalked excessively by the police and received threats that they would be jailed in some fake case if they didn't withdraw.[13] Arbitrary and bogus arrests of political candidates (or their family members) are also common elsewhere, including in Burundi where over 600 members of the opposition parties were arrested before the 2020 elections; others reported arbitrary beatings and killings.[14] Research on organized crime in Italy has shown that these tactics are geared to increase the chances of election victory: during local elections, criminal groups systematically intimidate strong candidates, thereby ensuring that elections feature only weak or compromised contenders (Daniele and Geys 2015).

Political actors may also suppress voter turnout in areas dominated by rival parties or coerce voters into supporting their preferred candidates (Ley 2018, Alesina et al. 2019, Arjona et al. 2025).[15] In Colombia, paramilitary forces threatened entire communities with violence if they failed to vote for particular candidates. In the municipality of Sanonofre, the paramilitary leader "Cadena" threatened to kill council members if voters did not support his preferred candidate. In other cases, they directly manipulated voter rolls by collecting identity cards to prevent targeted individuals from voting (Acemoglu et al. 2013). Suppressing voter turnout can be a particularly effective strategy in these contexts because changing a voter's preference is often more difficult than preventing them from voting altogether. Intimidation, forced displacement, or logistical barriers to voting can disproportionately affect opposition strongholds, reducing the electoral competitiveness of these regions (Alesina et al. 2019, Rauschenbach and Paula 2019). This form of intimidation ensures that elections are not genuinely competitive, but rather carefully managed exercises in securing power, where the outcome is shaped more by coercive strategies than by voter choice.

Of course, political actors do not always resort to violence to gain access to political

[13]Hannah Ellis-Petersen and Aakash Hassan. May 13, 2024, "Pressured to withdraw: BJP accused of intimidation tactics in India polls." The Guardian. https://www.theguardian.com/world/article/2024/may/14/bjp-accused-intimidation-india-elections-gujarat, last accessed: March 1, 2025.

[14]Human Rights Watch. June 1, 2020. "Burundi: Intimidation, Arrests During Elections." https://www.hrw.org/news/2020/06/01/burundi-intimidation-arrests-during-elections, last accessed: March 1, 2025.

[15]Christensen and Utas (2008) provide insights into these tactics for Sierra Leone. For example, one informant shared that "You know, we have to make people understand how to vote. We have so many illiterates, they know nothing about politics and they don't know their rights. (...) That's why we tell them how to vote (...) If we don't do it by force they will never understand" (Christensen and Utas 2008, 535).

office and control over aid resources. Whereas violence is a more flexible and agile tool that can be deployed relatively rapidly—in contrast to vote-buying which requires a more long-term building of patronage networks—the use of violence always entails significant dangers, including a loss of public legitimacy, the potential retaliation by political opponents, and the risk of legal punishment. The use of violence can severely damage a political actor's legitimacy in the eyes of the public. Legitimacy is crucial for winning and maintaining authority and public support; without it, governance becomes challenging. When political actors resort to violence, they may be perceived as unjust or oppressive, leading to a loss of trust and support among constituents. This erosion of legitimacy can result in decreased public cooperation and increased civil resistance. Violent actions against political opponents can also escalate conflicts, leading to cycles of retaliation. Rival factions or armed groups may respond with their own acts of violence, resulting in prolonged instability and insecurity.

Finally, engaging in political violence may expose perpetrators to legal sanctions. Domestic institutions may investigate or prosecute such acts, and, in principle, international donors could respond by reducing or withdrawing foreign assistance from implicated regions (Swedlund 2017, Cheeseman et al. 2024). If those threats are credible, they should reduce incentives to use targeted political violence. However, empirical evidence on donor punishment is mixed (Cheeseman et al. 2024); in many cases, donors prioritize stability and democratic institution-building in conflict-prone regions rather than withholding support (Findley 2018, Corwin 2023).[16] Even where donor pressure is plausible, it may not serve as a strong deterrent for local politicians. Aid agencies often lack timely or granular information about electoral violence, particularly in rural or subnational contexts, and may be reluctant to act due to geopolitical or strategic considerations. Local actors, in turn, may not perceive donor response as an imminent threat. In many cases, subnational politicians operate with limited exposure to international scrutiny and either not consider the impact of their actions on future delivery of aid or assume that their actions will escape notice by donors, especially when violence is localized. They may also believe—often correctly—that aid flows are driven more by strategic or geopolitical priorities than by local governance standards, reducing their fear of punishment. Even when donors formally condemn electoral violence, actual aid suspensions are rare, and attribution of blame to specific individuals or municipalities is difficult. As a result, local politicians may view domestic law enforcement and judicial consequences as more credible and immediate risks than any potential donor backlash.

Because of these costs, politicians may find non-violent strategies, such as vote-buying, lobbying, or mobilizing grassroots support to be more effective and less costly

[16]Empirically, we use an instrumental variable approach to address any concerns that aid flows may be endogenous to targeted political violence.

(De Luca et al. 2017).[17] We argue that the strategic use of targeted political violence is more pronounced in weakly institutionalized political contexts, where low transparency, high corruption, and weak rule of law create both stronger economic incentives and fewer (political and legal) constraints for engaging in violent electoral manipulation. In settings with strong informal institutions, political actors find it easier to capture foreign aid and re-purpose the resources for personal or political objectives. When oversight institutions such as independent audit agencies, anti-corruption commissions, and civil society watchdogs are weak or compromised, politicians face fewer obstacles in diverting aid into patronage networks, private enrichment, or illicit electoral strategies. Research has shown that aid is disproportionately misused in environments with low bureaucratic capacity and weak institutional constraints, where ruling elites can channel funds to political allies and selectively allocate development projects in ways that maximize electoral advantages (Jablonski 2014, Hodler and Raschky 2014, Briggs 2017).[18] The potential to exploit aid for political gain raises the stakes of elections, making violent strategies like assassinations, coercion, and suppression more attractive for politicians seeking to capture the foreign aid resources.

In addition to creating stronger incentives, weak institutional environments provide fewer deterrents against engaging in political violence. In contexts with low judicial independence and selective enforcement of the law, politicians and their allied armed groups are less likely to face prosecution for assassinations, intimidation, or electoral manipulation. Corruption in the judicial system means that even if opposition politicians or armed actors are implicated in assassinations, they can often avoid serious consequences through bribery or political protection. What is more, where state capacity is low, the government may simply lack the ability to provide sufficient security for local politicians, making them easy targets for violence (Chacon 2018). In such environments, law enforcement may be ineffective, complicit, or even directly involved in coercive electoral strategies. Political elites in these settings can also exert greater control over state security forces, mobilizing them not to prevent violence, but rather to suppress opposition groups or overlook crimes committed by pro-government militias. When state authority is fragmented, law enforcement is weak, and political elites lack full control over coercive power, this creates space for paramilitary groups, militias, and criminal organizations to emerge and operate. These groups usually possess the infrastructure, operational capacity, and organizational networks to engage in coercion, making them natural allies

[17] A significant debate evolves around the question whether vote-buying and political violence are complements or substitutes (Collier and Vicente 2012, Van Ham and Lindberg 2015, Mares and Young 2016, Rauschenbach and Paula 2019), with much of the evidence indicating that vote-buying and violence work in tandem. In line with our argument about targeted political violence as a strategy to eliminate political opponents, Rauschenbach and Paula (2019) find that vote-buying is more likely to target supporters, whereas violence tends to targets opponents.

[18] In line with this, Aidt et al. (2020) show that vote buying prior to elections is limited to countries with weak institutional environments.

for politicians seeking to eliminate rivals, intimidate opponents, and manipulate voter turnout (Bluhm et al. 2021).[19] These same conditions also hinder donor oversight and accountability. When violence is carried out through informal networks or criminal intermediaries, attribution becomes murky, making it difficult for donors to identify the perpetrators or impose targeted consequences. Weak monitoring capacity at the local level further obscures patterns of coercion, increasing the likelihood that violent electoral manipulation goes undetected or unpunished by international actors.

Ultimately, while electoral violence is not inevitable, weakly institutionalized contexts increase both the incentives and opportunities for targeted political violence to emerge. We expect that *foreign aid inflows increase the likelihood of targeted political violence, both by government and opposition actors, but this effect is stronger in weakly institutionalized contexts where aid resources are more easily captured and accountability mechanisms are weaker.*

Direct evidence of the incentives linking aid to political violence is extremely difficult to obtain. No politician will openly admit to resorting to violence to preserve fiscal rents, and in places where such dynamics are most acute, prosecutions are rare because dominant families control much of the local administration, judiciary, and law enforcement. This is why most of the evidence must remain indirect, as in the quantitative analyses presented in the next section. Nevertheless, in rare, extreme cases—where violence is so egregious that it forces formal prosecutions—we can glimpse the underlying motivations. The 2009 Maguindanao massacre in the Philippines provides one such case.[20]

When Esmael "Toto" Mangudadatu announced his candidacy for governor in late 2009, he was perceived as a serious threat by the ruling Ampatuan clan. That perception manifested violently on 23 November 2009, when a convoy of his wife, relatives, supporters, and 32 journalists was ambushed and murdered on the way to file his candidacy papers. In total, 58 people were killed, making it the deadliest attack on journalists worldwide.[21] Court testimony later revealed that members of the Ampatuan family had convened to plan the ambush, with the explicit goal of preventing Mangudadatu from filing his candidacy.[22] Indeed, Mangudadatu ultimately won the 2010 gubernatorial election—in part because his principal rival, Andal Ampatuan Sr., was removed from the race after being implicated as a co-conspirator in the massacre—but the outcome nonetheless underscored how credible a challenger he was, and why the

[19] In line with this argument, Van Ham and Lindberg (2015) find that violence becomes more prevalent than vote-buying in weaker democracies.

[20] Maguindanao is a Filipino province situated in the Bangsamoro Autonomous Region in Muslim Mindanao (BARMM).

[21] *Aljazeera*. May 10, 2010. "Philippines elects massacre target."; *The Guardian*. December 19, 2010. "Philippines massacre: masterminds of country's worst political attack jailed."

[22] *Inquirer*. December 19, 2019. "Court acquits Sajid Ampatuan, brother-in-law in Maguindanao massacre case."

clan had perceived him as such a threat.[23]

Why such extreme violence? The answer lies in the stakes of political office. Control of the governorship meant control over budgetary resources, both domestic and foreign aid. In 2009, Maguindanao's share of national government transfers was about US $22.7 million. By comparison, foreign donors were channeling amounts of similar magnitude into the region. The World Bank's ARMM Social Fund Project disbursed about US $61 million between 2003 and 2010, with over US $30 million expected to be disbursed after the election in 2010. USAID's Growth with Equity in Mindanao program, running from 2008–2012, distributed nearly US $99 million. Even if only a quarter of these donor-financed projects reached Maguindanao, they represented tens of millions of dollars annually, on par with or exceeding the province's own domestic revenues. Crucially, much of the foreign assistance to Maguindanao during this period was concentrated in infrastructure projects, such as farm-to-market roads, schools, and electrification. These types of projects were politically valuable because they were both highly visible to voters and involved large financial outlays that could be diverted or inflated. Politicians claimed credit by inaugurating roads or schools with their name emblazoned on a signboard and plausibly claimed personal responsibility for delivering it, even when financing came from foreign donors. For instance, in October 2006, ARMM Governor Zaldy Ampatuan co-hosted a switching-on ceremony for the U.S.-funded "Alliance for Mindanao Off-Grid Renewable Energy" rural electrification program at his liaison office in Shariff Aguak, providing a highly visible opportunity to associate himself with the donor-funded initiative.[24]

Control over the governorship offered not only the opportunity to claim political credit but also access to large streams of money that could be siphoned off for personal gain. The Ampatuans systematically converted these resources into political and personal rents. Their capacity to extract rents was further reinforced by their alliance with President Gloria Macapagal Arroyo. In exchange for delivering implausibly lopsided election tallies the Ampatuans enjoyed protection from oversight and auditing.[25] Even international donors expressed concerns: the World Bank noted that during Zaldy Ampatuan's tenure as ARMM governor and Andal Sr.'s control of Maguindanao, a "confluence of poor governance, monopoly of power and high security risks made it difficult for the implementing agency to prevent misuse of funds and political interference in subproject selection" under the ARMM Social Fund.[26] Investigations by the Philippine Center for Investigative Journalism documented that under Andal Ampatuan Sr. and his sons,

[23] *GMA News Only*. May 14, 2010. "Mangudadatu wins gubernatorial race in Maguindanao."

[24] *Gulf News*. October 23, 2006. "180 villages get power in Mindanao."

[25] *The Guardian*. November 29, 2009. "Clan allied to Philippine president suspected of being behind massacre."

[26] *World Bank*. February 3, 2010. "Implementation Completion and Results Report: ARMM Social Fund Project." Report No: ICR0001065.

billions of pesos in public funds were misused or could not be accounted for, with one audit official describing the treasury as a "personal purse."[27] Datu Sajid Islam Ampatuan, who was governor at the time of the massacre, was later convicted in multiple cases of graft and malversation for "ghost projects" in 2009. Courts found that contracts for farm-to-market roads worth about US$8.4 million and emergency food supplies worth US$340,000 had been fabricated, leading to convictions, multi-decade prison sentences, and orders to repay over US$8.3 million.[28] The scale of diversion was vividly illustrated when, after the 2009 massacre, the government declared martial law in Maguindanao and raided Ampatuan properties. Authorities uncovered hidden arsenals, crates of high-powered weapons, luxury vehicles, wads of cash, and opulent mansions; assets grossly disproportionate to official salaries and strongly suggesting that public and donor funds had been converted into private wealth and coercive capacity.[29]

The Maguindanao case illustrates in unusually stark fashion the broader argument advanced here: when foreign aid and budgetary transfers significantly enlarge the fiscal and political value of office, incumbents have powerful incentives to deploy coercion to defend their hold on power. The aid projects in Maguindanao were not only visible and electorally salient, they also represented sums comparable to or exceeding the province's domestic revenues, creating opportunities for both political credit-claiming and personal enrichment. Coupled with national-level protection from oversight, this confluence of resources and impunity helps explain why the Ampatuan clan resorted to extreme violence when challenged. At the same time, our argument also suggests that such high stakes can motivate not only incumbents but also challengers to resort to violence in their efforts to dislodge entrenched elites and gain access to these rents. While the Maguindanao massacre highlights the extreme lengths incumbents may go to retain control, it also underscores the broader mechanism: when the value of office is magnified by aid flows, both those in power and those seeking it may find violence an attractive, if costly, strategy. We now turn to the systematic evidence: the research design, methods, and data used to test these claims across a broader set of contexts.

3 Method and Data

We examine the impact of foreign aid on targeted political violence by opposition and government politicians across subnational jurisdictions in up to 121 developing

[27] *Philippine Center for Investigative Journalism.* March 30, 2010. "Ampatuans managed public funds like clan's own purse."

[28] *Philippine News Agency.* May 8, 2023. "Ampatuan kin, Maguindanao exec guilty in 'ghost' food purchases."

[29] *Human Rights Watch.* November 2010. "They Own the People: The Ampatuans, State-Backed Militias, and Killings in the Southern Philippines."

countries that have received aid and held elections, over the 1990 to 2020 period.[30] Most analyses focus on subnational units at the second level (ADM2) of administrative regions, but we report results at the first level (ADM1) of administrative regions for comparison. Typically, ADM2 regions correspond to counties or districts, while ADM1 regions represent larger divisions such as provinces or states. Our study utilizes subnational boundaries from the Database of Global Administrative Areas 3.6 (GADM), encompassing almost 30,000 ADM2 regions and more than 2,300 ADM1 regions across the countries analyzed.[31] Our empirical strategy is based on the following regression model:

$$Political\ Violence_{it} = \beta_1 Aid_{it-1} + \beta_2 Population_{it-1} + \alpha_{ct} + \delta_i + \nu_{it}, \tag{1}$$

where α_{ct} denotes country-year fixed effects, δ_i captures ADM2-region fixed effects, and β_1 is the coefficient of interest measuring the impact of (the logarithm of) foreign aid on political violence.

Our dependent variable, $Political\ Violence_{it}$, is a binary indicator that equals 1 if at least one act of targeted political violence occurs in subnational region i in year t, and 0 otherwise.[32] We take our preferred indicator for targeted political violence from the Global Terrorism Database (GTD), which is an open-source database that includes information on violent events worldwide from 1970 through 2021.[33] Its key advantages are that it covers the period from 1990 to 2020 for which aid data are also available for all countries worldwide, and allows us to extract attacks on public officials broadly defined. This includes attacks on judges, public attorneys, courts and the court system, politicians, royalty, heads of state, government employees (unless they are police or military), and election-related attacks, among others.

GTD defines terror as "the threatened or actual use of illegal force and violence by a non-state actor to attain a political, economic, religious, or social goal through fear, coercion, or intimidation" (START, National Consortium for the Study of Terrorism and Responses to Terrorism 2021, 12). This definition is broadly applied, with the goal of maximizing the number of attacks included in the data. As a consequence, the data include all types of political violence that are outside of the context of legitimate warfare activities, as long as the attack is "aimed at attaining a political, economic, religious, or

[30]Specifically, we exclude countries that have not received any aid over the sample period and those country-year observations that V-Dem defines as closed autocracies and where neither regional nor local governments were selected via elections.

[31]The GADM database only provides ADM1-level boundaries for some countries. In such cases, we use ADM1 regions in our ADM2-level analysis.

[32]We prefer using a binary indicator rather than a count of attacks, as the vast majority of non-zero cases involve only a single attack. Relying on counts could cause our results to be overly influenced by a small number of sub-national regions with unusually high numbers of attacks. Our results, however, do not depend on this choice, as we show in the appendix in Table B2.

[33]GTD derives its data from a broad range of media outlets.

social goal" and there is "evidence of an intention to coerce, intimidate, or convey some other message to a larger audience (or audiences) than the immediate victims" (START, National Consortium for the Study of Terrorism and Responses to Terrorism 2021, 12) and is thus well suited as a measure of targeted political violence at large.[34]

While our main indicator of targeted political violence has clear advantages because it focuses more broadly on targeted political killings and coercion, using it also entails costs. One is that it only includes attacks on members of the official sector but excludes those on political candidates for office or members of political parties that do not hold elected office. Another is that it includes some groups that are not directly relevant to our theoretical argument, especially judges and public attorneys.[35] In order to guard us against these drawbacks and to add nuance, we complement our analysis with data from a number of sources. These include the Deadly Electoral Conflict Dataset (DECO), which tracks killings of candidates and party members; the Electoral Contention and Violence dataset (ECAV), which records attacks on campaign events and electoral personnel; and the Armed Conflict Location & Event Dataset (ACLED), including its Violence Targeting Local Officials dataset. These sources allow us to disaggregate violence across political affiliation (e.g., opposition vs. ruling parties), electoral roles (e.g., candidates vs. officeholders), and electoral phases (e.g., pre-electoral vs. postelectoral), with specific emphasis on local elections. We introduce the precise operationalizations of these indicators in the results section below.[36] Crucially, these datasets also enable placebo tests by examining types of violence that should not respond to aid flows if our theory is correct—for example, attacks against civilians unrelated to electoral competition, such as violence related to referendums. This helps confirm that our results are not driven by broader violence trends but reflect targeted electoral violence tied to foreign aid incentives.

Figure 1 presents the distribution of the number of years with observed targeted violence across ADM1 regions over the study period, using our GTD-based indicator of political violence. We choose the less fine-grained ADM1-regions because it is difficult to decipher entries as detailed as the level of ADM2 in a map of all countries worldwide. We however provide such a map for one continent (Africa) in Figure 2. For countries without ADM2 regions in the GADM dataset—such as Libya—we use ADM1 regions instead.

Our primary explanatory variable is Aid_{it-1}, which represents the logarithm of gross ODA disbursements (plus 1) to subnational region i in country c in year $t-1$, adjusted to constant 2014 US dollars.[37] We use Bomprezzi et al.'s (2025a) Geocoded Official

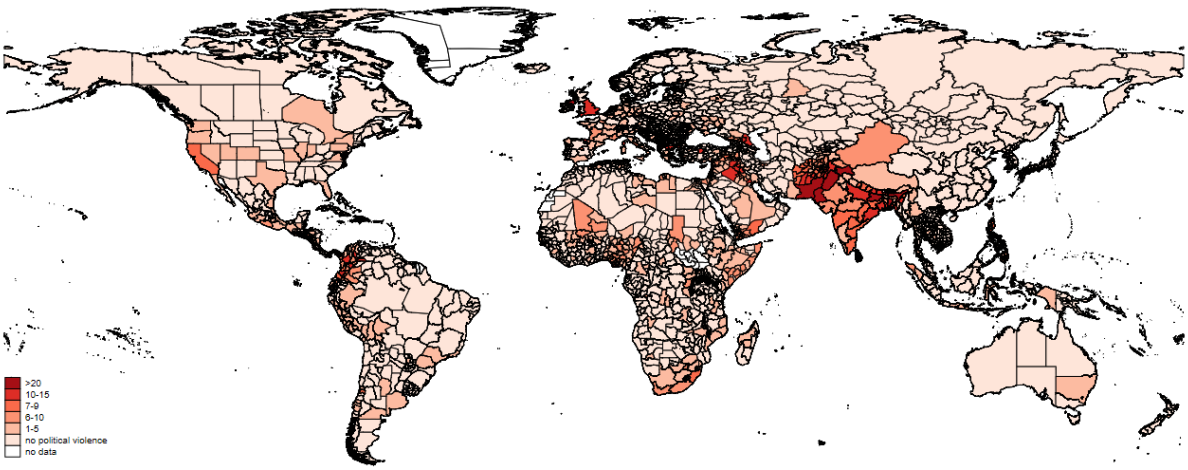
[34]Some scholars have rightfully raised concerns about potential reporting biases in events data (Eck 2012, von Borzyskowski and Wahman 2021). To address these concerns, we use country-year and region fixed effects in all our models and instrumental variables regressions as our preferred specification.

[35]In addition, GTD does not include data for the year 1993, as those have simply been lost.

[36]An alternative would be to code our dependent variable based on a combination of these (Gassebner et al. 2023). Given that the decision of which variables to combine would to some extent be arbitrary, we however prefer to stick with existing definitions and test robustness to using either of them.

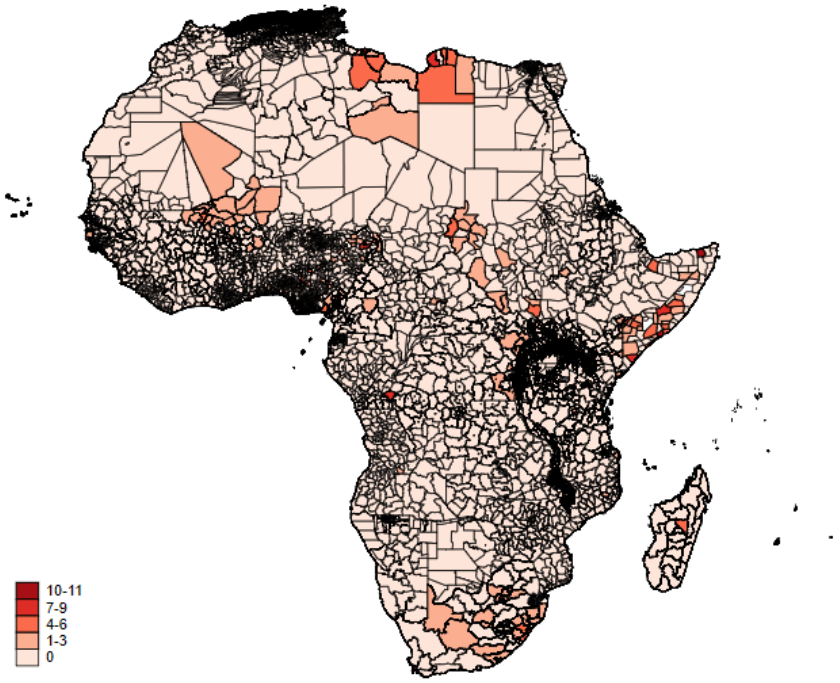
[37]We assume the effect of aid to be rather immediate in our main specifications, but Table B4 in the

Figure 1 – Number of years with targeted political violence in ADM1 regions, 1990–2020



n *Note:* The figure plots the number of years with targeted political violence in ADM1 regions, over the 1990-2020 period (GTD).

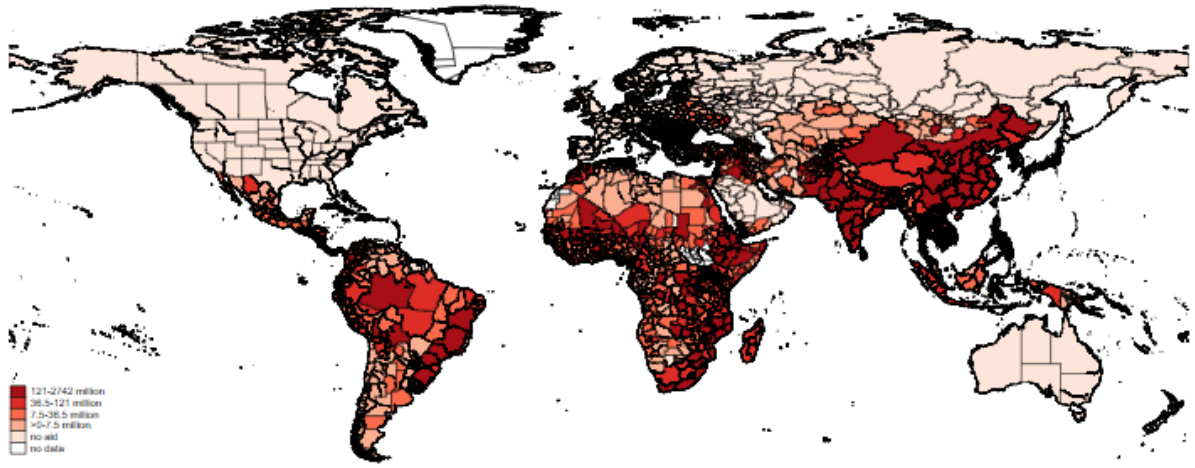
Figure 2 – Number of years with targeted political violence in ADM2 regions, Africa, 1990–2020



Note: The figure plots the number of years with targeted political violence in ADM2 regions in Africa, over the 1990-2020 period (GTD).

Development Dataset (GODAD), which includes aid disbursement information from 18 European donors and the United States. Figure 3 illustrates the allocation of aid from Western bilateral donors across ADM1 regions. As can be seen, in addition to variation in the amount of aid between countries, there is substantial variation between regions as well.[38]

Figure 3 – Aid in ADM1 regions, 1990–2020



Note: The figure plots the sum of aid received at the ADM1 level, over the 1990-2020 period (in million constant 2014 US dollars from Bomprezzi et al. 2025a).

Our baseline model is parsimonious and does not include many additional control variables, following standard practices in subnational aid effect studies (Dreher and Lohmann 2015, Dreher et al. 2021). Country-year fixed effects control for factors affecting all subnational regions within a country at any point in time, while region fixed effects account for geographic, institutional, and other time-invariant factors specific to a region. We control for $Population_{it-1}$, measured as the (logarithm of) population size of region i at time $t-1$, derived from high-resolution data on global population distribution provided by the Center for International Earth Science Information Network (CIESIN) (CIESIN 2018). Population is an important control variable included in most studies of conflict; we include it because aid flows more abundantly to more populated areas where conflict is also more likely. Despite keeping the main model parsimonious, we incorporate several control variables in our robustness checks to ensure robustness of our results. In particular, we control for the number of overall conflict events in the same region and year, in order to disentangle the effect of aid on targeted political violence from spurious correlations we may find due to omitted variables bias.[39]

appendix tests different timings.

[38] Figure A1 shows this map at the level of ADM2 regions, for Africa.

[39] Table B7 shows the countries that are included in our estimation sample, Table B8 shows definitions, and Table B9 reports summary statistics for all variables.

We estimate linear probability models rather than non-linear Probit or Logit models to facilitate the interpretation of results.[40] We cluster standard errors by country, allowing for arbitrary spatial and temporal correlation among all regions within a country.[41] Despite the use of lagged variables and fixed effects, there remains a concern that both aid disbursement and targeted political violence could be jointly influenced by unobserved, time-varying factors. To address this potential endogeneity, we employ instrumental variables regressions using the instrument proposed by Ahmed (2016) for the United States and generalized in Dreher and Langlotz (2020) for a broader set of donors. Their instrument exploits temporal variation in donor-country government fractionalization ($FRAC_{jt}$), which they interact with a time-invariant measure of the likelihood of receiving aid from a specific donor at the donor-recipient level. Intuitively, larger fractionalization increases demands on the government's budget, ultimately leading to larger ones.[42] This increase in the overall budget translates to larger development budgets as well (Fuchs et al. 2014). Dreher and Langlotz (2020) then show that more frequent recipients of aid receive larger chunks of this aid, on average.[43]

Following the strategy of Nunn and Qian (2014) and Ahmed (2016), Dreher and Langlotz (2020) introduce variation at the recipient country level by interacting donor government fractionalization with the share of years over the sample period during which a country received aid from specific donors. We adapt this strategy to the sub-national context and use the probability of a sub-national region to receive aid p_{ji}[44] to construct the instrumental variable:

$$IV_{it} = \sum_{j} FRAC_{jt} * p_{ji}. \tag{2}$$

We then estimate a first-stage regression at the recipient-region-year level as follows:

[40] Our use of instrumental variables further complicates the interpretation of results from non-linear models. Table B2 shows that our results do however not depend on this choice.

[41] This follows related work, such as Dreher et al. (2019) and Bluhm et al. (2025). Our results do however not depend on this choice, as we show in the appendix in Table B3.

[42] A substantial literature establishes the link between political fragmentation and government expenditures (e.g., Roubini and Sachs 1989 and Volkerink and De Haan 2001).

[43] Whether additional resources are given to traditional recipients or to new ones is an empirical question. Bilateral donors tend to give them to established clientele, while multilateral donors extend aid to less regular recipients (Lang 2021, Dreher et al. 2021).

[44] Specifically, a region's probability to receive aid from a particular donor j is $\bar{p_{ji}} = \frac{1}{31}\sum_{t=1990}^{2020} p_{jit}$, with p_{jit} indicating whether recipient region i received positive amounts of aid from donor j in year t. As Dreher and Langlotz (2020) explain, this approach is equivalent to using a "zero stage procedure" that runs a zero stage regression at the donor-recipient-year level where bilateral aid from all donors j to all recipient regions i is predicted with the instrumental variable $FRAC_{jt}$*p_{ji}. In contemporaneous work, Bomprezzi et al. (2025c) use the same instrumental variable to test whether aid affects development. Using the same instrument for aid with different dependent variables does not violate the exclusion restriction, but indicates that the effect of aid on political violence might arise as a consequence of its effect on development or the other way round.

$$Aid_{it} = \beta_1 IV_{it-1} + \beta_2 Population_{it} + \alpha_{ct} + \delta_i + \epsilon_{it}, \tag{3}$$

assuming that fractionalization affects disbursements with a lag of one year. We thus use donor government fractionalization in combination with regional likelihoods of receiving aid to instrument for aid disbursements at the subnational level.[45] We estimate the first stage based on all data available in order to increase the efficiency of the estimator and ensure the stability of the first-stage estimates across samples.[46] As Dreher and Langlotz (2020) explain, the exclusion restriction can be illustrated in the context of a difference-in-differences approach, leveraging variations in donor government fractionalization to estimate differential impacts of aid on regions with high versus low probabilities of receiving aid. Our identification thus hinges on the assumption that year-to-year changes in political violence do not systematically vary across regions based on their probability of receiving aid, apart from through changes in aid flows themselves. Given the lack of evident trends in donor fractionalization in our data and the absence of obvious confounders, this assumption appears plausible.

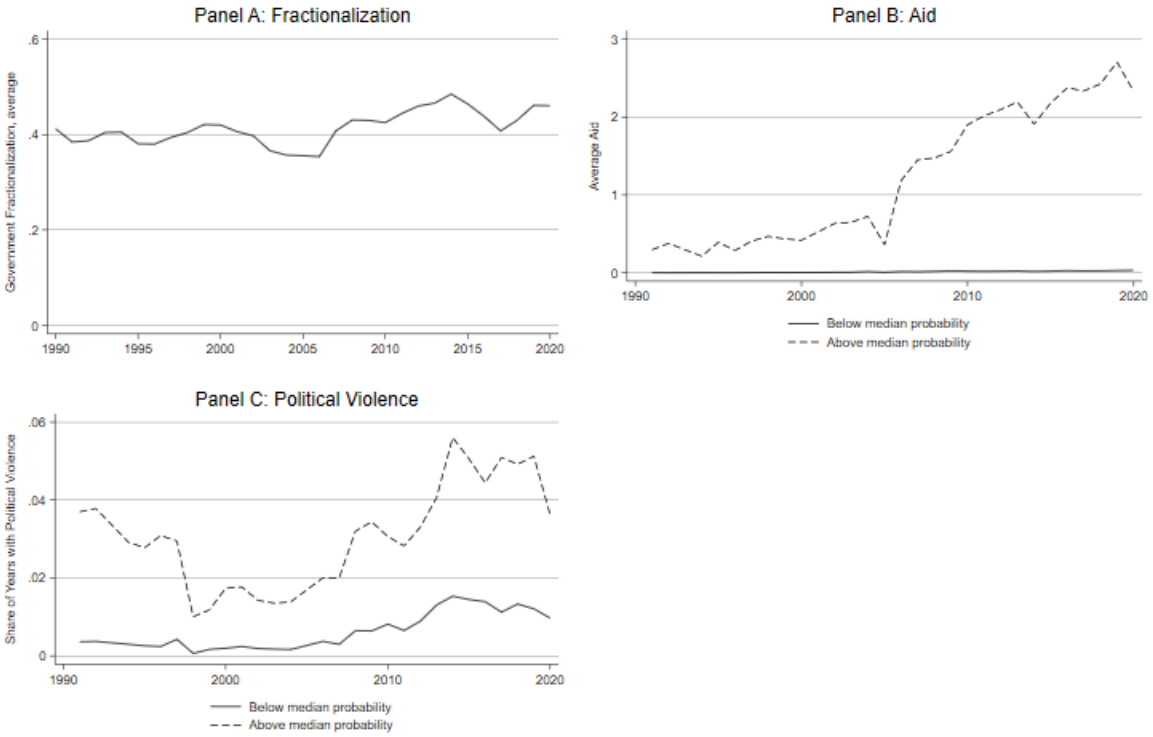
Following Christian and Barrett (2024), Figure 4 illustrates variations in government fractionalization in relation to average aid received and political violence across two groups. These groups are defined by the median probability of receiving aid. The results provide no evidence to suggest that the parallel-trends assumption is violated. Government fractionalization shows no discernible trends over time. While the probability-specific trends in aid diverge over time, the share of years with political violence appears largely parallel across regular recipients (those with an above-median probability of receiving aid) and irregular recipients (those with a below-median probability). Furthermore, there is no nonlinear trend in aid and the share of years with political violence that distinguishes regular recipients from irregular recipients (any linear trends are absorbed by the fixed effects).

The choice between using government or legislature fractionalization as part of the instrument is not immediately clear. As noted by Ahmed (2016) in the context of the United States, the "funding and allocation of bilateral economic aid involves both the executive branch and Congress" (p. 184), and this observation is applicable to other donor countries in our dataset. Since budget proposals are initiated by the government rather than the legislature, we measure donor fractionalization by the likelihood that two randomly selected members from the governing coalition belong to different parties (taken from Beck et al. 2001). However, this approach has a limitation: There is no variation

[45] Using a constructed instrument yields IV standard errors that are consistently estimated as long as the second-stage error term is not correlated with our donor-recipient-specific instrument ($FRAC_{jt}{}^{*}p_{ij}$) from the zero-stage regression (Wooldridge 2010).

[46] Our results do not depend on this choice, however, as we show in Table B2 in the appendix.

Figure 4 – Trends in Aid, Political Violence, and Fractionalization, 1990-2020



Note: Panel A shows donors' government fractionalization over time. Panel B shows average (log) aid receipts in ADM2 regions within the group that is below the median of the probability of receiving it and the group that is above the median. Panel C shows the share of years with political violence within these two groups over time. For the construction of the averages, we use observations from the sample of column 1 in Table 1.

in government fractionalization for the United States during the period under study. To address this, we substitute government fractionalization with legislature fractionalization there.[47]

We then estimate our second stages as:

$$Political\ Violence_{it} = \beta_1 Aid_{it-1} + \beta_2 Population_{it-1} + \alpha_{ct} + \delta_i + \hat{v}_{it} + \eta_{it}, \tag{4}$$

$$\begin{aligned} PoliticalViolence_{it} = \beta_1 Aid_{it-1} + \beta_2 (Aid_{it-1} \times INTER_{it-1}) \\ + \beta_3 Population_{it-1} + \alpha_{ct} + \delta_i + \hat{v}_{it} + \eta_{it}. \end{aligned} \tag{5}$$

In both equations, we include the residual from the first-stage regression in eq. (3), denoted as $\hat{\nu}_{it}$, to implement a Control Function Approach (CFA). When aid is not interacted with other variables—in eq. (4)—the CFA yields coefficient estimates identical to those obtained via standard two-stage least squares (2SLS). We prefer the CFA, however, because it allows for a direct test of the exogeneity of aid via the Durbin-Wu-Hausman test: under the assumption that the instrument satisfies the exclusion restriction, a statistically significant coefficient on the residual term indicates that the hypothesis of exogenous aid is rejected.

Because the second stage includes a predicted regressor (the residual from the first stage), standard errors differ slightly from those produced by 2SLS. To address this, we report bootstrapped standard errors that account for both stages of estimation in the robustness section. However, due to the computational burden of bootstrapping with ADM2-level and country-year fixed effects in a large sample, the main tables report conventional standard errors.

In eq. (5) we introduce interactions between aid and measures of institutional quality or other country-level characteristics.[48] We estimate this specification using the same CFA framework, which is more efficient than the standard 2SLS approach of interacting the instrument with the interacted variable to form a second instrument (Wooldridge 2015). Thus, we continue to address potential endogeneity by including the first-stage residual from eq. (3) (which continues to exclude the interaction).[49]

[47] It is not surprising that government fractionalization remains constant in the United States. While most donor countries in the Development Assistance Committee (DAC) have parliamentary systems with proportional representation, the United States has presidential elections. The United Kingdom and France also differ from other donors due to their lack of proportional representation. However, government fractionalization in both countries varies over time.

[48] The levels of these variables are absorbed by the country-year fixed effects.

[49] The gain in efficiency relies on an additional assumption: the endogeneity bias must be constant across levels of the interacted variables. In the robustness section, we show that our results do not hinge on this assumption.

4 Results

Table 1 shows our main results. We report results from OLS regression in column 1 and the results from the Control Function Approach in column 2. As can be seen, the (conditional) correlation between foreign aid and targeted political violence is low and imprecisely estimated. Note, however, that we can reject the hypothesis that aid is exogenous to targeted political violence at the regional level (as indicated by the p-value provided in column 2). Consequently, the coefficient from the OLS regression is biased and inconsistent, and we must rely on the IV regression for accurate interpretation of our results. The causal estimate of column 2 shows that the likelihood of targeted political violence increases with aid received in the previous year.[50] The first-stage F-statistic is 44.2, and thus clearly above the commonly cited rule-of-thumb value of 10.[51]

According to the coefficient, a one percent increase in aid increases the probability of conflict by 0.01 percentage points. The coefficient is substantially larger compared to the OLS results in Panel A, pointing to a downwards bias there. Given the standard deviation of logged aid (3.36), a one standard deviation increase corresponds to a 3.2 percentage point rise in the risk of political violence. This is large relative to the mean conflict probability in the sample (1%), implying that such increases in aid more than triple the baseline likelihood of violence. Given the mean of logged aid of 0.99—which corresponds to roughly US$ 2.69 million—a one standard deviation increase raises this to almost US$ 78 million. While an increase of around US$ 75 million represents more than a 28-fold rise over the mean, our sample includes 103 projects with disbursements exceeding US$ 75 million; almost 300 projects have volumes above US$ 50 million.[52]

As another way to illustrate the quantitative importance of aid, we estimate the CFA with a binary indicator for regions that receive aid in a year compared to when they do not. We find that the presence of an aid project increases the probability of political violence by 15.79 percentage points (see column 3). This suggests that large-scale aid inflows, relative to typical levels, not only significantly but also substantially heighten the risk of political violence.

According to our argument, targeted political violence should be particularly prevalent when governance is weak. To test this expectation, columns 4–6 interact aid with a broad indicator of political risk, taken from the PRS Group's International Country Risk Guide, ICRG, ranging from 0 to 100, with higher values indicating better governance (The PRS

[50] Note that violence decreases with population size, indicating that violence is more prevalent in remote, less densely populated places.

[51] Stock and Yogo (2005) propose more specific sets of critical values for weak identification tests based on the number of endogenous regressors, the number of instruments, and the acceptable maximum bias of the 2SLS relative to an OLS regression, or the maximum Wald test size distortion. For example, a 25% 2SLS size distortion of a 5% Wald test is associated with a critical value of 5.53 and a lower value of 4.42 for a 20% limited information maximum likelihood size distortion.

[52] Examples include US$ 269 million in budget support from France to the City of Cape Town in 2013 or a US$ 111 million public transportation project disbursed in 2016 in Lima, provided by Germany.

Group 2024). Column 4 shows results from OLS regression. Columns 5 and 6 estimate a Control Function Approach (CFA), measuring aid as disbursements and the binary indicator, respectively.

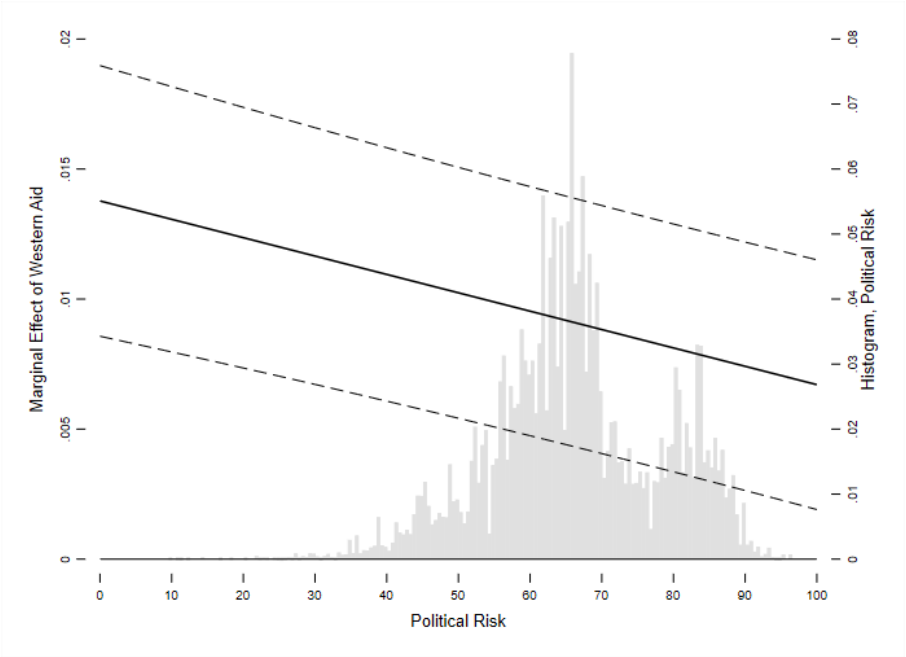
Our results show that aid increases political violence more strongly when the quality of governance is low. The interaction with political risk is significant at the one-percent level according to both estimates. However, the exogeneity of aid is again rejected, so we rely on the Control Function Approach for interpretation. To this end, we illustrate the marginal effect of aid estimated in column 5 over the range of the political risk score indicator, in Figure 5. The figure shows that aid increases political violence across all values of political risk. The effect is strongest when the political risk indicator is at its lowest (hypothetical) value of zero, where an increase in aid by one percent increases political violence by 0.014 percentage points. An improvement in the political risk score by 10 points reduces this effect by 0.001 percentage points. However, the effect stays positive even at the highest level of governance quality. These results hold when we measure aid with the binary indicator instead. According to column 6, aid increases conflict by 20.8 percentage points in years when political risk looms largest (i.e., the indicator of political risk is zero), but stays positive even when risk is low.

Table 1 – Aid and Targeted Political Violence, ADM2

	(1) OLS	(2) CFA	(3) CFA	(4) OLS	(5) CFA	(6) CFA
(log) Aid (t-1)	-0.00004	0.0094***	0.1579***	0.0044***	0.0138***	0.2080***
	(0.29)	(3.28)	(3.28)	(4.28)	(4.35)	(4.05)
Western aid*risk				-0.0001***	-0.0001***	-0.0007***
				(4.53)	(4.33)	(4.08)
(log) Population, t-1	-0.0007	-0.0031***	-0.0038***	-0.0005	-0.0028***	-0.0037***
	(1.17)	(3.40)	(3.56)	(0.61)	(2.79)	(3.10)
First year	1991	1991	1991	1991	1991	1991
Last year	2020	2020	2020	2020	2020	2020
Aid	disb.	disb.	dummy	disb.	disb.	dummy
Exogeneity (p-value)		0.00	0.00		0.00	0.00
Kleibergen-Paap F-stat		44.20	44.20		44.20	44.20
Number of countries	121	121	121	91	91	91
Number of regions	29308	29308	29308	27144	27144	27144
Number of observations	822468	822468	822468	756858	756858	756858

Note: The dependent variable is a binary indicator that is one for attacks on public officials (from GTD). Aid is (log) disbursements (plus 1) from 18 European donors and the U.S. in columns 1–2 and 4–5, and a binary indicator that is one if regions received any aid from those donors in columns 3 and 6; population is (log) population size. Columns 1 and 4 show OLS regressions. Columns 2–3 and 5–6 use the Control Function Approach, instrumenting aid with donor government fractionalization interacted with the recipient region's probability to receive aid. Risk is the political risk score, taken from the PRS Group's International Country Risk Guide, ICRG, ranging from 0 to 100, with higher values indicating better governance. All regressions include fixed effects for ADM2 regions and country-years. Standard errors are clustered at the country level.
t-statistics in parentheses; * p<0.10, ** p<0.05, *** p<0.01.

Figure 5 – Effects of Aid on Political Violence Conditional on Political Risk



Note: The figure plots the marginal effect and 90% confidence interval of (log) aid conditional on the value of political risk, corresponding to column 5 of Table 1. The histogram plots political risk, where higher values imply better governance.

In summary, these results are in line with our theoretical expectations: aid increases political violence, on average, but the effect is stronger when governance is weak, which suggests that they are more likely to use violence if they expect greater benefits from taking office and fewer constraints through, for example, domestic law enforcement. Notably, we also find a robust direct effect of aid on political violence that is independent of political risk. If politicians were meaningfully deterred by the threat of donor sanctions or reputational consequences, we would expect this average effect to vanish. That it persists even across contexts with stronger institutions suggests that political actors are either unconcerned with potential donor punishment or perceive such sanctions as unlikely or ineffectual.[53]

4.1 Facets of Targeted Violence

Whereas our main analysis focuses on broader measures of targeted political violence, to capture its various dimensions—i.e., the targeting of local government actors, opposition politicians, and voters—Table 2 provides a more fine-grained breakdown of political violence by categorizing different forms of electoral violence and their respective

[53]Recall that we exclude countries that have not received aid over the sample period or hold no elections. The country with the highest institutional score in our sample is therefore Chile, with a value of 81.75. Although this is beyond the scope of our paper, future research could explore the conditions under which the potential of external sanctions deter targeted political violence in these contexts.

measurement approaches. First, we capture general political violence against local officials using a dummy variable that indicates whether at least one violent event targeting local officials occurred in a given region and year (Column 1, data from ACLED). Second, we distinguish between different types of electoral violence based on whether state actors or non-state actors are involved and define electoral state-based violence when at least one actor in the conflict is a state government (Column 2, data from DECO). Third, we examine the role of incumbent political parties in electoral violence. We measure the occurrence of violence using events related to the incumbent party, regardless of whether they are the perpetrator or the target (Column 3, data from DECO), violent events where the incumbent party is the target, including incidents of clashes between groups of political supporters (Column 4, data from DECO), and violent events where the incumbent party is the perpetrator, including violence against opposition groups (Column 5, data from DECO). Fourth, we capture targeted electoral violence against politicians using violent events explicitly targeting political candidates or politicians (Column 6, data from DECO). Fifth, we investigate violence during subnational elections (Column 7, data from DECO). Sixth, we consider violence against party actors, specifically electoral violence targeting political party members, activists, or workers (Column 8, data from ECAV) and electoral violence targeting actors supporting the incumbent national government, including government officials, police, military personnel, election workers, and US-NATO Coalition Forces (Column 9, data from ECAV). Finally, we focus on violence targeting civilians: acts in which a formally organized group targets civilians (Column 10, data from DECO), violent events where an organized armed group inflicts violence upon unarmed non-combatants (from ACLED, column 11), and cases where an actor in power oppresses civilians (ICEWS, column 12, taken from Boschee et al. 2015).

Given that the exogeneity of aid is not rejected for all but one of the indicators—the one based on violence against civilians in column 11—Table 2 reports results from OLS regressions rather than the CFA. The results are overall consistent across indicators. In all regressions (shown in Panel A), political violence increases with aid, and most coefficients are estimated precisely. Exception are violence against local officials (column 1) and against party actors (column 8), where coefficients remain positive, but are estimated imprecisely. Most regressions also show that the effect of aid on violence turns weaker when governance improves (shown in Panel B): The interaction between aid and political risk is negative in all regressions and significant at least at the ten-percent level in eight of the 12 regressions. Overall, there is strong evidence in line with our theory. In short, across a broad range of different facets and indicators, aid increases political violence and the effect turns weaker with better governance.

Table 2 – Aid and Facets of Targeted Political Violence, OLS, ADM2

	(1)	(2)	(3)	(4)	(5)	(6)
	ACLED local officials	DECO state	DECO incumbent	DECO incumbent target	DECO incumbent perp	DECO politician
	Panel A: OLS					
(log) Aid (t-1)	0.0002	0.0001**	0.0001***	0.0001***	0.0001*	0.0001**
	(1.65)	(2.58)	(2.62)	(2.76)	(1.96)	(2.25)
Exogeneity (p-value)	0.41	0.25	0.26	0.46	0.52	0.58
Number of countries	102	121	121	121	121	121
Number of regions	26135	29308	29308	29308	29308	29308
Number of observations	226554	764520	764514	764514	764513	764520
	Panel B: OLS with risk interaction					
(log) Aid (t-1)	0.0008	0.0006**	0.0012**	0.0006*	0.0006	0.0005**
	(1.42)	(2.42)	(2.36)	(1.80)	(1.42)	(2.11)
Aid*risk	-0.0000	-0.0000**	-0.0000**	-0.0000*	-0.0000	-0.0000*
	(1.09)	(2.31)	(2.28)	(1.68)	(1.34)	(1.96)
Exogeneity (p-value)	0.41	0.30	0.34	0.61	0.44	0.63
Number of countries	82	91	91	91	91	91
Number of regions	24579	27144	27144	27144	27144	27144
Number of observations	208724	702001	701995	701995	701995	702001
First year	1997	1991	1991	1991	1991	1991
Last year	2020	2017	2017	2017	2017	2017

(continued)

Table 2 (continued)

	(7)	(8)	(9)	(10)	(11)	(12)
	DECO subnational	ECAV party	ECAV government	DECO one-sided	ACLED civilians	ICEWS coerce
	Panel A: OLS					
(log) Aid (t-1)	0.0001**	0.0001	0.0002**	0.0001**	0.0012***	0.0019***
	(2.20)	(1.22)	(2.22)	(2.48)	(3.98)	(6.63)
Exogeneity (p-value)	0.85	0.54	0.88	0.37	0.00	0.46
Number of countries	121	118	118	121	102	121
Number of regions	29308	29225	29225	29308	26135	29308
Number of observations	764520	621589	621589	764520	226554	742226
	Panel B: OLS with risk interaction					
(log) Aid (t-1)	0.0004	0.0018*	0.0016	0.0007**	0.0072***	0.0070**
	(1.28)	(1.94)	(1.49)	(2.00)	(2.90)	(2.26)
Aid*risk	-0.0000	-0.0000*	-0.0000	-0.0000*	-0.0001**	-0.0001*
	(1.10)	(1.92)	(1.41)	(1.91)	(2.61)	(1.71)
Exogeneity (p-value)	0.90	0.65	0.81	0.43	0.00	0.49
Number of countries	91	90	90	91	82	91
Number of regions	27144	27122	27122	27144	24579	27144
Number of observations	702001	571017	571017	702001	208724	683876
First year	1991	1991	1991	1991	1997	1995
Last year	2017	2012	2012	2017	2020	2020

Note: The dependent variable is a binary indicator that is one for attacks: on local officials (Column 1, data from ACLED), when at least one actor in the conflict is a state government (Column 2, data from DECO), or the incumbent party (Column 3, data from DECO), where the incumbent party is the target (Column 4, data from DECO), where the incumbent party is the perpetrator (Column 5, data from DECO), against political candidates or politicians (Column 6, data from DECO), during subnational elections (Column 7, data from DECO), targeting political party members, activists, or workers (Column 8, data from ECAV), targeting actors supporting the incumbent national government (Column 9, data from ECAV), involving electoral one-sided violence, in which a formally organized group targets civilians (Column 10, data from DECO), where an organized armed group inflicts violence upon unarmed non-combatants (column 11, data from ACLED), and where an actor in power oppresses civilians (column 12, data from ICEWS). Aid is (log) disbursements (plus 1) from 18 European donors and the U.S. Risk is the political risk score, taken from the PRS Group's International Country Risk Guide, ICRG, ranging from 0 to 100, with higher values indicating better governance. All regressions include ADM2 fixed effects, country-year fixed effects, and the logarithm of a region's population size. Standard errors are clustered at the country level.
t-statistics in parentheses; * p<0.10, ** p<0.05, *** p<0.01.

4.2 The Political Context of Targeted Violence

We now examine some additional implications of our argument, focusing on how the relationship between aid and political violence varies across political and institutional contexts.[54] Table 3 tests heterogeneity in a number of dimensions, interacting aid with indicators for elections, conflict, corruption, and democracy. We again estimate these models with the Control Function Approach, assuming that the extent of endogeneity of aid does not depend on these variables.[55]

Column 1 of Table 3 tests whether the effects of aid on political violence are stronger during elections. Whereas targeted political violence could be employed outside of elections, removing unwanted competition well before or after the elections, we expect the incentives to be particularly prevalent during election periods where candidates have more certainty about expected foreign aid resources and the level of political competition. To test this expectation, we include an interaction of *Aid* with a binary indicator for national (executive or legislative) elections in one or two years (using data from Beck et al. 2001). This indicator is not ideal for testing our theory, as we expect aid to primarily influence targeted violence against local politicians. However, due to the lack of data on local election dates, we use national election dates as proxy. Local and national elections often occur in concert. What is more, to the extent that aid fosters corruption within local political networks and these networks are also involved in nominating candidates for national office, we expect any aid-related violence to spill over into the context of national elections as well.[56]

Column 2 examines whether the effect of aid on targeted political violence is shaped by the broader conflict environment. One motivation for this test is to assess whether our findings are driven by localized aid dynamics or simply reflect underlying patterns of general violence. High levels of conflict may reflect deteriorating governance, weakened institutional oversight, and increased access to weapons; all of which could lower the costs of using violence for political ends. If this were the case, then the observed effect of aid might be conditional on pre-existing instability rather than reflecting a distinct mechanism linked to electoral competition. To evaluate this possibility, we interact lagged

[54] One may wonder whether the effect of aid on political violence should be conditioned on the degree of political competition—for example, the number of candidates in a race. While this is a theoretically plausible moderator, reliable subnational data on candidate entry is limited across the countries and years in our sample. What is more, measures of political competition typically capture the number of candidates at the time of the election, after violence may have already occurred to deter candidacies or eliminate rivals. This introduces endogeneity concerns and makes it difficult to interpret such measures as *ex ante* indicators of competition. In our context, targeted violence is precisely a strategy to shape the competitive landscape, so *post-hoc* measures may reflect the consequence rather than the condition of electoral coercion.

[55] Given that the hypothesis that aid is exogenous is rejected, we do not present results from OLS regressions to reduce clutter. The country-level variables forming the interaction are captured by the country-year fixed effects we include in all regressions.

[56] Recall that we also used an indicator of targeted violence during subnational elections as dependent variable in Table 2 (column 7), with similar results.

Table 3 – Aid and Targeted Political Violence, Interactions, CFA, ADM2

	(1)	(2)	(3)	(4)
(log) Aid (t-1)	0.00923***	0.00915***	0.01077***	0.00974***
	(3.20)	(3.23)	(3.48)	(3.43)
Aid*election year	0.00038**			
	(2.58)			
Conflict, number (t-1)		0.00047*		
		(1.81)		
Aid*conflict		-0.00001		
		(1.05)		
Aid*gov. stability			-0.00028***	
			(3.56)	
Aid*democracy				-0.00079*
				(1.69)
(log) Population, t-1	-0.00300***	-0.00307***	-0.00278***	-0.00305***
	(3.33)	(3.45)	(2.75)	(3.38)
First year	1991	1991	1991	1991
Last year	2020	2020	2020	2020
Kleibergen-Paap F-stat	44.20	44.20	44.20	44.20
Number of countries	120	121	91	121
Number of regions	29147	29308	27144	29308
Number of observations	820193	822468	756858	822450

Note: The dependent variable is a binary indicator that is one for attacks on public officials (from GTD). Aid is (log) disbursements (plus 1) from 18 European donors and the U.S.; population is (log) population size. The Control Function Approach (CFA) instruments aid with donor government fractionalization interacted with the recipient region's probability to receive aid. Column 1 interacts *Aid* with a binary indicator for national (executive or legislative) elections in one or two years (using data from Beck et al. 2001). Column 2 interacts with the number of conflict events at the ADM2 level (from UCDP, controlling for the level of it). Column 3 interacts with government stability, taken from the PRS Group's International Country Risk Guide, ICRG, with scales of 0–12, where higher values measure better governance. Column 4 interacts with V-Dem's binary indicator of electoral democracy (Coppedge et al. 2025). All regressions include fixed effects for ADM2 regions and country-years (which capture the level of the country-year-specific constituent terms of the interactions). Standard errors are clustered at the country level.
t-statistics in parentheses; * p<0.10, ** p<0.05, *** p<0.01.

aid with the total number of (lagged) violent events, capturing the intensity of general conflict in each region-year.[57]

To further assess whether foreign aid induces violence as a rational response to political constraints, we test whether the effect of aid is moderated by institutional features that shape the costs of political violence and the availability of alternative, non-coercive strategies for gaining power. In column 3, we assess whether the effect of aid is amplified in settings characterized by low government stability. When government stability is low, institutional constraints on the misuse of aid are weaker, enforcement of electoral rules is less credible, and the legal or reputational costs of violence are lower. Under such conditions, political actors may find coercive strategies more attractive than traditional patronage or persuasion. To evaluate this expectation, we interact lagged aid with the ICRG index of government stability, where higher values represent stronger governance safeguards.

In column 4, we examine whether regime type conditions the aid-violence relationship by interacting aid with the V-Dem indicator of electoral democracy (Coppedge et al. 2025), which ranges from zero (no electoral competition) to one (full electoral democracy). The logic here is similar: in more democratic contexts, political actors are more constrained by accountability mechanisms, public norms, and legal institutions, which raise the costs of resorting to violence. In less democratic settings, by contrast, where those constraints are weaker or absent, actors may face fewer obstacles to using violence as a tool for electoral advantage and access to aid-financed resources.

The results support our theoretical expectation that aid increases incentives for targeted political violence, particularly when political competition is heightened and institutional constraints are weak. Column 1 shows that a one-percent increase in aid leads to a 0.01 percentage point increase in violence when the election-indicator is zero. However, this effect intensifies prior to elections: the interaction term indicates an additional 0.0004 percentage point increase in violence, suggesting that political actors are more likely to resort to coercion when the opportunity to capture office—and with it, control over aid—is most immediate. Column 2 shows no significant interaction between aid and the overall level of conflict in a region, implying that it is not generalized violence but the electoral context that shapes the strategic use of coercion. Columns 3 and 4 provide further support for the argument that institutional weakness increases the attractiveness of violence as a tool for capturing aid. In less stable and democratic settings—where accountability is limited and the risks of punishment are lower—the effect

[57] We use data from the Uppsala Conflict Data Program (UCDP)(Sundberg and Melander 2013, Davies et al. 2024), version 24.1. UCDP defines a conflict event as "an incidence of the use of armed force by an organized actor against another organized actor, or against civilians, resulting in at least 1 direct death in either the best, low or high estimate categories at a specific location and for a specific temporal duration" (Sundberg and Melander 2013, 524). Given that we measure conflict at the regional level, the constituent term of the interaction is not captured by country-year fixed effects. We thus separately control for the level of sub-national conflict in the regression.

of aid on targeted political violence is stronger. While the interaction with democracy is only significant at the ten-percent level, the direction of the effect is consistent with our broader argument about the conditional nature of violence in aid-rich political environments.

Table 4 – Aid and Targeted Political Violence in Mexico, 2007–2012, CFA, ADM2

	(1)	(2)	(3)	(4)
(log) Aid (t-1)	0.22***	0.28***	0.01***	0.02***
	(2.76)	(3.24)	(4.83)	(5.47)
Aid*Prosecutor offices		-1.06		-0.05**
		(1.58)		(2.57)
Prosecutor offices	0.72	1.14	-0.02	-0.00
	(0.55)	(0.89)	(0.55)	(0.10)
Drug-related murder rate	0.27***	0.27***	0.06***	0.06***
	(3.80)	(3.79)	(8.85)	(8.84)
Fiscal revenue	0.03**	0.03**	0.00*	0.00*
	(2.17)	(2.18)	(1.87)	(1.82)
Mun. alternation	-0.04	-0.04	-0.00	-0.00
	(0.24)	(0.26)	(0.82)	(0.81)
St. alternation	-0.08	-0.08	0.00	0.00
	(0.39)	(0.37)	(0.17)	(0.10)
Mun. electoral competition	-0.48***	-0.49***	-0.01***	-0.01***
	(3.57)	(3.63)	(3.41)	(3.45)
St. electoral competition	0.73***	0.74***	0.02***	0.02***
	(2.60)	(2.64)	(2.76)	(2.81)
Method	NBREG	NBREG	OLS	OLS
Kleibergen-Paap F-stat	44.20	44.20	44.20	44.20
Number of regions	1753	1753	1753	1753
Number of observations	8789	8789	8789	8789

Note: The dependent variable is the number of criminal attacks against government officials, political candidates and party activists. Aid is (log) disbursements (plus 1) from 18 European donors and the United States. Prosecutor offices are the municipal number of public prosecutor offices per 1,000 population. The drug-related murder rate are battle deaths in state-cartel and inter-cartel conflicts per 1,000 population. Fiscal revenue is the percentage of the municipality's total income from local taxes. We also control for the municipal and state alternation of political parties in office, municipal and state electoral competition, and—not shown in the table—seven geographic regions. See Trejo and Ley (2021) for details and exact definitions. All regressions estimate a Control Function Approach (CFA) that instruments aid with donor government fractionalization interacted with the recipient region's probability to receive aid. Estimation is with with Negative Binomial Regressions in columns 1–2 and OLS in columns 3–4, both with random effects, as in Trejo and Ley (2021).
t-statistics in parentheses; * p<0.10, ** p<0.05, *** p<0.01.

4.3 Aid and Targeted Violence in Mexico

We conclude this section with a focused study of Mexico, which offers additional analytical leverage for evaluating our argument within a single-country context. Mexico receives a significant amount of aid from Western donors. Aid to Mexico peaks at more than US$ 82 million in the year 2018; in 2020, the largest donor was the U.S., followed by Germany, Spain, France, and the United Kingdom. Drawing on data from Trejo and

Ley (2021), this analysis provides a more specific operationalization of targeted political violence, capturing criminal attacks—including kidnappings, death threats, assassination attempts, and killings—against political actors such as local candidates, party activists, and elected officials. Importantly, this measure excludes public officials not directly involved in electoral competition, such as members of the judiciary or security services, who are included in the broader GTD-based coding of political violence in our cross-national dataset. At the same time, it includes important political actors who are not included in the GTD-based coding such as political candidates seeking office. The fact that 83% of all incidents in the Mexico data are directed toward local politicians aligns closely with our theoretical emphasis on local incentives tied to aid allocation.[58]

Although our instrumental variable strategy mitigates concerns about omitted variable bias, this case study allows us to probe the plausibility of common alternative explanations, particularly those related to drug-related violence and fiscal revenues. The fact that aid remains a robust predictor of political violence even when controlling for these covariates is reassuring—especially given how closely fiscal revenues and foreign aid resemble one another as sources of discretionary, territorially anchored finance. Finally, the Mexico analysis allows us to test our argument about the role of institutional quality in shaping the violence-inducing effects of aid with a subnational measure of state institutional presence. Specifically, we interact aid with the number of public prosecutor offices per 1,000 residents. This indicator offers a more localized and behaviorally grounded proxy for enforcement capacity than the broader national-level measures of political risk used in the cross-country analysis.[59]

We merged Trejo and Ley's (2021) replication data with our ADM2-level dataset, resulting in a sample of 1,753 municipalities. Column 1 of Table 4 replicates their core

[58] A potential concern is that the Mexico data only include political assassinations attributed to criminal organizations, which could bias our analysis by omitting political attacks committed independently of cartels. However, existing evidence strongly suggests that most targeted political killings in Mexico, especially at the local level, are carried out by organized crime groups acting on behalf of politicians rather than independently. In many developing country contexts, politicians rely on criminal organizations to supply the coercive "muscle" they lack through state institutions, subcontracting violence to cartels or militias to eliminate rivals and intimidate voters (Eaton 2006, Gallego 2018). Trejo and Ley (2020) document numerous cases of candidates forming pre-electoral alliances with cartels to intimidate or eliminate rivals, and note that such collusion is a widespread and strategic feature of local electoral competition. Albarracín (2018) similarly shows, in the Brazilian context, how elected mayors subcontract criminal groups to violently suppress opposition. According to data compiled by Gutiérrez-Romero and Iturbe (2024), 82% of political killings in Mexico are committed using high-caliber weapons typically associated with cartels, while lone-wolf attacks are rare (4%), further underscoring that most such violence is not politically autonomous but intertwined with criminal enforcement mechanisms.

[59] The number of public prosecutor offices may not always reflect impartial or effective law enforcement, particularly in settings where prosecutors are themselves entangled in political networks or complicit in violence. We therefore interpret this variable not as a direct measure of prosecutorial behavior, but as a proxy for the formal presence of state legal infrastructure at the local level. While imperfect, it complements broader national indicators of political risk by providing spatially disaggregated variation in institutional presence. Importantly, our results show that the presence of these offices is significantly associated with lower levels of political violence, which suggests that, on average, they still serve as a deterrent to targeted attacks, even in environments where institutional integrity may be uneven.

model, augmented to include our measure of aid and an adjustment for endogeneity using the residual from the first-stage regression, again applying a Control Function Approach.[60] Column 2 adds an interaction between aid and Trejo and Ley's measure of subnational institutional capacity—the number of municipal public prosecutor offices per 1,000 residents—to assess whether stronger local institutions mitigate the violence-inducing effects of aid. The results suggest that the effect of aid on political violence diminishes in municipalities with a stronger institutional presence. Columns 3 and 4 estimate the regressions with a linear model instead to facilitate the interpretation of results.

Despite the exclusion of nearly 250 municipalities due to merging constraints and the inclusion of aid as explanatory variable, the results closely mirror those reported in the original study and offer compelling support for our broader theoretical argument. The direct effect of aid on targeted political violence is positive, statistically significant, and consistent in magnitude with our cross-national findings. This reinforces the claim that foreign aid can create localized incentives for political coercion, especially in environments where the state has a weak or contested presence. The interaction between aid and subnational institutional capacity, measured by the number of public prosecutor offices per 1,000 residents, shows that institutional presence significantly moderates this relationship. Specifically, increases in local institutional infrastructure reduce the marginal effect of aid on political violence, suggesting that even in violent and competitive contexts like Mexico, stronger enforcement capacity can deter the strategic use of violence to capture aid-linked resources.[61]

In comparison, drug-related violence and fiscal revenue both exhibit positive and statistically significant effects on political violence, but the inclusion of these covariates does not attenuate the effect of aid.[62] This suggests that while illicit markets and local fiscal rents also drive political contestation and coercion, the impact of foreign aid is not simply a proxy for broader resource availability. Instead, aid appears to constitute a distinct and politically salient resource; one that becomes more violently contested in

[60]Following Trejo and Ley (2021), we include the following control variables: municipal public prosecutor offices per 1,000 population, drug-related violence (measured as battle deaths in state–cartel and inter-cartel conflicts per 1,000 population), fiscal revenue (the percentage of municipal income from local taxes), party alternation at the municipal and state level, electoral competition (measured by the effective number of parties), and binary indicators for seven geographic regions. See Trejo and Ley (2021) for exact definitions and coding of variables.

[61]The coefficient of the interaction effect is estimated less precisely in the negative binomial specification in Column 2, just failing to be significant at the 10%-level.

[62]When we calculate incidence rate ratios (IRRs) to facilitate interpretation (not shown in the table), our results in column 1 show that a 1-unit increase in the log of aid disbursements (i.e., approximately a 2.72-fold increase in aid) increases targeted political violence by 25.1 percent (IRR = 1.251). This compares to an increase in violence by 3.1 percent for every additional percentage point of a municipality's income from local tax revenues (IRR = 1.031). According to the linear model of column 3, a ten percent rise in aid increases the number of attacks by 0.00132, compared with an increase by 0.0009 for an increase in fiscal revenue of one percentage point (and a mean number of attacks of 0.07).

the absence of credible institutional checks. Taken together, these results lend further support to our theoretical argument.

5 Robustness Tests

This section tests the robustness of our key results. We report permutations of our main regressions shown in Table 1, including and excluding the interaction with political risk.

Operationalization of Targeted Violence: Table B1 tests the robustness of our key results to different choices of how we measure the occurrence of political violence. We have chosen our main indicator of political violence based on conceptual fit and data availability in terms of number of countries and years covered in the underlying dataset. However, a number of alternative indicators are available: Column 1 defines the binary indicator for political violence based on electoral state-based violent events that involve the government of the state. Our source for this indicator is the Deadly Electoral Conflict Dataset (DECO), which provides information on violent electoral events worldwide. DECO constructs its data using the geocoded event database of the Uppsala Conflict Data Program (UCDP-GED), with each event assessed individually to identify instances of violent electoral strife. Its focus on electoral motives neatly fits our purpose; however, the dataset ends in 2017 and thus covers a shorter period compared to GTD. Column 2 instead relies on violent events that are targeted at state actors, including national government and local government, as defined by the Electoral Contention and Violence (ECAV) dataset. ECAV contains information on both nonviolent and violent attacks, specifically focusing on contestation and violence linked to national elections. The data do not however cover events related to sub-national elections and end in the year 2012, thus further reducing the range of our sample. Column 3 defines violence based on the Armed Conflict Location & Event Data's (ACLED) "political violence" indicator. ACLED collects conflict events in every country and territory worldwide, relying on broad coverage of political violence and demonstrations without a minimum fatality requirement. It includes a wide variety of conflict types, such as protests and riots. However, no data exist before 1997 and data collection outside of Africa started more recently, resulting in a dramatic loss of observations and an imbalance of observations included, overrepresenting Africa.

Panel A of Table B1 reports results from OLS regressions; Panels B and C estimate a Control Function Approach, excluding and including the interaction of aid and risk, respectively. According to the results, aid consistently steers political violence. Across the various definitions, aid is significantly correlated with political violence in the OLS regressions. Panel B shows that the hypothesis of exogenous aid is rejected for just one of the regressions, those based on ACLED in column 3. Using this definition, aid increases violence according to the instrumental variables regressions in Panels B and C (as well as

according to the OLS regression in Panel A). While this is not the case according to the two alternative definitions, we rely on the more efficient OLS regressions for interpretation there given that the exogeneity of aid is not rejected in these regressions. The OLS regressions of Panel A show that a one-percent increase in aid increases the probability of electoral state-based violent events involving the government of the state by 0.0001 percentage points (column 1), and the probability of electoral violent events that are targeted at state actors by 0.0002 percentage points (column 2). According to Panel B of column 3 (where the hypothesis of exogenous aid is rejected), the corresponding increase in the probability of political violence (as measured by ACLED) is 0.026 percentage points. Panel C reports results including the interaction of aid and political risk. The coefficient is negative in all columns, but imprecisely estimated in column 2 (where the hypothesis of exogenous aid is not rejected, however) and just fails to be significant at the ten-percent level in column 3.[63] Overall, while we prefer the GTD-based indicator for interpretation due to its better coverage and resulting representativeness of the estimates, our key results do not depend on this choice.

Control Variables and Sub-Samples: Table B2 tests the robustness of our results in a number of additional dimensions, focusing on the instrumental variables regressions given that the hypothesis of exogenous aid is rejected for our main definition of targeted violence. Column 1 controls for the total number of conflict events in the same region and year.[64] This is potentially important as—to the extent that overall conflict and political violence are correlated—our estimate for the effect of aid on political violence might simply capture its effect on conflict more broadly. In column 2, we test whether our results depend on the inclusion of other control variables. Papers investigating determinants of violence at the level of countries typically control for indicators of democracy, election observation, corruption, per capita GDP, electoral systems, oil rents, intergovernmental transfers, criminal activity, and electoral competition. Some of these variables do not vary substantially between regions at any point in time and are thus covered by the fixed effects. Other data are not available sub-nationally. Given that the exclusion restriction for our instrumental variable discussed above does not depend on us controlling for any of these additional variables this does not threaten identification in our setting. We nevertheless include those variables that are available to us at the level of ADM2 regions: Control of corruption as a measure of institutional quality, (log) nightlights and the Human Development Index as measures of development, and the number of oil extractions sites to proxy for the availability of resource rents.[65]

[63]Note that the test for exogeneity is borderline here. When we estimate the regression with OLS, the coefficient is significant at the ten-percent level.

[64]We again draw from UCDP, as in column 2 of Table 3 above.

[65]We take these variables from Bomprezzi et al. (2025c). Corruption is defined as "the abuse of entrusted power for private gain," originally provided in Crombach and Smits (2024), who extract these data from a number of sources. They measure it on a 0–100 scale, with higher values indicating less corruption. Nightlight measures the logarithm of average nightlight emissions in an ADM2 region and

Columns 3–5 report results for Africa, the Americas, and Asia. Column 6 replaces the dependent variable with the number of attacks in a year rather than using a binary indicator. In column 7 we estimate 2SLS with the first stage being restricted to the sample available in the second stage rather than including all observations; we use the interaction of our donor government fractionalization-based instrumental variable with the political risk score indicator as second instrument in Panel B rather than estimating a Control Function Approach. Column 8 reports results from Conditional Fixed Effects Logit estimation.[66] Finally, column 9 reports results for our main regressions at the more aggregate level of ADM1 regions, where we use the count of conflict events rather than a binary indicator because in these larger regions at least one conflict happens comparably often.

As can be seen in Table B2, our results are robust to these permutations. They do not depend on the inclusion of control variables.[67] Neither do they depend on using the number of attacks rather than a binary indicator as dependent variable. They hold for Africa and Asia. When we restrict the sample to the Americas, the point estimates are very similar, but coefficients are less precisely estimated. Rather than showing that results are different for the Americas, we thus see them as a consequence of insufficient statistical power. They equally hold when we estimate with the alternative instrumental variables approach and when we estimate with Logit rather than a linear probability model. They also hold at the level of ADM1 regions.[68]

Clustering of Standard Errors: We next test whether our results depend on how we cluster standard errors. Table B3 shows that clustering at the level of ADM1 regions, ADM2 regions or—using two-way clustering—at the (i) country and year, (ii) ADM1-

year. We add 0.01 before taking logs in order to not lose zero observations. It is the sum of emissions of all pixels in a region weighted by the fraction of each cell that falls within a specific polygon, taken from Li et al. (2020), who combine data from the Defense Meteorological Satellite Program (DMSP) and DMSP converted Visible Infrared Imaging Radiometer Suite (VIIRS). Sherman et al. (2023) provide the Human Development Index at the 0.1 × 0.1 degree resolution for the years 2012 to 2022, which Bomprezzi et al. (2025c) aggregated to the ADM2 level. They expanded the data back to 1990 by applying the coefficient of change from one year to a previous year from the ADM1 time-series to ADM2 regions. We take the number of oil extraction sites from Denly et al.'s (2022) Global Resources Datset, assuming that data are complete for major sites and thus setting missing data to zero.

[66]We have netted out country-year fixed effects from all variables before running the Logit model with conditional fixed effects for ADM2 regions, which addresses the incidental parameter problem.

[67]While the correlation between conflict and political violence is positive and significant at the five-percent level (not shown in the table), our results for how aid affects political violence remain unchanged when we control for overall conflict. The positive correlation between aid and conflict is in line with the analysis in Bomprezzi et al. (2025b). Also see Chen (2025). Aja-Eke and Brazys (2024) instead find that aid reduces the likelihood that conflict relapses.

[68]At this more aggregated level of analysis, our instrumental variable however lacks power. We therefore replace government fractionalization as part of our interacted instrument with donor governments' aid budgets, which more directly measure available resources to be allocated to recipient regions in a particular year. The instrument is nevertheless comparably weak, the aid variable estimated imprecisely, and the hypothesis of exogenous aid not rejected. When we estimate these regressions with OLS instead (which we do not show in the table) the aid variable and the interaction with political risk are significant and in line with our theory.

level and year, or (iii) ADM1-level and country-year does not change standard errors substantially. The same holds true if we bootstrap standard errors to explicitly take account of the use of a generated regressor in our Control Function Approaches (with 1,000 repetitions).

Timing of Aid Disbursements: Table B4 investigates the timing between when aid is given and in which year it affects conflict. Panel A reports an event time specification, where aid enters with different lags. We include aid given one year *before* we measure violence to test potential pre-trends. We then include aid contemporaneously, and with lags of one, two, and three years. We show results for two regressions, using the Control Function Approach, excluding and including the interaction of aid and political risk (in columns 1 and 2, respectively). We continue to control for the endogeneity of aid lagged by one year, as in previous specifications.[69] In Panel B, aid is instead a moving average over three years, from t–3 to t–1, which we instrument with the moving average of the interacted instrument lagged by one year (i.e., from year t–4 to t–2). We again show one regression with just aid and one with the aid-political risk interaction included.

According to the results of Panel A, aid increases violence already in the year of disbursement. The estimates however show a substantial increase in the coefficient of aid one year after disbursements compared to a small effect of contemporaneous aid. Aidxr given in the year before we measure violence is insignificant, indicating the absence of a significant pre-trend; the coefficients of aid lagged by two and three years are also estimated imprecisely. When we turn to the moving average regressions of Panel B, results are very similar to the baseline regressions reported above (though the first-stage F-statistic is weaker).

Placebo Tests: We also report results from placebo regressions, testing the effect of aid on violence where our theoretical expectations are unlikely to be met. We test whether aid affects violent events related to a (national or subnational) referendum. Arguably, violence related to a referendum should not be affected by aid via the mechanisms we hypothesize. Table B5 shows the results. As can be seen in Panel A, aid does not increase the probability of referendum-related violence, on average, and this holds when we add the interaction between aid and political risk (in Panel B). We take this as additional evidence that the results we report throughout this paper measure the effects of aid on violence against politicians rather than simply proxying for broader dimensions of conflict.

Aid Sectors: Finally, we disaggregate the effects of aid by sector to examine whether the observed relationship between aid and political violence varies across types of assistance. Specifically, we analyze aid allocated to the three main sectors defined by

[69] We would ideally like to instrument all aid variables but do not have instrumental variables to do so. Our fractionalization-based instrument is arguably not excludable to different timings of aid, given that fractionalization in one year affects aid allocations in various years and not exclusively one year later.

the OECD: economic infrastructure, social infrastructure, and production.[70] To account for sector-specific variation, we adapt our instrumental variables strategy and use sectoral aid probabilities to construct corresponding instruments, allowing us to isolate the effects of aid in each domain while preserving the identification strategy.

The results of these sector-level analyses are presented in Table B6. Panel A reports the baseline estimates of the effect of aid on political violence for each sector, while Panel B includes the interaction between aid and political risk. The instruments for aid in all three major sectors—economic infrastructure, social infrastructure, and production—are strong, and the estimated effects mirror those obtained for aggregate aid. These results suggest that the underlying mechanisms linking aid to targeted political violence are not specific to particular types of development assistance, but reflect a broader logic of political competition over aid-financed resources.[71]

In summary, our results are robust to a wide range of alternative specifications, including the inclusion of additional control variables, regional subsamples, and alternative estimation strategies. They remain consistent when varying the timing of aid disbursements, with the strongest effects observed one year after aid is received. Standard error clustering at various levels and moving average specifications of aid also do not alter our conclusions.

6 Conclusion

This paper examines the unintended consequences of foreign aid on targeted political violence, arguing that aid can serve as a powerful incentive for political actors to engage in targeted political violence—the strategic use of violence to manipulate electoral competition and secure access to state resources. While existing research has largely

[70]The OECD's Development Assistance Committee defines these sectors as follows: Social Infrastructure & Services includes Education, Health, Population Policies/Programs & Reproductive Health, Water Supply & Sanitation, Government & Civil Society, and Other Social Infrastructure & Services. Economic Infrastructure & Services includes Transport & Storage, Communications, Energy, Banking & Financial Services, and Business & Other Services. The Production Sector includes Agriculture, Forestry, Fishing, Industry, Mining, Construction, Trade Policies & Regulations, and Tourism.

[71]We have also investigated donors individually, using each donor's government fractionalization interacted with the recipient region's probability of receiving aid from that donor as instrumental variable, in line with the strategy introduced in Ahmed (2016); however, the instrumental variable is not sufficiently strong in all regressions. We find that aid from seven donors is associated with an increase in political violence: Denmark, Germany, Ireland, Italy, Spain, the UK, and the United States. When we include the interaction between aid and political risk, all estimated coefficients have the expected negative sign, and twelve are statistically significant, indicating that the effect of aid on political violence intensifies under conditions of higher political risk. Our results also broadly hold for aid from the International Development Association (where we instrument aid relying on the World Bank's measure of IDA's funding position interacted with the recipients' probability to receive aid, following Dreher et al. 2021), with a positive though imprecisely estimated average effect, but significant conditional effect. Given the lack of a theory that can explain different results across donors we do not show these results in a table, and instead leave them for future research.

focused on how incumbents use aid to bolster electoral support through clientelism and public goods provision, we highlight a more coercive dynamic: the assassination of political opponents, intimidation of candidates, and coercion of voters to influence election outcomes. Using geo-coded aid disbursement data from 18 European donors and the United States (1990–2020) and employing an instrumental variables approach, our findings indicate that foreign aid is associated with increased levels of targeted political violence against local politicians and electoral actors, particularly prior to elections and in weakly institutionalized environments. These results underscore how foreign aid, rather than fostering political stability, can heighten the stakes of electoral competition and encourage violent strategies to secure power.

Our study makes several theoretical and empirical contributions. First, we extend the literature on the political effects of foreign aid by demonstrating that aid does not only sustain incumbents through traditional clientelist strategies but also fuels electoral violence as a means of capturing government resources. While previous research has examined the role of aid in patronage politics and elite co-optation, we provide the first systematic argument and empirical evidence that aid can also incentivize targeted violence as a political strategy. Second, we contribute to the broader literature on electoral violence by identifying targeted political violence as a distinct mechanism of political contestation, separate from civil conflict or mass unrest. Unlike broad forms of political violence, targeted political violence is characterized by deliberate, strategic targeting of political rivals, electoral candidates, and local officials, and is directly linked to the political economy of aid distribution. Third, methodologically, our study offers a comprehensive subnational analysis, incorporating multiple aid donors and electoral violence datasets to provide a fine-grained understanding of how aid shapes localized patterns of political violence.

Future research could explore additional dimensions of how foreign aid interacts with political violence beyond the electoral cycle. While this paper has focused on targeted attacks against political rivals for electoral gain, aid may also influence patterns of repression, intimidation, and coercion in non-electoral contexts, including efforts to suppress dissent, constrain civil society, or entrench authoritarian rule. A promising avenue for future work involves examining how different donors or aid modalities—such as general budget support versus project-based assistance—shape the political incentives for violence, and whether some forms of aid are more prone to misuse than others. Another important extension would be to evaluate the downstream consequences of aid-fueled political violence. Does such violence undermine democratic consolidation, reduce government legitimacy, or shift patterns of voter behavior? Additionally, while this paper has focused on recipient incentives, more work is needed on donor responses. Under what conditions do donors sanction, ignore, or accommodate violence linked to aid distribution? Understanding whether and how donors react to such misuse would not only deepen our

understanding of the political economy of aid but also clarify the broader implications for democratic stability and governance quality in recipient countries.

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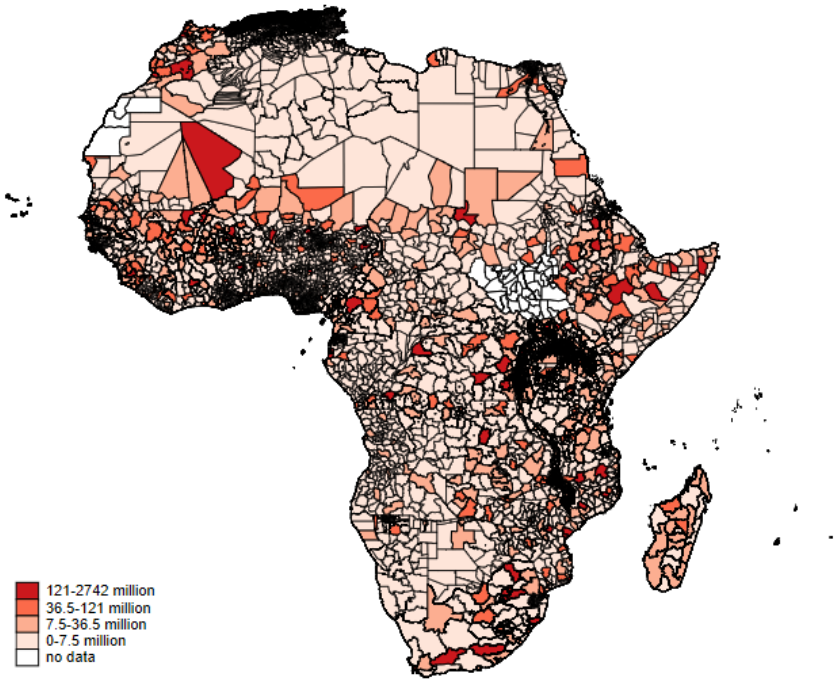
Appendix

A Additional Figure

Figure A1 – Aid in ADM2 regions, 1990–2020



Note: The figure plots the sum of aid received at the ADM2 level in Africa, over the 1990-2020 period (in million constant 2014 US dollars from Bomprezzi et al. 2025a).

B Robustness

Table B1 – Aid and Targeted Political Violence, ADM2, Different Definitions

	(1) DECO	(2) ECAV	(3) ACLED
	Panel A: OLS		
(log) Aid (t-1)	0.0001**	0.0002**	0.0011***
	(2.58)	(2.40)	(3.16)
	Panel B: CFA		
(log) Aid (t-1)	0.0008	-0.0012	0.0259***
	(1.26)	(0.53)	(2.71)
Number of countries	121	118	102
Number of regions	29308	29225	26135
Number of observations	764520	621589	226554
Exogeneity (p-value)	0.25	0.54	0.01
	Panel C: CFA		
(log) Aid (t-1)	0.0012	-0.0008	0.0304***
	(1.57)	(0.31)	(2.75)
Aid*risk	-0.0000**	-0.0000	-0.0001
	(2.07)	(0.82)	(1.65)
Number of countries	91	90	82
Number of regions	27144	27122	24579
Number of observations	702001	571017	208724
Exogeneity (p-value)	0.04	0.41	0.10
First year	1991	1991	1997
Last year	2017	2012	2020
Kleibergen-Paap F-stat	44.20	44.20	44.20

Note: We report our regressions shown in columns 1, 2 and 4 of Table 1—our main OLS and IV regressions in concert with the interaction with political risk—using different definitions of political violence. The dependent variable is a binary indicator that is one for attacks involving the state for electoral reasons (DECO, column 1), including national government and local government for electoral reasons (ECAV, column 2), and involving political violence (ACLED, column 3). Aid is (log) disbursements (plus 1) from 18 European donors and the U.S. Panel A shows OLS regressions. Panels B and C estimate a CFA, instrumenting aid with donor government fractionalization interacted with the recipient region's probability to receive aid. Risk is the political risk score, taken from the PRS Group's International Country Risk Guide, ICRG, ranging from 0 to 100, with higher values indicating better governance. All regressions include country-year fixed effects, ADM2 fixed effects, and the logarithm of a region's population size.
t-statistics in parentheses; * p<0.10, ** p<0.05, *** p<0.01.

Table B2 – Aid and Targeted Political Violence, ADM2, Tests for Robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Conflict	Controls	Africa	Americas	Asia	Count	2SLS	Logit	ADM1
	Panel A: CFA/2SLS								
(log) Aid (t-1)	0.009***	0.006**	0.007***	0.008	0.014*	0.049*	0.009***	0.251***	0.177
	(3.18)	(2.56)	(2.99)	(1.16)	(1.88)	(1.85)	(3.15)	(3.25)	(1.30)
Kleibergen-Paap F-stat	44.20	63.89	44.20	44.20	44.20	44.20	41.61	44.20	7.29
Exogeneity (p-value)	0.00	0.01	0.00	0.22	0.07	0.06	0.00	0.00	0.20
Number of countries	121	103	50	25	34	121	121	109	121
Number of regions	29308	28453	6188	12052	8946	29308	29308	2988	2302
Number of observations	822468	672425	171718	349508	244227	822468	822468	81068	62967
	Panel B: CFA/2SLS								
(log) Aid (t-1)	0.0130***	0.0093***	0.0099***	0.0096	0.0216**	0.0738*	0.0490***	0.4579***	0.1975
	(4.20)	(3.60)	(3.44)	(1.36)	(2.80)	(1.89)	(4.84)	(5.46)	(1.38)
Aid*risk	-0.0001***	-0.0000***	-0.0000**	-0.0000	-0.0001***	-0.0005**	-0.0008***	-0.0034***	-0.0007**
	(4.01)	(2.97)	(2.31)	(0.84)	(5.03)	(2.02)	(4.61)	(4.30)	(2.46)
Kleibergen-Paap F-stat	44.20	63.89	44.20	44.20	44.20	44.20	20.00	44.20	7.29
Exogeneity (p-value)	0.00	0.01	0.00	0.23	0.06	0.09	0.00	0.00	0.29
Number of countries	91	82	36	24	23	91	91	85	91
Number of regions	27144	26517	5604	12041	7678	27144	27144	2577	1871
Number of observations	756858	630974	156932	349189	211256	756858	756858	72578	50753
First year	1991	1996	1991	1991	1991	1991	1991	1991	1991
Last year	2020	2020	2020	2020	2020	2020	2020	2020	2020

Note: The dependent variable in columns 1–5 and 7–8 is a binary indicator that is one for attacks on public officials (from GTD) and the number of attacks in columns 6 and 9. Aid is (log) disbursements (plus 1) from 18 European donors and the U.S. Panels A and B estimate a CFA (except for column 7). Column 8 shows conditional fixed effects Logit regressions where we have netted out country-year fixed effects from all explanatory variables before running the model with conditional fixed effects for ADM2. Columns 1–8 instrument aid with donor government fractionalization interacted with the recipient region's probability to receive aid. Column 9 instruments aid with donors' aid budgets interacted with the recipient region's probability to receive aid. Column 1 controls for the number of conflict events (from UCDP). Column 2 includes as control variables: corruption, (log) nightlights, (log) Human Development Indicator, and the number of oil extraction sites. Column 3 focuses on Africa, column 4 on the Americas, and column 5 on Asia. Panel A of Column 7 uses 2SLS for the estimation sample rather than predicting the first stage on all available observations; Panel B uses the interaction of the main IV with the political risk score as second instrument for the interaction of aid and risk rather than estimating a CFA. Risk is the political risk score, taken from the PRS Group's International Country Risk Guide, ICRG, ranging from 0 to 100, with higher values indicating better governance. Columns 1–7 and column 9 include country-year fixed effects; all regressions include the logarithm of a region's population size. Columns 1–8 include ADM2 fixed effects, column 9 includes ADM1 fixed effects. Standard errors are clustered at the country level.
t-statistics in parentheses; * p<0.10, ** p<0.05, *** p<0.01.

Table B3 – Aid and Targeted Political Violence, CFA, ADM2, Clustering Options

	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A			
(log) Aid (t-1)	0.009***	0.009***	0.009**	0.009***	0.009***	0.009***
	(4.06)	(4.35)	(2.60)	(2.85)	(3.82)	(5.47)
Number of countries	121	121	121	121	121	121
Number of regions	29308	29308	29308	29308	29308	29308
Number of observations	822468	822468	822468	822468	822468	822468
			Panel B			
(log) Aid (t-1)	0.0138***	0.0138***	0.0138***	0.0138***	0.0138***	0.0138***
	(5.28)	(5.58)	(3.77)	(4.18)	(5.03)	(7.54)
Aid*risk	-0.0001***	-0.0001***	-0.0001***	-0.0001***	-0.0001***	-0.0001***
	(5.46)	(5.29)	(3.53)	(3.98)	(5.26)	(6.54)
Number of countries	91	91	91	91	91	91
Number of regions	27144	27144	27144	27144	27144	27144
Number of observations	756858	756858	756858	756858	756858	756858
First year	1991	1991	1991	1991	1991	1991
Last year	2020	2020	2020	2020	2020	2020
Kleibergen-Paap F-stat	44.20	44.20	44.20	44.20	44.20	44.20
Cluster1	ADM1	ADM2	Country	ADM1	ADM1	Bootstrap
Cluster2			Year	Year	Country-year	

Note: We report regressions with different clustering of standard errors. The dependent variable is a binary indicator that is one for attacks on public officials (from GTD). Aid is (log) disbursements (plus 1) from 18 European donors and the U.S. Panels A and B use the CFA, instrumenting aid with donor government fractionalization interacted with the recipient region's probability to receive aid. Risk is the political risk score, taken from the PRS Group's International Country Risk Guide, ICRG, ranging from 0 to 100, with higher values indicating better governance. All regressions include country-year fixed effects, ADM2 fixed effects, and the logarithm of a region's population size. Standard errors are clustered as indicated in the table (column 6 bootstraps standard errors, with 1,000 repetitions).
t-statistics in parentheses; * p<0.10, ** p<0.05, *** p<0.01.

Table B4 – Aid and Targeted Political Violence, ADM2, CFA, Event Time and Moving Average

Variable Names	(1)	(2)
	Panel A: Event Time Specification	
(log) Aid (t+1)	0.00004	0.00008
	(0.48)	(1.07)
(log) Aid (t)	0.00019**	0.00022**
	(2.19)	(2.29)
(log) Aid (t-1)	0.00926***	0.01290***
	(3.32)	(4.33)
(log) Aid (t-2)	0.00002	0.00002
	(0.17)	(0.17)
(log) Aid (t-3)	0.00001	-0.00000
	(0.11)	(0.00)
Aid*risk		-0.00005***
		(3.42)
First year	1994	1994
Last year	2019	2019
Kleibergen-Paap F-stat	44.20	44.20
Number of countries	121	91
Number of regions	29308	27144
Number of observations	741227	681752
	Panel B: Moving Average	
(log) Aid (t-3 to t-1)	0.01507***	0.01926***
	(3.26)	(4.07)
Aid*risk		-0.00005***
		(3.39)
First year	1994	1994
Last year	2020	2020
Kleibergen-Paap F-stat	10.90	10.90
Number of countries	121	91
Number of regions	29308	27144
Number of observations	769428	708322

Note: The dependent variable is a binary indicator that is one for attacks on public officials (from GTD). Aid is (log) disbursements (plus 1) from 18 European donors and the U.S. Panel A shows an event time specification; Panel B includes aid as a moving average over the years t–3 to t–1. We instrument aid in t–1 with donor government fractionalization interacted with the recipient region's probability to receive aid (Panel B uses a moving average over the years t–4 to t–2). Risk is the political risk score, taken from the PRS Group's International Country Risk Guide, ICRG, ranging from 0 to 100, with higher values indicating better governance. All regressions include country-year fixed effects, ADM2 fixed effects, and the logarithm of a region's population size. Standard errors are clustered at the country level.
t-statistics in parentheses; * p<0.10, ** p<0.05, *** p<0.01.

Table B5 – Aid and Targeted Political Violence, CFA, ADM2, Placebo

	(1) DECO referendum
	Panel A
(log) Aid (t-1)	0.0000437
	(0.30)
Number of countries	121
Number of regions	29308
Number of observations	764520
	Panel B
(log) Aid (t-1)	0.0001705
	(0.79)
Aid*risk	-0.0000022
	(1.12)
Number of countries	91
Number of regions	27144
Number of observations	702001
First year	1991
Last year	2017
Kleibergen-Paap F-stat	44.20

Note: The dependent variable is a binary indicator that is one for violent events related to a (national or subnational) referendum (data from DECO). Aid is (log) disbursements (plus 1) from 18 European donors and the U.S. Panels A and B use the CFA, instrumenting aid with donor government fractionalization interacted with the recipient region's probability to receive aid. Risk is the political risk score, taken from the PRS Group's International Country Risk Guide, ICRG, ranging from 0 to 100, with higher values indicating better governance. All regressions include country-year fixed effects, ADM2 fixed effects, and the logarithm of a region's population size. Standard errors are clustered at the country level.
t-statistics in parentheses; * p<0.10, ** p<0.05, *** p<0.01.

Table B6 – Aid and Political Violence by Sector, CFA, ADM2

	(1)	(2)	(3)
	Panel A		
(log) Aid (t-1), social	0.0080***		
	(3.34)		
(log) Aid (t-1), production		0.0094**	
		(2.45)	
(log) Aid (t-1), economic			0.0071***
			(2.68)
Number of countries	121	121	121
Number of regions	29308	29308	29308
Number of observations	822468	822468	822468
	Panel B		
(log) Aid (t-1), social	0.0123***		
	(4.47)		
Aid*risk, social	-0.0001***		
	(3.22)		
(log) Aid (t-1), production		0.0134***	
		(3.48)	
Aid*risk, production		-0.0001**	
		(2.36)	
(log) Aid (t-1), economic			0.0113***
			(3.57)
Aid*risk, economic			-0.0001***
			(3.87)
Number of countries	91	91	91
Number of regions	27144	27144	27144
Number of observations	756858	756858	756858
First year	1991	1991	1991
Last year	2020	2020	2020
Kleibergen-Paap F-stat	76.48	61.89	48.65

Note: The dependent variable is a binary indicator that is one for attacks on public officials (from GTD). Estimation is with the Control Function Approach, using donor government fractionalization interacted with the recipient region's sector-specific probability to receive aid as instrument. Aid is (log) disbursements (plus 1) from 18 European donors and the U.S. Our sectoral definitions follow the OECD-DAC: Social Infrastructure & Services includes Education, Health, Population Policy/Programs & Reproductive Health, Water Supply & Sanitation, Government & Civil Society, and Other Social Infrastructure & Services. Economic Infrastructure & Services includes Transport & Storage, Communications, Energy, Banking & Financial Services, and Business & Other Services. The Production Sector includes Agriculture, Forestry, Fishing, Industry, Mining, Construction, Trade Policies & Regulations, and Tourism. Risk is the political risk score, taken from the PRS Group's International Country Risk Guide, ICRG, ranging from 0 to 100, with higher values indicating better governance. All regressions include ADM2 fixed effects and country-year fixed effects, and the logarithm of a region's population size. Standard errors are clustered at the country level.
t-statistics in parentheses; * p<0.10, ** p<0.05, *** p<0.01.

Table B7 – List of Countries

Afghanistan	Fiji	Nigeria
Albania	Gabon	North Korea
Algeria	Gambia	Oman
Angola	Georgia	Pakistan
Argentina	Ghana	Panama
Armenia	Guatemala	Papua New Guinea
Azerbaijan	Guinea	Paraguay
Bangladesh	Guinea-Bissau	Peru
Barbados	Guyana	Philippines
Belarus	Haiti	Republic of Congo
Belize	Honduras	Rwanda
Benin	India	Senegal
Bhutan	Indonesia	Serbia
Bolivia	Iran	Sierra Leone
Bosnia and Herzegovina	Iraq	Slovenia
Botswana	Jamaica	Solomon Islands
Brazil	Jordan	Somalia
Burkina Faso	Kazakhstan	South Africa
Burundi	Kenya	Sri Lanka
Cambodia	Kyrgyzstan	Sudan
Cameroon	Laos	Suriname
Cape Verde	Lebanon	Swaziland
Central African Republic	Lesotho	Syria
Chad	Liberia	Tajikistan
Chile	Libya	Tanzania
China	Macedonia	Thailand
Colombia	Madagascar	Timor-Leste
Comoros	Malawi	Togo
Costa Rica	Malaysia	Trinidad and Tobago
Croatia	Mali	Tunisia
Cuba	Mauritania	Turkey
Côte d'Ivoire	Mauritius	Turkmenistan
Democratic Republic of the Congo	Mexico	Uganda
	Moldova	Ukraine
Djibouti	Mongolia	Uruguay
Dominican Republic	Morocco	Uzbekistan
Ecuador	Mozambique	Venezuela
Egypt	Myanmar	Vietnam
El Salvador	Namibia	Yemen
Equatorial Guinea	Nepal	Zambia
Eritrea	Nicaragua	Zimbabwe
Ethiopia	Niger	

Note: This table lists the countries included in the estimation sample of column 1 in Table 1.

Table B8 – Variable Definitions

Variable	Definition	Source
(log) Aid	(log) Aid disbursements (plus 1) from 18 European donors and the U.S.	Bomprezzi et al. (2025a)
Aid dummy	Binary indicator that is one for any aid disbursements from 18 European donors and the U.S.	Bomprezzi et al. (2025a)
(log) Aid, social	(log) Aid disbursements (plus 1) to social infrastructure sector from 18 European donors and the U.S.	Bomprezzi et al. (2025a)
(log) Aid, production	(log) Aid disbursements (plus 1) to production sector from 18 European donors and the U.S.	Bomprezzi et al. (2025a)
(log) Aid, economic	(log) Aid disbursements (plus 1) to economic infrastructure sector from 18 European donors and the U.S.	Bomprezzi et al. (2025a)
(log) Aid, IDA	(log) Aid disbursement (plus 1) from IDA	Bomprezzi et al. (2025a)
Fractionalization IV	Donor government fractionalization instrumental variable	own calculation
Aid budget IV	Donor government aid budget instrumental variable	own calculation
Political violence, dummy, GTD	Attacks on public officials, binary	START (2022)
Political violence, number, GTD	Number of attacks on public officials	START (2022)
Political violence, dummy, DECO	Attacks involving the state for electoral reasons, binary	Fjelde and Höglund (2022)
Political violence, dummy, ECAV	Contentious events related to national elections, binary	Daxecker et al. (2019)
Political violence, dummy, ACLED	Political violence and demonstrations, binary	Raleigh et al. (2023)

Variable	Definition	Source
ACLED local officials, dummy	Political violence targeting local officials, binary	Raleigh et al. (2023)
DECO incumbent, dummy	Fatal electoral violence related to the incumbent party, binary	Fjelde and Höglund (2022)
DECO incumbent target, dummy	Fatal electoral violence targeting the incumbent party, binary	Fjelde and Höglund (2022)
DECO incumbent perpetrator, dummy	Fatal electoral violence with the incumbent party as perpetrator, binary	Fjelde and Höglund (2022)
DECO politician, dummy	Fatal electoral violence targeting politicians, binary	Fjelde and Höglund (2022)
DECO legislative election, dummy	Fatal electoral violence related to legislative elections, binary	Fjelde and Höglund (2022)
DECO executive election, dummy	Fatal electoral violence related to executive elections, binary	Fjelde and Höglund (2022)
DECO subnational election, dummy	Fatal electoral violence related to subnational elections, binary	Fjelde and Höglund (2022)
ECAV party, dummy	Electoral violence targeting party actors, binary	Daxecker et al. (2019)
ECAV government, dummy	Electoral violence targeting the government, binary	Daxecker et al. (2019)
DECO one-sided, dummy	Fatal electoral violence targeting civilians, binary	Fjelde and Höglund (2022)
ACLED civilians, dummy	Political violence targeting journalists and media workers, binary	Raleigh et al. (2023)
ICEWS coerce, dummy	An actor in power oppresses civilians, binary	Boschee et al. (2015)
DECO referendum, dummy, dummy	Violent events related to referendum, binary	Fjelde and Höglund (2022)
Total number of conflict events, UCDP	Political violence, number of events	Sundberg and Melander (2013), Davies et al. (2024)
Election, dummy	Election in one or two years, binary	Beck et al. (2001)
Political Risk Score	Political risk score, with higher values indicating better governance	The PRS Group (2024)

Variable	Definition	Source
Government stability	Index of absence of government stability, with higher values indicating better governance	The PRS Group (2024)
Democracy	Electoral democracy index	Coppedge et al. (2025)
(log) Population	(log) Population count in the region	CIESIN (2018)
Corruption, subnational*	Subnational corruption index	Crombach and Smits (2024)
(log) Nightlights*	(log) Mean of nighttime lights	Li et al. (2020)
(log) Human Development Index*	(log) Subnational human development index	Sherman et al. (2023)
Oil extraction sites, number	Number of oil extraction sites	Denly et al. (2022)
Africa, dummy	Region in Africa, dummy	Bomprezzi et al. (2025a)
Americas, dummy	Region in America, dummy	Bomprezzi et al. (2025a)
Asia, dummy	Region in Asia, dummy	Bomprezzi et al. (2025a)

* We take these variables from Bomprezzi et al. (2025c).

Table B9 – Descriptive Statistics

variable	count	mean	sd	min	max
(log) Aid	822468	0.99	3.36	0.00	20.33
Aid dummy	822468	0.08	0.27	0.00	1.00
(log) Aid, social	822468	0.66	2.74	0.00	20.31
(log) Aid, production	822468	0.12	1.23	0.00	20.04
(log) Aid, economic	822468	0.37	2.08	0.00	19.82
(log) Aid, IDA	742226	1.23	3.86	0.00	21.12
Fractionalization IV	822468	0.06	0.20	0.00	4.49
Aid budget IV	822468	6574.27	21179.11	0.00	487939.47
Political violence, dummy, GTD	822468	0.01	0.09	0.00	1.00
Political violence, number, GTD	822468	0.02	0.42	0.00	140.00
Political violence, dummy, ECAV	593653	0.00	0.06	0.00	1.00
Political violence, dummy, ACLED	227346	0.17	0.37	0.00	1.00
ACLED local officials, dummy	227346	0.01	0.11	0.00	1.00
DECO state, dummy	736551	0.00	0.03	0.00	1.00
DECO incumbent, dummy	736545	0.00	0.04	0.00	1.00
DECO incumbent target, dummy	736545	0.00	0.03	0.00	1.00
DECO incumbent perpetrator, dummy	736544	0.00	0.03	0.00	1.00
DECO politician, dummy	736551	0.00	0.02	0.00	1.00
DECO legislative election, dummy	736551	0.00	0.03	0.00	1.00
DECO executive election, dummy	736551	0.00	0.03	0.00	1.00
DECO subnational election, dummy	736551	0.00	0.03	0.00	1.00
ECAV party, dummy	593653	0.00	0.05	0.00	1.00
ECAV government, dummy	593653	0.00	0.06	0.00	1.00
DECO one-sided, dummy	736551	0.00	0.03	0.00	1.00
ACLED civilians, dummy	227346	0.10	0.31	0.00	1.00
ICEWS coerce, dummy	742226	0.08	0.27	0.00	1.00
DECO referendum, dummy	736551	0.00	0.01	0.00	1.00
Total number of conflict events, ACLED	822468	0.25	8.10	0.00	2659.00
Election in one or two years, dummy	820193	0.29	0.45	0.00	1.00
Political Risk Score	758658	61.44	8.25	14.08	81.75
Government stability	758658	7.67	1.61	1.00	12.00
Democracy	822468	0.55	0.23	0.06	0.91
(log) Population	822468	10.33	1.66	0.00	16.93
Corruption, subnational	727205	56.05	9.37	12.70	86.10
(log) Nightlights	797963	0.24	2.36	-4.61	4.14
(log) Human Development Index	778218	-0.52	0.20	-2.06	-0.03
Oil extraction sites, number	822468	0.00	0.11	0.00	20.00
Africa, dummy	822468	0.21	0.41	0.00	1.00
Americas, dummy	822468	0.42	0.49	0.00	1.00
Asia, dummy	822468	0.30	0.46	0.00	1.00

Note: Descriptive statistics are for the estimation sample of column 1 in Table 1.