Pacifying Elites, Endangering Communities: A Dual-Arena Theory of Post-Conflict Violence[*]

Giovanny Alvarez[1]

[1]Department of Public Policy, University of North Carolina at Chapel Hill

Abstract

Why do amnesties that succeed in ending armed conflict fail to reduce—or even increase—societal violence? This study develops a Dual-Arena Theory arguing that transitional justice mechanisms operate differently across two spheres: the Elite Arena of political violence and the Local Arena of societal violence. Amnesties covering human rights violations (HRV amnesties) pacify elites by providing credible security guarantees but endanger communities by transmitting an impunity signal that erodes rule of law. These amnesties represent strategic elite choices that do not satisfy international accountability norms in form while preserving impunity in substance. Using a Callaway-Sant'Anna difference-in-differences estimator across 68 post-conflict countries (1989–2020), I find that HRV amnesties reduce conflict-related deaths by 82% while increasing homicides by 13–31% over fifteen years. Truth commissions implemented without HRV coverage reduce political violence without increasing societal violence, confirming that impunity—not transition itself—drives Local Arena effects. These findings reveal a hidden cost of negotiated peace: elite pacification comes at the expense of everyday security.

1 Introduction

In the aftermath of intrastate war, post-conflict transition is often a violent affair. While peace agreements successfully halt clashes between armies, many post-conflict

---

[1]**Giovanny Alvarez** is a Ph.D. candidate at the Department of Public Policy at Chapel Hill. Email: giovanny@ad.unc.edu

societies witness a paradoxical surge in lethal violence. In countries ranging from El Salvador and Guatemala to South Africa and recently Colombia, the formal end of hostilities has been followed not by the restoration of rule of law, but by an increase in homicide that frequently surpasses the lethality of the war itself (Cruz, 2011; Steenkamp, 2005; Steenkamp, 2022; Trejo et al., 2018). This phenomenon challenges the Weberian definition of the state: a government may successfully reclaim the monopoly on political force while simultaneously losing the monopoly on societal force. Understanding this paradox—where political peace coincides with societal violence—requires examining the institutional choices that shape post-conflict transitions.

The existing literature offers limited guidance. Studies present mixed and often contradictory findings (Olsen et al., 2010), with some suggesting transitional justice fosters sustainable peace (Sikkink, 2011) and others warning it can destabilize transitions or exacerbate conflict (Loyle & Binningsbø, 2018; Snyder & Vinjamuri, 2003). This lack of consensus reflects a deeper conceptual problem: the literature has largely treated "post-conflict violence" as a unitary phenomenon measured through conflict recurrence, while ignoring the societal violence that claims far more lives in many post-conflict settings. By failing to disaggregate violence, existing studies cannot detect why mechanisms that succeed in ending wars may simultaneously fuel homicide.

This study addresses this gap by asking: *Why do amnesties that succeed in ending armed conflict fail to reduce—or even increase—societal violence in post-conflict communities?*

I propose the **Dual-Arena Theory of Post-Conflict Violence**, which posits that the state's monopoly on violence operates across two distinct spaces: the **Elite Arena**, where armed leaders negotiate political settlements, and the **Local Arena**, where communities navigate everyday security. Building on Loyle (2025) insight that

transitional justice policies reflect strategic elite choices, I argue that elites design amnesty arrangements to resolve their own security dilemmas while externalizing costs onto local communities. As international norms have delegitimized blanket amnesties, states increasingly adopt mechanisms framed as "restorative" alternatives—truth commissions, reparations, conditional amnesty schemes—that satisfy international audiences while functioning domestically as vehicles for perpetrators to escape prosecution (Mallinder, 2008; Sikkink, 2011).

While amnesties covering human rights violations (HRV amnesties) may resolve elite security dilemmas, they generate a "Local Arena Effect" through reinforcing channels. At the symbolic level, granting immunity for serious crimes communicates that violations carry no consequences—altering cost-benefit calculations for demobilized combatants while foreclosing legitimate redress for victims, who may turn to extrajudicial revenge. At the practical level, HRV amnesties typically accompany security sector and justice reforms that create immediate capacity gaps precisely when impunity perceptions are highest (Call & Wyeth, 2008; Tiscornia, 2024). The Local Arena thus experiences not merely rising crime but a broader unraveling of everyday security.

From this theory, I derive two hypotheses. First, post-conflict societies granting HRV amnesties will experience higher societal violence than those that do not (**H1**). Second, transitional justice mechanisms signaling accountability—truth commissions without HRV amnesties—will not increase societal violence (**H2**), serving as a falsification test distinguishing impunity from general post-conflict fragility.

I test these hypotheses using a staggered difference-in-differences design across 68 post intra-state conflict country cases occurred between 1989 and 2019, disaggregating outcomes into conflict deaths (Elite Arena) and homicides (Local Arena). The results support the Dual-Arena trade-off: HRV amnesties reduce conflict violence by

approximately 82% while increasing homicides by 13–31% over fifteen years. Truth commissions without HRV coverage reduce political violence with no increase in homicides, confirming that impunity—not transition itself—drives Local Arena violence.

This study makes three contributions. First, it introduces the Dual-Arena framework, demonstrating that elite peace and local security are trade-offs paid in different currencies of violence. Second, it identifies impunity signaling as a driver of the "conflict-crime nexus," challenging structural explanations attributing post-war crime solely to weak institutions. Third, it reveals that the price of buying elite peace with impunity is paid by communities bearing the resulting disorder.

The paper proceeds as follows. Section 2 reviews the literature on transitional justice and post-conflict violence. Section 3 develops the Dual-Arena Theory and hypotheses. Section 4 describes the research design. Section 5 presents results. Section 6 discusses implications. Section 7 concludes.

2   Literature Review

This review navigates the extensive literature on post-conflict violence to situate the central argument of this study. I begin by establishing the dual nature of violence that persists after war. I then examine the dominant policy framework designed to address this challenge—transitional justice—synthesizing the core "peace versus justice" debate and the evolution of amnesty practices in the post-ICC era. By charting this intellectual landscape, I identify a critical gap: the lack of systematic analysis of how amnesties covering human rights violations produce divergent effects across political and societal violence.

2.1   The Dual Nature of Post-Conflict Violence

The relationship between post-conflict environments and violence has been examined through two primary dimensions. Political violence refers to deliberate physical aggression stemming from political cleavages, motivated by the desire to obtain or maintain political power (Bellamy, 2012; Kalyvas, 2006). Societal violence encompasses harmful acts within community relationships—homicide, sexual assault, gang activity—violence that often persists or escalates after formal hostilities cease (Archer & Gartner, 1976, 1984; Bourgois, 2001).

Modern civil war creates a durable nexus between political conflict and organized crime. The economic logic of rebellion often requires armed groups to develop illicit enterprises; as Collier and Hoeffler (2004) argue, the viability of rebellion depends on access to revenue from sources like primary commodity exports. These activities can extend beyond ideologies and prove lethal against civilians through extortion or kidnapping (Balcells & Kalyvas, 2025; Sanín, 2004). Critically, wartime economic structures do not vanish with peace agreements; they often transform into criminal organizations that continue to prey on populations (Davies et al., 2024; Finkelstein et al., 2023; Muggah & Krause, 2009). The tactics of war also create legacies: systematic sexual violence can become entrenched as societal harm (Wood, 2009), and targeting social leaders may persist as localized violence (Alvarez, 2025; Voyvodic, 2021).

States transitioning from civil conflict face significant challenges due to fragile institutional frameworks (Campbell & Spilker, 2021). Inadequate institutions may fail to constrain elite power (Walter, 2015), fostering environments susceptible to corruption and social unrest that increase post-conflict violence (Collier et al., 2008, 2009). Various factors—political and economic incentives (Collier et al., 2003; Fearon & Laitin, 2003), impunity (Hayner, 2010), and cultural legacies of violence (Gal-

tung, 1969)—perpetuate societal violence even after formal peace. Violence frequently transforms rather than dissipates, suggesting that peace agreements may sustain or exacerbate underlying harm (Haidar, 2022; Pearce & Dietrich, 2019).

This literature illustrates the core empirical puzzle: peace agreements do not end all violence. Post-conflict societies face two fundamentally different types of violence—elite-level political violence driven by strategic calculations, and local-level societal violence shaped by community dynamics, criminal enterprises, and perceptions of legitimacy.

2.2   The Transitional Justice Debate

The international community has adopted transitional justice as the principal framework guiding post-war reconstruction. From Nuremberg to contemporary settlements like Colombia's, transitional justice has evolved as an approach to confronting mass atrocity, re-establishing rule of law, and preventing conflict recurrence (UNSC, 2004).

The field remains characterized by fundamental tensions. At its core lies the "peace versus justice" dilemma: should priority be securing immediate peace through compromise, or establishing long-term stability through accountability? The academic literature documents various mechanisms: criminal trials in domestic, international, or hybrid formats; truth commissions designed to support victims' right to know; reparation programs; and vetting procedures (Bunselmeyer & Schulz, 2020; De Greiff, 2012; Hayner, 2010; Roht-Arriaza, 2016; Skaar, 2011; Teitel, 2000).

A traditional perspective emphasizes accountability. Scholars argue that trials deter future violence and establish stable democracy (Meernik, 2005). Research by Dancy and Wiebelhaus-Brahm (2018) demonstrates that prosecuting rebels increases

the likelihood of conflict termination. From this view, impunity signals that violence is legitimate, undermining long-term peace. The pursuit of accountability is complicated by the pragmatic use of amnesties. Proponents contend that amnesties provide security guarantees needed to bring armed actors to negotiation (Mallinder, 2008; Snyder & Vinjamuri, 2003). Some evidence suggests peace agreements with amnesty provisions are not inherently less stable (Olsen et al., 2010). Critics counter that amnesties violate international law, entrench impunity, and fail to deter atrocities (Binningsbø & Nordås, 2022; De Greiff, 2006; Roht-Arriaza, 2016).

Amnesties: Conditionality, Scope, and Human Rights Violations

The first dimension concerns *conditionality*—whether beneficiaries must meet certain requirements to receive immunity. *Unconditional amnesties* grant immunity automatically, without requiring anything from beneficiaries such as truth-telling, acknowledgment of wrongdoing, or formal application. *Conditional amnesties*, by contrast, require beneficiaries to fulfill specific obligations, which may include disclosure of information, cooperation with truth commissions, acknowledgment of responsibility, or meeting criteria related to the political motivation of the offense (Mallinder, 2007; Olsen et al., 2010; Roht-Arriaza, 2016). The second dimension concerns *scope*—specifically, whether amnesties cover human rights violations. Some amnesties provide comprehensive immunity for all conflict-related crimes, including severe human rights abuses such as torture, extrajudicial killing, and sexual violence. Others explicitly exclude such violations from coverage, limiting protection to political crimes such as rebellion, sedition, and treason while preserving prosecutorial options for the most egregious offenses (Sriram et al., 2012; Teitel, 2014).

These two dimensions are analytically independent: a conditional amnesty may still cover human rights violations (as in South Africa's Truth and Reconciliation Com-

mission, where perpetrators could obtain amnesty for politically motivated killings and torture by fully disclosing their actions), while an unconditional amnesty could theoretically exclude such crimes. However, in practice, unconditional amnesties have typically encompassed both political offenses and human rights violations (Mallinder, 2008). For this study, I focus on the scope dimension: whether amnesties cover human rights violations (HRV amnesties) or exclude them (Non-HRV amnesties). This focus reflects my theoretical argument that the impunity signal transmitted to local communities depends primarily on whether perpetrators of the most serious crimes receive immunity, rather than on the procedural conditions attached to that immunity.

The historical frequency of amnesties has declined since the ICC became operational in 2002 (Figure 1). According to the Transitional Justice Evaluation Tool (**TJET**; Dancy et al., 2025), this aggregate trend masks a nuanced reality: while total provisions decreased, HRV amnesties have remained steady. The UN's 2004 position established that it cannot endorse amnesties for genocide, crimes against humanity, or war crimes (UNSC, 2004). Yet Mallinder (2007) Amnesty Law Database reveals that while blanket immunity for state agents declined, HRV amnesties persist. Recent cases confirm this: Syria's 2022 decree pardoned terrorism-related offenses; Chad's 2023 amnesty nullified prosecutions for a crackdown killing 128–300 protesters (USDS, 2023). A significant gap persists between international prohibitions and state practice.

Truth Commissions and the Amnesty-Truth Nexus

Scholarship suggests that combining amnesties with truth commissions balances peace and justice demands. Truth commissions provide forums for victim acknowledgment and collective memory, potentially mitigating impunity risks (Hayner, 2010; Olsen et al., 2010). Porter (2016) ethnographic work in Uganda found that community-

based truth-telling emphasized restoration over punishment, suggesting positive local effects. Case studies from Sierra Leone and South Africa highlight truth commissions' capacity for reconciliation (Hayner, 2010; Olsen et al., 2010).

This shift toward hybrid mechanisms presents challenges for assessing effectiveness (Ansorg & Kurtenbach, 2023; Belgorodski, 2024; Figari Layus & Vargas Trujillo, 2023; Voyvodic, 2021). Impunity can signal that violence is legitimate (De Greiff, 2012), yet research shows victim preferences for retribution versus reconciliation are highly contextual (Kao & Revkin, 2023). Contemporary mechanisms—such as Colombia's Special Jurisdiction for Peace, which sentences commanders to restorative activities rather than imprisonment, or Nepal's 2024 law permitting avoidance of prosecution through truth disclosure—substitute incarceration with conditional sanctions. Critics argue such frameworks signal that grave violations are negotiable in elite bargaining. Loyle (2025) captures this concern, identifying *strategic compliance*: institutions conforming to international expectations in form but not substance.

2.3   The Critical Gap: Societal Effects of HRV Amnesties

Despite this rich literature, a significant gap remains regarding how HRV amnesties affect violence beyond ending armed conflict. Scholars acknowledge that timing and sequencing matter (Hayner, 2010), and case studies show amnesties, truth commissions, and prosecutions are increasingly combined in complex arrangements (Autesserre, 2010; Figari Layus & Vargas Trujillo, 2023; Porter, 2016; Quinn, 2020). Amnesty effectiveness depends on timing, design, and scope (Dancy, 2018; Sriram et al., 2012). Yet systematic understanding of how HRV amnesties affect local-level violence—distinct from elite bargaining—remains underdeveloped.

This gap matters because top-down mechanisms securing elite pacts often clash
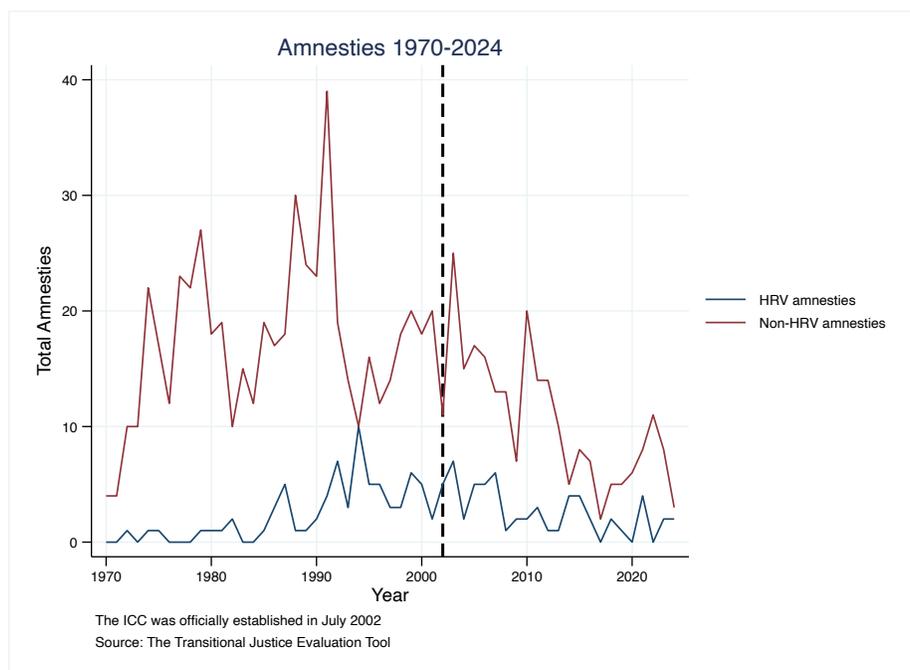
Figure 1: Amnesties, 1970 to 2024

with bottom-up requirements for local security—the peacebuilding dilemma (Autesserre, 2010, 2014; Dancy, 2018; Mac Ginty & Richmond, 2013; Steenkamp, 2022). While work has explored how trials affect conflict termination (Dancy & Wiebelhaus-Brahm, 2018), cross-national analysis of how HRV amnesties shape societal violence is absent. Research has not tested whether HRV amnesties succeed in pacifying elites while undermining rule of law perceptions locally, thereby increasing societal violence.

The core problem is that studies treat post-conflict violence as unitary, failing to disaggregate political from societal violence. A mechanism may appear successful if it reduces conflict recurrence, even as it harms everyday security. This study addresses this gap by analyzing HRV amnesty effects—with and without truth commissions—across both arenas. I ask: why do HRV amnesties that succeed in ending armed conflict fail to reduce—or even increase—societal violence in post-conflict communities?

3 Theoretical Framework: The Dual-Arena Theory of Post-Conflict Violence

3.1 Conceptual Foundations

I ground this study in the fundamental premise that a principal function of the state and the rule of law is to resolve the problem of violence (North et al., 2009; Pinker, 2018). Drawing upon Max Weber's (Weber, 1965) canonical definition, wherein a state constitutes a human community that successfully claims the monopoly of legitimate use of physical force within a given territory, I argue that post-conflict violence must be understood through the lens of two complementary arenas: the elite arena of political violence and the local arena of societal violence.

The *Elite Arena* constitutes a top-down political sphere composed of government leaders and the commanders of armed groups. In this arena, the dominant logic is one of strategic bargaining and credible deterrence, where the primary goal is to prevent a return to large-scale political violence. Transitional justice mechanisms are interpreted by these actors as signals about the costs and benefits of future aggression (Dancy et al., 2019; Elster, 2004). Within this framework, amnesties that cover human rights violations function as powerful incentives for demobilization, offering combatants and their leaders protection from future prosecution.

Simultaneously, these same policies operate in a *Local Arena*, a bottom-up social sphere inhabited by communities, victims, ex-combatants, and the criminal organizations that have survived the war. Here, the logic is not about elite deterrence but about perceptions of state legitimacy and the rule of law. A transitional justice path that succeeds in the Elite Arena can fail catastrophically in this space. When states grant immunity for human rights violations, they send a signal to local communi-

ties that violence carries no consequences—a message that can erode the normative foundations of peaceful social order.

Civil war profoundly reshapes the landscape of societal violence. The normalization of aggression, the proliferation of small arms, the breakdown of social trust, and the trauma experienced by individuals can all contribute to elevated levels of societal violence that persist long after a peace agreement is signed (Bara et al., 2021). This enduring violence is not merely a public health challenge; it directly reflects the state's capacity and legitimacy. Elevated levels of societal violence during post-conflict periods signal institutional weakness, particularly regarding the state's capacity to administer justice and maintain its monopoly on legitimate force. Given that the distinction between political and societal violence frequently becomes ambiguous following conflict termination, persistent societal violence transcends mere public security concerns and reflects the weakness of state institutions and the failure of peace-builders to extend legitimate state authority (Call & Wyeth, 2008; Finkelstein et al., 2023; Muggah & Krause, 2009).

The term *arena* is chosen deliberately over dimension because it signifies more than just two types of violence; it points to two separate spaces of action, each populated by different actors who operate according to distinct behavioral logics (Pinker, 2018). A core assumption of my theory is that the nexus between crime and war established during the conflict does not simply vanish with a peace agreement. Wartime criminal enterprises often persist, providing a direct pathway for the continuation of societal violence even when political violence is deterred (Finkelstein et al., 2023).

3.2 HRV Amnesties as Strategic Compliance

I focus my analysis on a specific and consequential form of transitional justice: amnesties that grant immunity for human rights violations. This focus is motivated by both theoretical and empirical considerations. Theoretically, HRV amnesties represent the most powerful incentive that negotiating parties can offer to armed actors, as they provide protection from prosecution for the most serious crimes committed during conflict.

Recent scholarship has fundamentally reshaped our understanding of how states engage with transitional justice mechanisms. Loyle (2025) argues that transitional justice institutions are not neutral technical responses to past violence, but profoundly political instruments whose structure reflects the balance of power that exists when violence ends. In this view, institutional design becomes a strategic act: decisions about what counts as a crime, who counts as a perpetrator, and which events are remembered are choices that shape the boundaries of accountability and, crucially, preserve impunity for those in power.

This insight is central to my theoretical framework. Loyle identifies what she terms *strategic compliance*: governments create transitional justice institutions that conform to international expectations in form but not in substance. These institutions may document violence and offer symbolic gestures of reconciliation, but they do so in ways that exclude or minimize the state's own role in perpetrating abuses. Transitional justice, in this sense, becomes a mechanism not for accountability, but for escaping it.

Contemporary mechanisms illustrate this logic. Colombia's Special Jurisdiction for Peace found former FARC-EP commanders responsible for over 21,000 kidnappings, constituting war crimes and crimes against humanity, yet sentenced them to

"special sanctions"—restorative activities including humanitarian demining, searching for the disappeared, and environmental recovery—rather than imprisonment (JEP, 2025). Nepal's 2024 transitional justice law permits individuals involved in human rights violations from the 1996–2006 conflict to avoid prosecution through truth disclosure, apology, or victim compensation, excluding only "serious violations" such as rape and intentional killings. However, the law's design creates significant accountability gaps: it distinguishes between "serious human rights violations" and ordinary "violations of human rights," with the latter eligible for amnesty, and requires that violations be committed "in a targeted or planned manner against an unarmed individual or community"—a threshold that could exclude many cases of war crimes and crimes against humanity from any criminal accountability (Watch, 2024). In Rwanda, the post-genocide government established Gacaca courts that projected a powerful narrative of justice, yet these mechanisms overwhelmingly targeted members of the defeated regime while leaving abuses committed by the victorious Rwandan Patriotic Front largely unexamined (Loyle, 2025). These cases reveal a common pattern: mechanisms substituting incarceration with conditional sanctions or selectively applying accountability to satisfy international standards while shielding those in power. From local community perspectives, such frameworks may signal that grave violations are negotiable commodities in elite bargaining rather than categorically prohibited acts.

This strategic logic leads me to center my framework on observable variation: comparing locations implementing HRV amnesties with those that did not, and examining the interactive effects of truth commissions with different amnesty frameworks.

3.3 Elite Impunity and Local Violence: Beyond the Weak State Argument

A conventional explanation for post-conflict violence centers on state capacity. According to this view, weak states lack the institutional infrastructure to enforce laws, maintain order, and protect citizens from violence (Fearon & Laitin, 2003; Walter, 2015). The causal story is straightforward: conflict destroys state capacity, and without effective policing, courts, and administrative presence, violence proliferates in the post-conflict period (Call & Wyeth, 2008; Collier et al., 2008). Under this logic, transitional justice policies are largely epiphenomenal—what matters is whether the state can rebuild its coercive and administrative apparatus.

I depart from this explanation. While I acknowledge that state capacity matters for violence outcomes, I argue that the *signal of elite impunity* transmitted through HRV amnesties operates as an independent causal mechanism that shapes violence in the Local Arena regardless of underlying state capacity. This argument rests on three interrelated claims.

First, HRV amnesties are not simply symptoms of weak states; they are strategic choices made by elites to secure their interests. Following Loyle (2025), I understand these policies as products of negotiation, contestation, and strategic choice rather than as reflections of institutional incapacity (see also Bell & Pospisil, 2017; Moe, 2005). Governments facing pressure to account for violence respond not by rejecting accountability norms, but by adapting to them—creating institutions that signal compliance while ensuring that perpetrators are shielded from responsibility (Sikkink, 2011). This strategic behavior can occur in states with varying levels of capacity; what matters is the political incentives facing elites at the moment of transition: the desire of government and military leaders to avoid prosecution for their own violations,

the need to offer credible security guarantees to armed opponents as a condition for demobilization (Mallinder, 2008; Snyder & Vinjamuri, 2003), and the imperative to project international legitimacy while preserving domestic power arrangements. These incentives—self-preservation, negotiation leverage, and regime survival—drive the design of transitional justice institutions independent of state capacity.

Second, the impunity signal operates through normative and cognitive channels that are distinct from state enforcement capacity. When a state grants immunity for human rights violations, it communicates to local communities that violence—even the most egregious forms—will not be punished (De Greiff, 2012). This message undermines the legitimacy of the legal order and weakens the normative constraints against violence (Roht-Arriaza, 2016). Citizens who observe perpetrators of atrocities going unpunished may update their beliefs about the acceptability and consequences of violence, independent of whether police are present on their streets (Tyler, 2021). The signal, in other words, shapes behavior through its effect on expectations and norms, not merely through its effect on the probability of punishment.

Third, the weak state argument cannot explain variation in violence outcomes among post-conflict societies with similar levels of state capacity but different transitional justice configurations. If state weakness were the primary driver of post-conflict violence, I would expect violence levels to track closely with measures of institutional capacity. Following Fearon and Laitin (2003), who use GDP per capita as a proxy for a state's "overall financial, administrative, police, and military capabilities," (p. 80). I control for GDP per capita and war duration in my empirical models. If the weak state argument is correct, these controls should absorb most of the variation in post-conflict violence, leaving little explanatory power for transitional justice variables. My theory, by contrast, predicts that HRV amnesties will have independent effects on local violence even after accounting for economic development and conflict

intensity—that is, the impunity signal operates through normative channels that are not reducible to state capacity. While GDP per capita is an imperfect proxy for state capacity (Hendrix, 2010), it remains the most widely used measure in cross-national conflict research and allows for comparison with prior studies.

I therefore propose that elite impunity—operationalized as HRV amnesties—constitutes a distinct causal pathway to local violence. This pathway operates through the erosion of rule of law perceptions and the weakening of normative constraints against violence. Importantly, while state capacity may condition the magnitude of this effect (stronger states may be better able to mitigate the consequences of impunity signals), the direction of the effect remains consistent: HRV amnesties increase local violence, holding state capacity constant.

This is not to say that state capacity is irrelevant. Indeed, state capacity likely shapes how peace agreements are negotiated in the first place. Weaker states may face stronger pressures to offer HRV amnesties because they lack the coercive resources to credibly threaten prosecution. In this sense, state capacity may be a confounder that influences both the selection of transitional justice policies and subsequent violence outcomes. However, the core theoretical claim remains: the impunity signal transmitted through HRV amnesties has independent effects on local violence that cannot be reduced to state capacity alone.

The Policy Bundle: HRV Amnesties and Complementary Transition Reforms

HRV amnesties rarely occur in isolation. Peace agreements that include immunity for human rights violations typically form part of a broader policy bundle designed to facilitate transition from conflict to peace. This bundle often includes security sector reforms mandating demilitarization, reductions in military and police personnel,

and decreased defense budgets—measures that are institutionally rational given the reduced threat environment following conflict termination (Call & Wyeth, 2008). In Central America, peace agreements in El Salvador and Guatemala required significant military downsizing, the dissolution of wartime security units, and the creation of new civilian police forces with reduced personnel and budgets (Pérez, 2015). Similarly, post-conflict criminal justice reforms often emphasize rehabilitation over punishment, reduce sentences for certain offenses, and prioritize restorative approaches—reforms consistent with international human rights standards but which may further weaken deterrent signals (UNSC, 2004).

I argue that these complementary reforms amplify the impunity signal transmitted by HRV amnesties through two channels. First, at the *symbolic level*, the combination of amnesty for past atrocities with softer criminal justice frameworks communicates a coherent message: the state prioritizes reconciliation over punishment. While normatively defensible, this message may erode the perceived certainty and severity of sanctions for violence more broadly. Second, at the *practical level*, demilitarization and police reform create transitional security gaps precisely when impunity signals are strongest. Reduced police presence, undertrained new forces, and budget constraints limit the state's capacity to enforce laws and deter potential offenders in local communities (Muggah & Krause, 2009; Tiscornia, 2024). The confluence of symbolic impunity and practical enforcement weakness may explain why HRV amnesties are associated with particularly severe societal violence outcomes in the Local Arena. This policy bundle logic has important implications for my empirical analysis. Because HRV amnesties are systematically associated with broader transition reforms, my estimates capture the combined effect of the amnesty and its typical policy accompaniments. While I cannot fully disentangle the independent contribution of each component, my theoretical framework suggests that the HRV amnesty is the linch-

pin: it is the amnesty that signals elite impunity and legitimizes the broader softening of the state's coercive posture. Without the amnesty, security sector reforms would not carry the same normative message about the acceptability of past violence. The amnesty, in other words, gives meaning to the policy bundle.

Scope: Pathways to HRV Amnesties

This theoretical framework encompasses HRV amnesties that emerge through distinct pathways. Drawing on post-conflict cases identified in my sample through the Uppsala Conflict Data Program (UCDP) and transitional justice provisions coded in the Transitional Justice Evaluation Tool dataset (**TJET**; Dancy et al., 2025), I find that approximately three-quarters of HRV amnesties arise from formal peace negotiations—comprehensive agreements that address not only transitional justice but also power-sharing, political participation, and institutional reform (Bell & Pospisil, 2017). In these cases, HRV amnesties function as bargaining instruments: governments extend immunity to armed opposition as a condition for demobilization, and in some cases accept mutual immunity covering state forces as well. However, roughly one-quarter of cases involve HRV amnesties granted outside formal peace processes—unilateral government decisions to extend immunity without comprehensive negotiated settlements. Importantly, across all cases in my analysis, HRV amnesties cover armed opposition forces—either exclusively or alongside state actors. No cases involve immunity extended solely to state forces while excluding opposition. This empirical pattern suggests that the core function of HRV amnesties in post-conflict settings is to facilitate rebel demobilization by removing prosecution threats, whether through negotiated agreement or unilateral government policy. The prevalence of opposition-only amnesties within negotiated settlements reflects a fundamental logic of conflict resolution: amnesty serves as the benefit rebels receive in exchange for

renouncing armed struggle. HRV amnesties function as one component of a comprehensive bargain in which armed groups abandon their political objectives—territorial control, revolutionary transformation, or secessionist demands—in exchange for judicial protection from prosecution (Mallinder, 2008; Snyder & Vinjamuri, 2003). The government concedes immunity but preserves its institutional authority. For my theoretical argument, what matters is that HRV amnesties—regardless of pathway or beneficiary configuration—transmit an impunity signal to the Local Arena. When armed actors who committed atrocities are seen to escape accountability, the message undermines rule of law perceptions whether those actors fought for or against the state. The core theoretical expectation is that HRV amnesties increase Local Arena violence relative to non-HRV amnesties across all pathways.

3.4   A Model of Elite Policy Choice and Arena-Specific Consequences

I model the selection of transitional justice mechanisms as a strategic decision embedded within broader peace negotiations. Transitional justice provisions—including amnesties—are rarely designed in isolation; they emerge as components of comprehensive agreements that address power-sharing, political participation, territorial control, and economic reform (Bell & Pospisil, 2017; Government-of-Colombia & FARC-EP, 2016). Following the logic of institutional design as an instrument of power (Loyle, 2025; Moe, 2005), I conceptualize transitional justice choices not as responses to abstract justice imperatives but as strategic acts that reflect elite interests and the balance of power at the moment of transition. Elites accept HRV amnesties not because they are indifferent to justice, but because amnesty provisions are often prerequisites for achieving other negotiation objectives—ending hostilities, securing

political inclusion, or preserving institutional arrangements. The rationality of HRV amnesties, in this sense, is political rather than juridical: they are instruments for achieving negotiated settlements, not expressions of normative commitments about accountability.

The central policy variable in my framework is whether an amnesty covers human rights violations (HRV amnesty) or does not (non-HRV amnesty). A secondary consideration is whether this amnesty is accompanied by a truth commission. This framework generates three analytically distinct policy configurations:

1. **HRV Amnesty**: Countries that grant amnesty covering human rights violations, with or without a truth commission. This represents the maximum incentive package for armed actors, offering comprehensive protection from prosecution. It also transmits the strongest impunity signal to the Local Arena.

2. **Truth Commission**: Countries that grant truth-seeking mechanisms and victims rights to truth and might or a not grant political amnesty excluding human rights violations. This configuration represents an attempt to balance demobilization incentives with accountability signals.

3. **Non-HRV Amnesty without Truth Commission**: Countries without truth commissions that might grant only limited political amnesties. This serves as the reference category, representing a minimal transitional justice approach with reduced impunity signals.

Theoretical Logic: The Elite Arena

From the perspective of the Elite Arena, HRV amnesties function as credible commitments that reduce the expected costs of demobilization for armed actors. By guaranteeing immunity for past violations, these policies lower the threshold for negotiated

settlements and reduce incentives for spoiler violence. The logic is straightforward: commanders who fear prosecution have strong incentives to continue fighting or to undermine peace processes; those who are guaranteed immunity do not. Consequently, I expect HRV amnesties to reduce political violence in the Elite Arena by removing the threat of prosecution that might otherwise motivate continued armed resistance.

Theoretical Logic: The Local Arena

From the perspective of the Local Arena, HRV amnesties carry a fundamentally different meaning. They signal to communities that violence—even the most egregious forms—will not be punished. This signal operates through two mechanisms.

First, it erodes the deterrent function of law. If human rights violations during conflict carry no legal consequences, why should ordinary violence in peacetime? The amnesty communicates that the state is unwilling or unable to enforce prohibitions against violence, weakening the credibility of legal threats more broadly.

Second, it undermines social trust and the legitimacy of state institutions. Communities that witness perpetrators go unpunished may lose faith in the state's commitment to justice. Transitional justice institutions shape not only who is punished and who is forgiven, but also how societies remember violence and rebuild legitimacy (see Loyle, 2025). When these institutions are designed to shield perpetrators, they undermine the very legitimacy they purport to restore.

3.5   The Dual-Arena Equilibrium

The Dual-Arena Theory posits a fundamental tension: the policies most attractive to elites for stopping a war may not be the policies that minimize overall societal violence. I represent this trade-off by plotting the expected violence curves for both

arenas against a continuum of policy configurations ranked by elite attractiveness (Figure 2).

This trade-off reflects the foundational dilemma of post-conflict state building: the tension between top-down, elite-focused peace pacts and bottom-up, justice-oriented societal rule of law. I model this from the perspective of a state seeking to minimize a *Social Cost Function*, $C(x)$, which represents the total risk of violence to the post-conflict order.

The horizontal axis represents the policy configuration x, ranked by elite preference—ranging from **Impunity** (HRV Amnesties) on the left to **Accountability** (Truth Commissions/Trials) on the right. The vertical axis denotes the expected violence level, normalized from 0 to 1.

The *Elite Arena Violence* curve, $V_E(x)$, represents the resistance to demobilization. It is monotonically increasing:

$$V_E(x) = C_E + k_E x^\alpha \tag{1}$$

Crucially, I do not argue that accountability $(x \to 3)$ automatically triggers an explosion of violence. Rather, this curve illustrates that as policies shift away from amnesty, the incentives for elites to renounce war diminish. Without the guarantee of legal immunity, the cost of exit for armed actors becomes prohibitively high (e.g., imprisonment), increasing the difficulty of reaching a negotiated settlement and raising the probability of war recurrence or spoiler violence. Thus, V_E captures the rising marginal cost of achieving elite pacification as accountability increases.

Conversely, the *Local Arena Violence* curve, $V_L(x)$, is monotonically decreasing. As policies shift from impunity toward accountability, the state signals a stronger commitment to the rule of law, increasing the cost of crime and diminishing interper-

sonal violence:

$$V_L(x) = C_L - k_L x^\beta \tag{2}$$

To identify the equilibrium, I adopt a *minimax* approach from decision theory, assuming that a stable state must avoid catastrophic failure in either arena (Neumann & Morgenstern, 2007). The Social Cost Function is defined as:

$$\mathbb{C}(x) = \max\{V_E(x), V_L(x)\} \tag{3}$$

The theoretical optimal policy, x^*, is the value that minimizes this function. As shown in Figure 2, this equilibrium occurs at the intersection of the two curves. Any deviation to the left (towards pure impunity) minimizes elite violence but causes local violence to spike—the "Local Arena Effect." Any deviation to the right (towards pure accountability) minimizes local violence but makes the war significantly harder to end, risking elite defection and recurrence.

My empirical results confirm that elites, driven by self-preservation, typically force the policy outcome toward $x = 1$ (HRV Amnesty). This choice successfully suppresses the recurrence risk (V_E) but allows local violence (V_L) to rise unchecked.

3.6   Hypotheses

Based on the Dual-Arena Theory, I derive two testable hypotheses. While I expect HRV amnesties to successfully deter conflict violence in the Elite Arena (as rationalist bargaining theory predicts), my primary theoretical focus is on the unintended consequences of these policies in the Local Arena.

Theoretical Equilibrium in the Dual-Arena Model
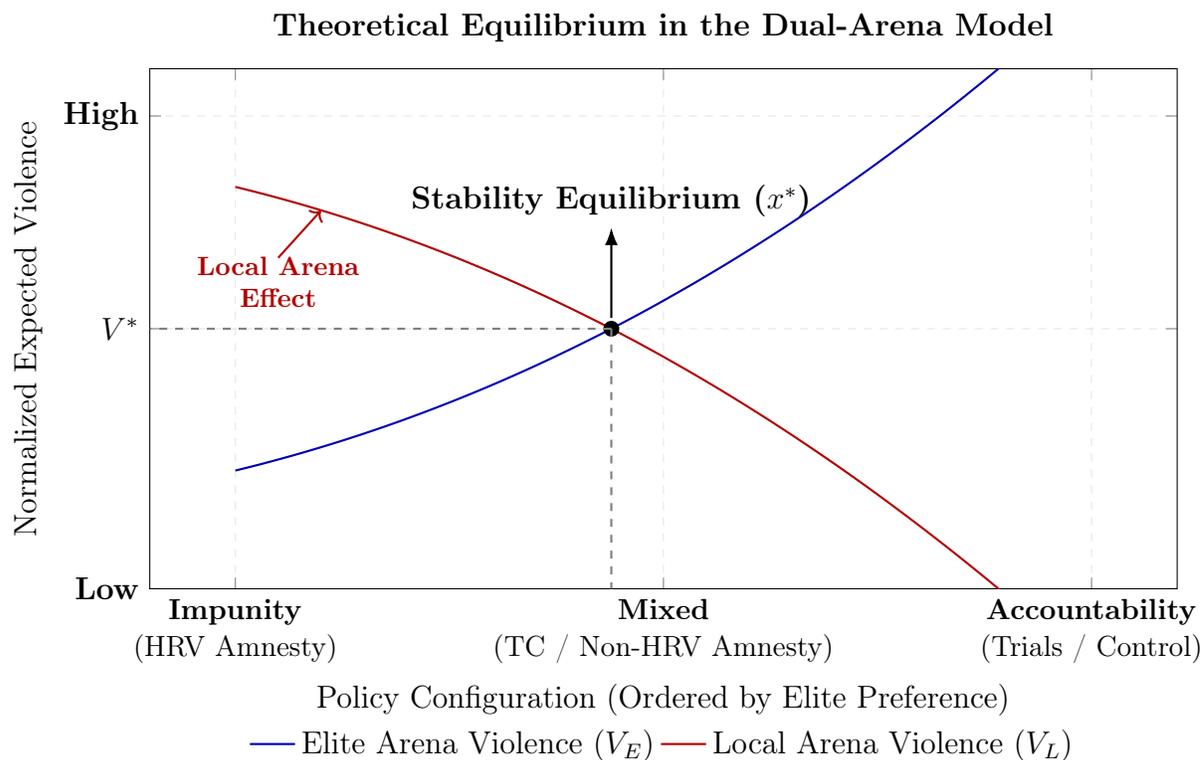
Figure 2: **The Dual-Arena Trade-off.** The graph illustrates the tension between minimizing Elite Violence (V_E) and Local Violence (V_L). While the stability equilibrium lies at x^*, policies of impunity (left) suppress elite violence at the cost of spiking local violence.

Main Hypothesis: Local Arena Effects

HRV Amnesty and Local Arena Violence (H1): Post-conflict societies that grant amnesties covering human rights violations will experience higher levels of societal violence compared to societies that do not grant such amnesties, controlling for state capacity and war duration.

This hypothesis follows directly from my argument regarding the impunity signal. By granting legal immunity for serious crimes, HRV amnesties undermine the credible threat of enforcement that underpins rule of law. For demobilized combatants and surviving criminal networks, this signals that the state prioritizes stability over justice, effectively lowering the expected costs of engaging in predatory violence. I expect this

effect to persist even after accounting for structural factors such as state capacity, proxied by GDP per capita (Fearon & Laitin, 2003), and the duration of the preceding conflict.

Secondary Hypothesis: The Mechanism of Impunity

To distinguish whether Local Arena violence is driven by the specific signal of impunity or merely by general post-conflict fragility, I propose a mechanism test using truth commissions.

The Mechanism of Impunity (H2): The increase in societal violence is driven specifically by the impunity signal transmitted through HRV amnesties. Therefore, transitional justice mechanisms that signal accountability—specifically truth commissions implemented *without* HRV amnesties—will not be associated with increased societal violence.

This hypothesis serves as a falsification test. If the rise in Local Arena violence described in H1 were simply a function of weak state capacity or general post-conflict instability, violence should rise across all transitional contexts regardless of the specific mechanism adopted. However, if the Dual-Arena Theory is correct, societal violence should increase only in cases where the state signals impunity through HRV amnesties. Cases that implement accountability mechanisms without granting immunity for human rights violations should not experience this effect, as the normative signal supporting rule of law remains intact.

3.7 Scope Conditions

The applicability of this framework is bounded by four scope conditions.

First, I focus exclusively on post-intrastate conflict settings where the state has

experienced significant erosion of its monopoly on violence. Post-interstate conflict dynamics, which often involve intact state bureaucracies and clearly defined external borders, operate under a different logic of deterrence outside the scope of this study.

Second, I assume that observed transitional justice variation reflects genuine elite strategic choice, even when constrained by structural factors. Following Loyle (2025), I conceptualize these policies not as purely exogenous impositions by international actors but as strategic acts reflecting the domestic balance of power at transition. Elites retain meaningful agency to prioritize their own survival in the Elite Arena over stability in the Local Arena.

Third, regarding temporal dynamics, I posit that the impunity mechanism operates through a dual temporal logic. The impunity signal is transmitted immediately upon amnesty enactment, altering the cost-benefit calculus of violence from the moment legal immunity is granted. However, the full magnitude of Local Arena effects—including the structural transformation of demobilized combatants and wartime networks into entrenched criminal enterprises—is cumulative. Thus, the theory predicts an effect that is immediate in onset but widens over the medium-to-long term.

Fourth, I acknowledge that state capacity influences transitional justice selection; weaker states may face stronger pressure to grant HRV amnesties to powerful spoilers who could otherwise derail peace processes. I address this endogeneity concern in the research design by conditioning on pre-treatment state capacity. The core claim of the Dual-Arena Theory is that the impunity signal operates independently: among states with similar economic capacity, those that grant HRV amnesties should experience higher Local Arena violence due to the normative signal these policies transmit.

4   Research Design

To test the hypotheses derived from the Dual-Arena Theory, this study employs
a staggered difference-in-differences (DiD) design analyzing a cross-national panel of
68 countries that experienced termination of intrastate armed conflicts between 1989
and 2020. The panel is balanced from 1980 to 2023, with the country-year as the unit
of analysis. Table 1 presents summary statistics for the sample.

4.1   Case Selection

The universe of cases is derived from the Uppsala Conflict Data Program (UCDP).
This study utilizes the UCDP Dyadic Dataset and Battle-Related Deaths Dataset,
both version 25.1 (Davies et al., 2025), to identify intrastate armed conflicts and their
termination dates. For countries experiencing multiple conflict episodes, I select the
most recent termination up to 2020. Countries with ongoing conflicts or terminations
after 2020 were excluded to ensure all cases have experienced a post-conflict period.

Inclusion criteria require that conflicts be concluded without immediate recur-
rence; in cases of war recurrence, the final post-conflict episode was selected. A
conflict dyad is considered terminated when battle-related deaths fall below the 25-
death annual threshold established by UCDP. Termination may thus reflect negotiated
peace agreements, decisive military victories, or exhaustion of hostilities without for-
mal settlement. The sample excludes military coups and one-sided violence without
sustained, organized armed opposition. Despite focusing on post-conflict episodes,
approximately half of the sample (34 countries) were simultaneously involved in at
least one other active conflict dyad during the observation period.

4.2 Variables

Dependent Variables

Data for both outcome variables are drawn from the Global Burden of Disease Study 2023 (GBD) produced by the Institute for Health Metrics and Evaluation (**IHME**), which provides annual estimates from 1980 to 2023.

The **Elite Arena** outcome is the logged rate of conflict-related deaths per 100,000 inhabitants, operationalized through the GBD variable "conflict and terrorism" (IHME, 2025). GBD defines these deaths as resulting from the instrumental use of violence by members of a group against another group to achieve political, economic, or social objectives. This variable captures the strategic behavior of political and military elites.

The **Local Arena** outcome is the logged rate of homicides per 100,000 inhabitants. This metric serves as a proxy for societal violence and the state's capacity to provide everyday security. The GBD dataset categorizes these deaths under "interpersonal violence," deliberately excluding combat-related fatalities to ensure analytical separation from organized political violence (IHME, 2025).

Independent Variables

The key explanatory variables are sourced from the Transitional Justice Evaluation Tool (TJET) dataset (Dancy et al., 2025).

HRV Amnesties: TJET defines amnesties as any legislative, constitutional, or executive provision granting immunity for criminal activity (Geoff et al., 2024). The treatment variable is a binary indicator for the year when amnesty covering human rights violations—including serious violations of international humanitarian law and war crimes—was enacted. All HRV amnesties in the sample were implemented be-

tween two years before conflict termination and three years after.

Truth Commissions: A binary variable coded as 1 if a country established a truth commission, following Hayner (2010) canonical definition: a body that (1) focuses on the past, (2) investigates a pattern of events over time, (3) engages broadly with the affected population, (4) is temporary with the aim of a final report, and (5) is officially authorized (p. 48).

Control Variables

Given that only 16 countries implemented HRV amnesties, I restrict the specification to two controls to ensure sufficient common support between treated and control groups. **War duration** is measured as years from conflict onset to termination using UCDP data. **Logged GDP per capita** is drawn from World Bank data (Bank, 2025) and serves as a proxy for state capacity following Fearon and Laitin (2003).

4.3   Empirical Strategy

This study employs the staggered difference-in-differences estimator proposed by Callaway and Sant'Anna (2021), which is robust to heterogeneous treatment effects and avoids the negative weighting biases inherent in traditional two-way fixed effects models when policy adoption timing varies across units (Goodman-Bacon, 2021).

Estimation Framework

Let Y_{it} denote the logged violence outcome for country i in year t, and let G_i represent the year country i first implemented the policy under investigation. Countries that never implement the policy serve as the control group. The primary parameter of interest is the group-time average treatment effect on the treated, $ATT(g, t)$, which

captures the average effect of adopting the policy in year g on the outcome at time t for units in cohort g. The model relies on a conditional parallel trends assumption: after controlling for pre-treatment covariates X_i, the average change in outcomes for treated units would have been the same as for untreated units, absent treatment.

Estimation utilizes the Doubly Robust Inverse Probability Weighting method (`dripw`), combining outcome regression and propensity score weighting (Sant'Anna & Zhao, 2020). I use only "not-yet-treated" and "never-treated" units as comparisons, avoiding already-treated units that could introduce bias.

Model Specifications

To test the Dual-Arena Theory and isolate the impunity mechanism, I structure the analysis around two models:

Model A (Testing H1): Compares countries implementing HRV amnesties against not-yet-treated and never-treated units. The objective is to estimate whether HRV amnesties produce simultaneous reductions in conflict deaths (Elite Arena) and increases in homicides (Local Arena).

Model B (Testing H2): Examines truth commissions implemented *without* HRV amnesties as a falsification test. If Local Arena violence were driven by general post-conflict fragility rather than impunity, this model should show increased homicides. A null effect would confirm that the impunity signal—not transition itself—drives societal violence.

The analysis defines three groups: (1) the *impunity group*—countries implementing HRV amnesties regardless of truth commission status; (2) the *accountability group*—countries implementing truth commissions without HRV amnesties; and (3) the *control group*—countries implementing neither mechanism.

Dynamic Estimation

Group-time ATTs are aggregated into event-study coefficients θ_τ, representing average effects τ years relative to treatment adoption. I specify an event-study window of $\tau \in [-5, 15]$. The pre-treatment window serves as a placebo test for parallel trends; systematic pre-trends would suggest selection into treatment correlates with pre-existing violence trajectories. The post-treatment window extends to 15 years to capture medium-term dynamics: while the impunity signal is transmitted immediately, Local Arena effects are expected to accumulate as demobilized combatants consolidate into criminal enterprises. I restrict the window to 15 years to mitigate compositional bias, as longer horizons increasingly reflect only early-treated cohorts (Callaway & Sant'Anna, 2021).

4.4   Descriptive Patterns

Table 2 presents descriptive statistics comparing the control group with the HRV amnesty group before and after treatment. Three patterns emerge.

First, unadjusted trends provide preliminary support for the Dual-Arena Theory. The HRV amnesty group exhibits high-intensity conflict prior to treatment (Mean Log Conflict Deaths = 0.64), dropping substantially post-treatment (-1.35). Conversely, societal violence is elevated pre-treatment (1.95 vs. 1.59 in controls) and increases further post-treatment (2.34). This divergence—reduced political violence coinciding with rising societal violence—mirrors the predicted trade-off.

Second, countries granting HRV amnesties are significantly poorer (Mean Log GDP per Capita = 6.26 vs. 7.23) and experience longer conflicts (15.13 years vs. 11.10 years), confirming that HRV amnesties are endogenous to contexts of lower state capacity. Conditioning on these covariates ensures estimated effects capture the

impunity signal rather than antecedent fragility.

Third, negotiated peace agreements are substantially more prevalent in the HRV amnesty group (79% vs. 17%), consistent with the theoretical expectation that HRV amnesties function primarily as bargaining instruments within comprehensive peace negotiations.

Table 3 compares the accountability and impunity groups directly. Both originate from structurally similar contexts (comparable GDP and conflict intensity), yet post-treatment outcomes diverge: societal violence rises in the impunity group but *declines* in the accountability group, converging with control levels. This pattern provides preliminary evidence that the Local Arena effect is specific to impunity rather than general post-conflict fragility.

4.5   Limitations

Several limitations warrant caution in interpreting results as strictly causal. First, policy adoption is endogenous; elites select HRV amnesties based on factors that may correlate with future violence trajectories. Although conditioning on GDP per capita and war duration mitigates observable selection bias, unobserved heterogeneity remains a threat.

Second, potential spillover effects between neighboring countries could violate the Stable Unit Treatment Value Assumption (SUTVA). To address this, I employ a "Donut" difference-in-differences design in robustness checks, excluding immediate geographic neighbors of treated units (Butts, 2023a, 2023b; Roth et al., 2023). The persistence of results under this specification suggests effects are driven by domestic institutional signals rather than regional contagion.

Third, the small number of treated units (16 countries with HRV amnesties) limits

statistical power and precludes additional sample restrictions.

Given these limitations, estimated ATTs are best interpreted as robust measures of the dynamic association between transitional justice mechanisms and violence patterns, conditional on observed state capacity, rather than definitive causal effects.

5   Results

This section presents the empirical findings from the Callaway-Sant'Anna difference-in-differences analysis. I first report results for the main hypothesis (H1) concerning the dual-arena trade-off, then turn to the mechanism test (H2) distinguishing impunity from general post-conflict fragility. Finally, I present robustness checks addressing spatial spillover concerns.

5.1   Main Results: The Dual-Arena Trade-off (H1)

Table 4 presents the estimated effects of HRV amnesties on both arenas of violence. Model A1 reports unconditional estimates, while Model A2 conditions on war duration and logged GDP per capita to address selection bias related to state capacity. I focus interpretation on Model A2 as the preferred specification. The results strongly support Hypothesis 1. In the **Elite Arena**, HRV amnesties are associated with a substantial and statistically significant reduction in conflict-related deaths. The average post-treatment effect is -1.70 log points ($p < 0.01$), indicating that political violence falls by approximately 82% following amnesty adoption. This effect emerges immediately—conflict deaths decline by 1.31 log points in the first year post-treatment—and remains stable through the observation period.

In the **Local Arena**, HRV amnesties produce the opposite effect. The average post-treatment effect on homicides is 0.119 log points ($p < 0.05$), representing approx-

imately a 13% increase in societal violence. Critically, the dynamic effects reveal that this increase is not immediate but *cumulative*. While the effect at $t = 1$ is modest (0.040, $p < 0.10$), it grows steadily over time, reaching 0.271 log points by year 15 ($p < 0.05$)—a 31% increase relative to the counterfactual.

Figure 3 displays the event-study plots corresponding to Model A2, illustrating this divergence graphically. The pre-treatment coefficients cluster around zero for both outcomes, supporting the parallel trends assumption. Following treatment, the trajectories diverge sharply: conflict deaths plummet while homicides rise gradually. This "X-shaped" pattern—where Elite Arena pacification coincides with Local Arena deterioration—provides striking visual confirmation of the dual-arena trade-off.

These findings are robust to the inclusion of covariates. Comparing Models A1 and A2, the point estimates remain stable, though precision improves slightly in the conditional specification. The substantive interpretation is unchanged: HRV amnesties successfully end wars but generate unintended consequences for everyday security.

5.2 Mechanism Test: Impunity versus Accountability (H2)

Table 5 presents results from the mechanism test, comparing HRV amnesties (Model B1) against truth commissions implemented without HRV coverage (Model B2). Both models use the "pure control" group—countries adopting neither mechanism—as the baseline comparison.

The results provide strong support for Hypothesis 2. In the Elite Arena, *both* mechanisms reduce political violence. HRV amnesties yield a larger reduction ($ATT = -2.086$, $p < 0.01$) than truth commissions ($ATT = -1.663$, $p < 0.05$), but both successfully pacify elites. This suggests that armed actors can be induced to demobilize
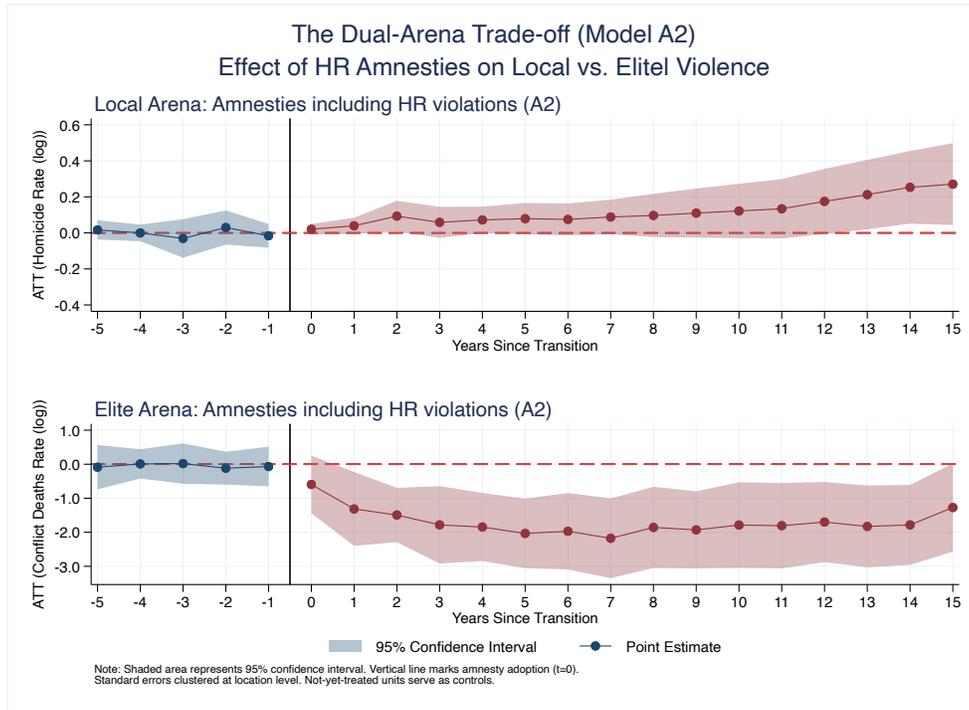
Figure 3: Models A

through either guaranteed impunity or structured truth-telling processes that offer acknowledgment without prosecution.

The critical divergence emerges in the Local Arena. Under HRV amnesties, homicides increase ($ATT = 0.104$), with the effect reaching statistical significance by year 15 ($ATT = 0.242$, $p < 0.10$). Under truth commissions without HRV coverage, however, the effect on homicides is *negative* ($ATT = -0.171$) and statistically indistinguishable from zero. The dynamic effects confirm this pattern: at no point in the 15-year post-treatment window do truth commissions produce an increase in societal violence.

Figure 4 displays the event-study plots for this comparison. The contrast is striking: while both mechanisms show downward trajectories in the Elite Arena (upper panels), only HRV amnesties show an upward trajectory in the Local Arena (lower-left panel). Truth commissions produce a flat or slightly declining trend in homicides
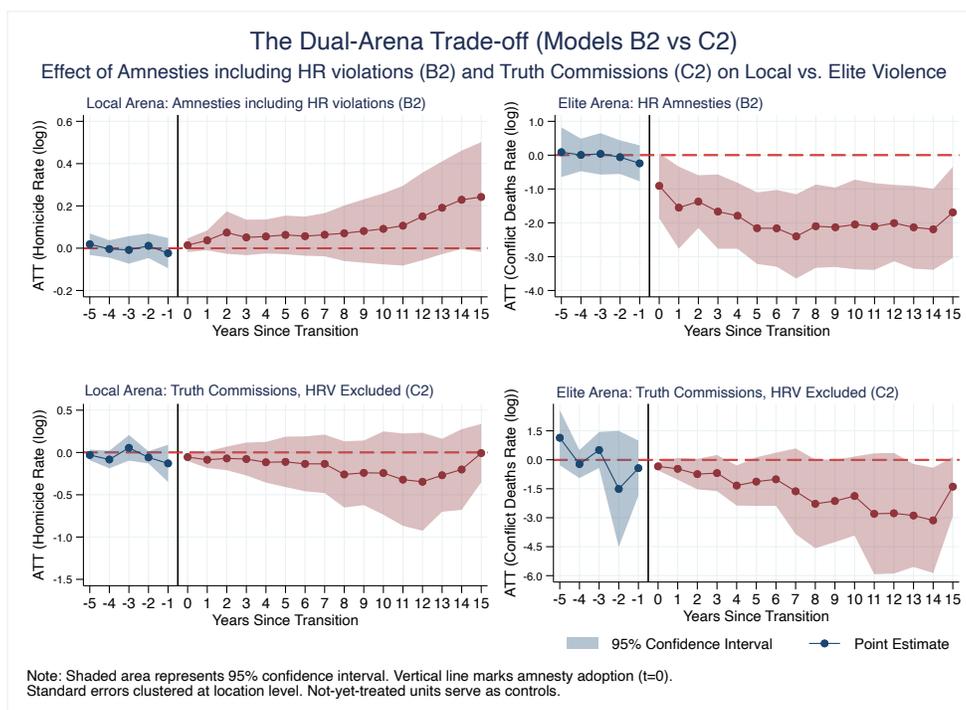
Figure 4: Models B

(lower-right panel).

This pattern directly falsifies the "weak state" alternative explanation. If post-conflict violence were simply a function of state fragility or the security vacuum left by war, we would expect homicides to rise in *both* groups, given their similar pre-treatment characteristics (comparable GDP per capita and conflict intensity, as shown in Table 3). The fact that violence increases only where impunity is granted—and remains stable or declines where accountability is pursued—confirms that the impunity signal is the operative mechanism.

5.3 Robustness: Spatial Exclusion

A potential threat to identification is spatial spillover: if violence diffuses across borders through regional contagion or criminal displacement, the estimated effects

could reflect neighborhood dynamics rather than domestic institutional mechanisms. To address this concern, I implement a "Donut" difference-in-differences design that excludes all immediate geographic neighbors of treated units from the control pool.

Table 7 presents results from this spatial exclusion test. Panel A shows that the dual-arena trade-off persists under HRV amnesties: conflict deaths decline ($ATT = -1.585$, $p < 0.01$) while homicides increase ($ATT = 0.130$, $p < 0.05$). The dynamic effects confirm that this is not a regional artifact—the divergence begins at $t = 1$ and grows over the post-conflict decade.

Panel B confirms the falsification test. Under truth commissions, the effect on homicides is statistically indistinguishable from zero ($ATT = -0.031$, $p = 0.78$), even when using only spatially distant controls. This provides definitive support for the Dual-Arena mechanism: the increase in Local Arena violence is specifically triggered by the impunity signal and is absent when states signal accountability.

6   Discussion

The empirical findings confirm the central prediction of the Dual-Arena Theory: HRV amnesties produce divergent effects across the two spheres of post-conflict violence. They succeed in the Elite Arena by providing credible security guarantees that induce combatant demobilization, but they fail in the Local Arena by transmitting an impunity signal that erodes rule of law perceptions and increases societal violence. This trade-off—invisible to studies focused solely on conflict recurrence—reveals a previously unexamined cost of negotiated peace.

6.1   Theoretical Implications

These findings advance our understanding of transitional justice in three ways.

First, they demonstrate the importance of *disaggregating* post-conflict violence. The dominant approach in the literature evaluates transitional justice against a single outcome: whether armed conflict recurs. By this metric, HRV amnesties appear successful—and indeed, the Elite Arena results confirm that they substantially reduce political violence. However, this metric obscures the consequences for ordinary citizens who must navigate daily life in post-conflict communities. The Local Arena results reveal that the peace elites negotiate is not the peace communities experience. Studies that aggregate political and societal violence, or that focus exclusively on conflict recurrence, systematically underestimate the costs of amnesty-based transitions.

Second, the findings support the theoretical mechanism of impunity signaling over alternative explanations rooted in state capacity. The falsification test using truth commissions is particularly important here. If post-conflict crime were simply a product of weak institutions or the security vacuum left by war, violence should rise regardless of which transitional justice mechanism is adopted. The null effect of truth commissions on Local Arena violence—combined with the positive effect of HRV amnesties—confirms that it is the *content* of the institutional signal, not the mere fact of transition, that drives societal violence. When states communicate that atrocities will be documented but not punished, they undermine the normative foundations of legal order. When states communicate that truth-telling provides a path to acknowledgment without blanket immunity for human rights violations, rule of law perceptions remain intact.

Third, the cumulative nature of the Local Arena effect has important implications for how we conceptualize post-conflict violence. The immediate effect of HRV amnesties on homicides is modest; the substantial increase emerges over the medium-to-long term, reaching 31% by year fifteen. This temporal pattern is consistent with

a mechanism operating through the gradual transformation of wartime networks into criminal enterprises. Demobilized combatants and surviving armed structures do not immediately pivot to predatory violence; rather, they exploit the permissive environment created by impunity to consolidate criminal operations over time.

Case-study evidence from Central America illustrates this process. Cruz (2011) demonstrates that post-conflict criminal violence in Guatemala, El Salvador, and Honduras stems not from socioeconomic conditions alone, but from the survival of "violent entrepreneurs"—wartime security forces, paramilitary networks, and their collaborators—who became embedded in post-transition institutions while maintaining relationships with governing elites. Where public security reforms failed to dismantle these networks, specialists in violence continued operating with impunity, gradually consolidating enterprises that drove homicide rates to among the highest in the world.

Trejo et al. (2018) provide systematic cross-national evidence for this mechanism, demonstrating that transitional justice mechanisms have divergent effects on criminal violence. Their central insight is that authoritarian regimes often allow "state specialists in violence" to transform political impunity—license to repress dissidents—into criminal immunity, enabling them to control illicit markets. When post-transition governments adopt robust accountability measures, particularly truth commissions that expose systemic patterns of abuse and generate evidence for prosecution, they break this cycle by removing perpetrators from state institutions. Conversely, amnesty laws that exonerate these specialists preserve the organizational infrastructure through which political violence is repurposed for criminal ends. Their conclusion—that "stability mechanisms" prioritizing elite accommodation produce more violence while "accountability mechanisms" produce less—directly parallels the dual-arena trade-off documented here.

Together, these findings illuminate the institutional mechanism linking HRV amnesties

to Local Arena violence: impunity preserves networks of wartime coercion, which are subsequently redirected toward criminal predation once political constraints relax. The policy implication is sobering—the full costs of amnesty-based transitions may not become apparent until years after peace agreements are signed.

6.2   Relation to Existing Literature

These findings speak to several debates in the transitional justice literature.

The results challenge the influential position that amnesties are necessary for peace and carry acceptable costs. Snyder and Vinjamuri (2003) argued that in contexts where perpetrators retain significant power, pursuing accountability risks reigniting conflict, making amnesty a pragmatic necessity. The present findings partially vindicate this position: HRV amnesties do facilitate conflict termination, substantially reducing Elite Arena violence. However, the Local Arena effects reveal that the cost-benefit calculus is more complex than previously understood. The 13% average increase in homicides—growing to 31% over fifteen years—represents thousands of additional deaths in post-conflict communities. These costs are not merely symbolic or ethical concerns raised by justice advocates; they are material consequences borne by civilians who had no seat at the negotiating table. The trade-off is not between peace and justice, as traditionally framed, but between elite peace and community security.

The findings also contribute to debates over truth commission effectiveness. Previous research has produced mixed results regarding whether truth commissions improve human rights outcomes (Dancy et al., 2019; Olsen et al., 2010). Olsen et al. (2010) proposed that transitional justice mechanisms work best in combination—particularly trials balanced by amnesties—rather than in isolation. The present

analysis suggests a different conditionality: the effect of truth commissions depends critically on whether they operate alongside HRV amnesties. When implemented as standalone accountability mechanisms without blanket immunity, truth commissions reduce political violence without generating Local Arena costs. When paired with HRV amnesties, however, the impunity signal dominates. This distinction may explain heterogeneous findings in prior work and suggests that future research should disaggregate truth commission effects by the broader transitional justice configuration in which they are embedded.

Finally, the results align with recent scholarship emphasizing the strategic dimensions of transitional justice. Loyle (2025) argues that transitional justice policies reflect elite calculations about survival rather than genuine commitments to accountability, with mechanisms designed to satisfy international audiences while preserving domestic impunity. The dual-arena trade-off documented here provides empirical support for this "strategic compliance" framework. HRV amnesties are not technical responses to post-conflict reconstruction challenges; they are political instruments reflecting the balance of power at transition. Elites successfully externalize the costs of their bargains onto local communities—securing their own safety through immunity while ordinary citizens bear the consequences of weakened rule of law. The irony is that mechanisms framed as promoting reconciliation and stability may ultimately undermine the everyday security upon which sustainable peace depends.

6.3   Policy Implications

The findings carry sobering implications for peacebuilding practice. The international community has increasingly accepted that some form of amnesty may be necessary to end protracted conflicts, while maintaining that immunity for the most

serious human rights violations should be avoided (UNSC, 2004). The present analysis provides empirical support for this position—but suggests that the stakes are higher than previously understood.

The dual-arena trade-off implies that policymakers face a genuine dilemma, not merely a normative tension. HRV amnesties are effective tools for ending wars; the Elite Arena results confirm this unambiguously. However, their effectiveness in one arena comes at the cost of failure in another. This trade-off cannot be resolved through better implementation or complementary programming; it is inherent to the mechanism by which amnesties operate. The same signal that reassures armed actors—"you will not be prosecuted"—undermines rule of law perceptions among ordinary citizens.

The results also suggest that truth commissions may offer a viable alternative pathway. When implemented without HRV coverage, they reduce political violence while avoiding the Local Arena costs associated with impunity. This finding supports the design of transitional justice frameworks that combine truth-telling with selective or conditional accountability—preserving incentives for demobilization while maintaining the normative signal that human rights violations carry consequences.

6.4 Limitations

Several limitations warrant caution in interpreting these findings.

First, despite the use of doubly robust estimation and extensive robustness checks, the analysis cannot definitively establish causality. Transitional justice adoption is endogenous to factors—such as the relative strength of armed actors or private information about spoiler threats—that may also influence violence trajectories. While conditioning on GDP per capita and war duration addresses observable selection, unobserved heterogeneity remains a threat.

Second, the relatively small number of treated units (16 countries with HRV amnesties, 17 with truth commissions) limits statistical power and precludes some desirable extensions. Heterogeneity analyses examining whether effects vary by amnesty configuration (mutual versus one-sided) or regional context would require larger samples than currently available.

Third, the analysis treats HRV amnesties as a homogeneous category, but considerable variation exists in amnesty design—including scope, conditionality, and implementation. Future research with more granular data could examine whether specific design features moderate the dual-arena trade-off.

Fourth, the reliance on national-level data may obscure important subnational variation. In many post-conflict settings, violence is geographically concentrated in specific regions—former conflict zones, demobilization corridors, or areas with weak state presence—rather than uniformly distributed across the national territory. National homicide rates may thus understate the severity of Local Arena effects in affected communities while overstating impacts in regions that experienced neither conflict nor its aftermath. This limitation means the estimated effects represent average national trends that may mask considerable within-country heterogeneity in how HRV amnesties translate into local violence patterns.

Fifth, the mechanism linking HRV amnesty to Local Arena violence—the impunity signal—is inferred rather than directly observed. While the falsification test provides strong circumstantial evidence, direct measurement of rule of law perceptions in post-conflict communities would strengthen the causal interpretation.

7 Conclusion

This study asked why amnesties that succeed in ending armed conflict fail to reduce—or even increase—societal violence in post-conflict communities. The answer lies in the dual-arena structure of post-conflict violence and the divergent signals that transitional justice mechanisms transmit to different audiences.

HRV amnesties operate as effective instruments for elite pacification. By removing the threat of prosecution, they provide credible security guarantees that induce combatant demobilization and reduce political violence. The empirical results confirm this: conflict-related deaths fall by approximately 82% following amnesty adoption—a substantial and sustained reduction that validates the rationalist logic underlying negotiated settlements.

However, the same mechanism that succeeds in the Elite Arena fails in the Local Arena. By signaling that human rights violations carry no legal consequences, HRV amnesties erode rule of law perceptions in local communities. Demobilized combatants, surviving criminal networks, and ordinary citizens update their beliefs about the acceptability and consequences of violence. The result is a gradual increase in societal violence—modest initially but growing to 31% above the counterfactual by year fifteen.

The mechanism test confirms that this increase is driven specifically by impunity, not by general post-conflict fragility. Truth commissions implemented without HRV coverage reduce political violence while producing no increase in homicides. This falsification of the "weak state" explanation provides strong evidence that the content of the institutional signal—impunity versus accountability—determines outcomes in the Local Arena.

These findings reveal a hidden cost of negotiated peace. The metric by which

the international community typically evaluates transitional justice—whether armed conflict recurs—systematically understates the costs of amnesty-based transitions. By disaggregating violence into its elite and local components, this study demonstrates that successful peacebuilding in one arena may come at the expense of everyday security in another. The peace that elites negotiate is not the peace that communities experience.

The Dual-Arena Theory developed here offers a framework for understanding this trade-off and, potentially, for designing transitional justice mechanisms that mitigate it. The results suggest that truth commissions without HRV coverage may offer a pathway to elite pacification that does not generate the Local Arena costs associated with impunity. More broadly, the findings underscore the importance of evaluating transitional justice against multiple outcomes—and of centering the security of ordinary citizens, not just the calculations of armed elites, in post-conflict policy design.

Future research should extend this analysis in several directions. First, disaggregating amnesties by design features—scope, conditionality, beneficiary configuration—could identify whether specific provisions moderate the dual-arena trade-off. Second, and perhaps most critically, subnational analysis could examine whether Local Arena effects are concentrated in specific regions rather than nationally diffuse. Comparative case studies pairing national-level findings with municipal or departmental homicide data—for instance, comparing violence trajectories in former conflict zones versus unaffected areas within the same country—would reveal whether the impunity signal operates uniformly or targets communities most exposed to demobilized combatants and wartime networks. Such subnational approaches could also identify local factors that amplify or mitigate the dual-arena trade-off, offering more precise guidance for post-conflict security programming. Third, direct measurement of rule of law perceptions through survey data could provide evidence on the signaling mecha-

nism. Finally, process-tracing case studies could illuminate the micro-level dynamics through which impunity signals translate into increased societal violence—tracing, for example, how specific amnesty beneficiaries transition from political to criminal violence.

The stakes of this research agenda are high. Armed conflicts continue to end through negotiated settlements, and amnesties remain common components of peace agreements—even when not explicitly labeled as such. Increasingly, HRV amnesties are embedded within broader transitional justice packages that frame impunity as "restorative justice" or "reconciliation," obscuring the trade-offs documented here. Moreover, amnesties are not always negotiated at the moment of transition; in several cases, HRV coverage has been extended years into the post-conflict period as political pressures mount or as former combatants leverage residual power to secure retrospective immunity. These "hidden" and delayed amnesties pose particular challenges for accountability advocates and complicate efforts to design transitional justice frameworks that balance elite pacification with community security. Understanding the full consequences of these choices—whether granted openly at the negotiating table or quietly enacted years later—is essential for designing transitions that deliver peace not only to negotiating parties but to the communities they claim to represent.

Acknowledgments

yses were conducted using Stata 19 (StataCorp LLC, College Station, TX).

Table 1: Summary Statistics

	Observaations	Mean	Standard Deviation
Conflict-related deaths (rate)	2992	9.63	12.852
Homicides (rate)	2992	15.65	192.9
HRV Amnesty (yes=1)	2992	.235	.424
Truth Commission (yes=1)	2992	.324	.468
GDP per capita (constant USD-2021)	2763	3237.2	6232.8
Dyad duration (years)	2992	11.61	13.32
Ongoing another dyad (yes=1)	2992	.456	.498
Post conflict first year	2992	2007.4	9.46
Peace accords (yes=1)	2992	.308	.462
Country	68		
HRV Amnesty (countries)	16		
Truth Com. Non-HRV Amnesty (countries)	17		

Table 2: **Descriptive Statistics: Amnesty vs. Control Group (Model A)**

	Control Group (Never/Not Yet Treated)		**Amnesty (Pre-Treatment)** ($t < 0$)		**Amnesty (Post-Treatment)** ($t \geq 0$)	
Variable	Mean	SD	Mean	SD	Mean	SD
Outcome Variables						
Log Homicide Rate	1.59	(0.98)	1.95	(0.94)	2.34	(1.12)
Log Conflict Deaths Rate	-0.50	(2.09)	0.64	(2.64)	-1.35	(1.36)
Covariates						
Log GDP per Capita	7.23	(1.41)	6.26	(0.72)	7.44	(0.77)
War Duration (Years)	11.10	(14.03)	15.13	(12.12)	-	-
Contextual Factors						
Truth Commission	0.33	(0.47)	-	-	0.37	(0.48)
Negotiated Peace	0.17	(0.38)	-	-	0.79	(0.41)
Observations	2,288		335		369	

Notes: Table displays unadjusted means and standard deviations (in parentheses). "Pre-Treatment" refers to years prior to amnesty adoption, and "Post-Treatment" refers to years following adoption. Log variables are measured as $\ln(x + 1)$.

Table 3: **Descriptive Statistics: Mechanism Test (Model B)**

	Control (Pure)	Accountability Group (TC Only)		Impunity Group (HRV Amnesty)	
	All Years	Pre ($t < 0$)	Post ($t \geq 0$)	Pre ($t < 0$)	Post ($t \geq 0$)
Outcome Variables					
Log Interpersonal Violence	1.43	2.12	1.41	1.95	2.34
	(0.97)	(0.85)	(0.92)	(0.94)	(1.12)
Log Conflict Violence	-0.80	0.39	-0.69	0.64	-1.35
	(1.99)	(2.28)	(1.61)	(2.64)	(1.36)
Covariates					
Log GDP per Capita	7.50	6.38	7.49	6.26	7.44
	(1.42)	(1.13)	(1.10)	(0.72)	(0.77)
War Duration (Years)	9.57	14.35	-	15.13	-
	(14.03)	(13.73)	-	(12.12)	-
Contextual Factors					
Truth Commission	-	-	-	-	0.37
	-	-	-	-	(0.48)
Negotiated Peace	0.06	-	0.47	-	0.79
	(0.23)	-	(0.50)	-	(0.41)
Total Countries	35	17		16	
Observations	1,540	548	200	335	369

Notes: Table compares the "Pure Control" group (no TC, no Amnesty) against the "Accountability" treatment (TC without Amnesty) and the "Impunity" reference group (HRV Amnesty). Means are reported with standard deviations in parentheses. The divergence in post-treatment interpersonal violence highlights the mechanism effect.

Table 4: **Main Results: The Effect of HR Amnesties on Violence**

	Model A1: Unconditional (No Covariates)		Model A2: Conditional (Duration + Log GDP pc)	
	Conflict	Interpersonal	Conflict	Interpersonal
ATT (Post-Avg)	**-1.749***	**0.096***	**-1.7***	**0.119***
	(0.519)	(0.056)	(0.526)	(0.054)
Dynamic Effects				
Year $t = 1$	-1.259**	0.041*	-1.314**	0.040*
Year $t = 2$	-1.396***	0.080*	-1.492***	0.093**
Year $t = 3$	-1.718***	0.042	-1.782***	0.059
Year $t = 5$	-2.045***	0.048	-2.034***	0.079*
Year $t = 10$	-1.899***	0.097	-1.788***	0.122
Year $t = 15$	-1.384**	0.242**	-1.273*	0.271**
Treatment Group		($N = 16$)		
Control Group		($N = 52$)		
Observations	2,992	2,992	2,762	2,762

Notes: Callaway and Santana (2021) CSDID estimates. Model A2 serves as the main benchmark, conditioning on *War Duration* and *Log GDP per Capita* to address selection bias and state capacity. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: **Mechanism Test: Impunity (B1) vs. Accountability (B2) using Pure Controls**

	Treatment: HRV Amnesty (Impunity) (Model B1: Conditional)		Treatment: Truth Commission (Accountability) (Model B2: Conditional)	
	Conflict	**Interpersonal**	**Conflict**	**Interpersonal**
ATT (Post-Avg)	**-2.086***	**0.104**	**-1.663****	**-0.171**
	(0.522)	(0.064)	(0.774)	(0.159)
Dynamic Effects				
Year $t = 1$	-1.551**	0.037	-0.468	-0.086*
Year $t = 2$	-1.372***	0.074	-0.743*	-0.070
Year $t = 5$	-2.158***	0.063	-1.130*	-0.111
Year $t = 10$	-2.048***	0.092	-1.878*	-0.243
Year $t = 15$	-1.690**	0.242*	-1.390*	-0.008
HRV Amnesty Group			($N = 16$)	
Truth Commission Group			($N = 17$)	
Control Group			($N = 35$)	
Observations	2067	2067	2032	2032
Covariates		War Duration, Log GDP pc		
Control Group		Pure Controls Only (No Amnesty & No TC)		

Notes: Comparison of two distinct transitional justice mechanisms against a "Pure Control" baseline ($N = 34$). Panel B1 (HRV Amnesty) shows a long-term increase in interpersonal violence, while Panel B2 (Truth Commissions) shows no such increase, refuting the argument that general state weakness drives post-conflict crime. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6: **Robustness Check: Spatial Exclusion ("Donut" DiD)**

	Restricted Sample (Non-Neighbors Only) ($N = 2,050$)	
	Elite Arena (Log Conflict)	**Local Arena** (Log Interpersonal)
ATT (Post-Avg)	**-1.585***	**0.130**
	(0.542)	(0.055)
Dynamic Effects		
Year $t = 1$	-1.190**	0.046**
Year $t = 5$	-1.932***	0.079
Year $t = 10$	-1.734***	0.150**
Year $t = 15$	-1.106	0.302**
Control Group	Distant Controls Only (No Neighbors)	
Controls	War Duration, Log GDP pc	

Notes: Estimates derived from a "Donut" Difference-in-Differences design excluding all immediate geographic neighbors of treated units to address spatial spillover. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 8: Cases used in the sample

Country	Postconf. Year	Amnesty Year	Truth Com. Year	HRV Amnest	Peace Accord	Dyads	Another Dyad
South Africa	1989	1989	1995	Yes	Yes	SWAPO, ANC	No
Morocco	1989	1991	No	No	No	POLISARIO	No
Nicaragua	1990	1990	No	Yes	Yes	Contras	No
Lao People's	1990	1991	No	No	No	LRM	No
El Salvador	1991	1992	1992	Yes	Yes	FMLN	No
Spain	1991	No	No	No	No	ETA	No
Moldova	1992	1992	No	Yes	No	PMR	No
Guatemala	1995	1996	1997	Yes	Yes	URNG	No
Azerbaijan	1995	No	No	No	No	OPON Forces	Yes
Bosnia & Herz	1995	1998	2003	No	Yes	Serbian irregu	No
Croatia	1995	1996	No	No	No	Serbian Repu	No
Papua New G.	1996	1998	No	Yes	Yes	BRA	No

Table 8 – Continued from previous page

Country	Postconf. Year	Amnesty Year	Truth Com. Year	HRV Amnest	Peace Accord	Dyads	Another Dyad
Mexico	1996	1999	No	No	No	EZLN,EPR	No
Cambodia	1998	No	No	No	No	KR	No
United Kingdom	1998	No	No	No	No	RIRA	No
Djibouti	1999	2000	No	Yes	Yes	FRUD	No
Serbia	1999	No	No	No	No	UCK	No
Sierra Leone	2001	1999	2000	Yes	Yes	RUF	No
Guinea	2001	No	No	No	No	RFDG	No
North Maced.	2001	2001	No	No	Yes	UCK	No
Senegal	2003	2004	No	Yes	No	MFDC	Yes
Algeria	2003	No	2003	No	No	GIA	Yes
Eritrea	2003	No	No	No	No	EIJM - AS	No
Liberia	2003	2005	2005	No	Yes	LURD, MODE	No
Nigeria	2004	2004	No	Yes	Yes	NDPVF, Ahlul	Yes
Israel	2004	No	No	No	No	AMB	Yes
Uzbekistan	2004	No	No	No	No	IMU, JIG	No
Indonesia	2005	2005	No	Yes	Yes	GAM	Yes
Nepal	2006	2006	2015	Yes	Yes	CPN-M	No
Burundi	2008	2006	2014	No	Yes	Palipehutu-F	Yes
Georgia	2008	No	No	No	No	Republic of S	No
Niger	2008	No	No	No	No	MNJ	Yes
Angola	2009	2006	No	Yes	Yes	FLEC-FAC	No
Chad	2009	2009	No	Yes	No	AN, UFR	Yes
Sri Lanka	2009	2009	2010	No	No	LTTE	No
Peru	2010	No	2001	No	No	Sendero Lum	No
Iraq	2011	No	No	No	No	Ansar al-Islan	Yes
Ivory Coast	2011	No	2011	No	No	FRCI, FDSI-C	No
Tajikistan	2011	2012	No	No	No	IMU, Forces	No
Afghanistan	2013	2016	No	Yes	No	Hizb-i Islami-y	Yes
Malaysia	2013	No	No	No	No	Sultanate of	No
Myanmar	2013	No	2018	No	No	RCSS	Yes
Colombia	2015	2016	2017	No	Yes	FARC	Yes
Russian Fed.	2015	No	No	No	No	Forces of the	Yes
Syria	2015	No	No	No	No	PYD	Yes
Ukraine	2015	No	No	No	No	United Armed	Yes

Table 8 – Continued from previous page

Country	Postconf. Year	Amnesty Year	Truth Com. Year	HRV Amnest	Peace Accord	Dyads	Another Dyad
Mozambique	2016	2019	No	Yes	Yes	Renamo	No
Congo	2016	No	No	No	No	Ntsiloulous	No
Ethiopia	2016	No	No	No	No	ONLF, OLF	Yes
Pakistan	2016	2015	No	No	No	BLA, BRA, B	Yes
Philippines	2016	2014	2014	No	Yes	MILF, BIFM	Yes
Tunisia	2016	No	No	No	No	AQIM, JAK-T	No
Turkey	2016	No	No	No	No	TAK, IS, Yurt	Yes
Bangladesh	2017	No	No	No	No	IS	No
Lebanon	2017	No	2005	No	No	IS	No
Mali	2017	No	2015	No	No	AQIM, Ansar	Yes
Sudan	2017	2020	No	No	Yes	SRF, SARC	Yes
Uganda	2017	No	1986	No	No	ADF	No
Central Africa	2018	2019	2019	No	Yes	anti-Balaka	Yes
DR of the Congo	2018	No	2003	No	No	Kamuina Nsa	Yes
India	2018	No	No	No	No	UNLFW	Yes
Iran	2018	No	No	No	No	KDPI	Yes
Somalia	2018	No	No	No	No	Republic of S	Yes
South Sudan	2018	2018	2018	No	Yes	SPLM/A-IO	Yes
Yemen	2018	No	No	No	No	AQAP, GPC	Yes
Egypt	2019	No	No	No	No	Harakit Sawa'	Yes
Libya	2020	No	No	No	No	Forces of the House	No
Rwanda	2020	No	1993	No	No	FDLR-RUD R	Yes

Table 7: **Robustness Check: Spatial Exclusion ("Donut" Design)**

	Elite Arena (Log Conflict Violence)		Local Arena (Log Interpersonal Violence)	
	Coeff.	SE	Coeff.	SE
Panel A: Impunity Model (HRV Amnesty)				
Comparison: Amnesty vs. Distant Controls (N = 2,050)				
ATT (Post-Average)	**-1.585*****	(0.542)	**0.130****	(0.055)
Dynamic Snapshots				
Year $t = 1$	-1.190**	(0.587)	0.046**	(0.022)
Year $t = 5$	-1.932***	(0.553)	0.079	(0.051)
Year $t = 10$	-1.734***	(0.667)	0.150**	(0.074)
Panel B: Accountability Model (TC without Amnesty)				
Comparison: TC vs. Distant Controls (N = 1,273)				
ATT (Post-Average)	-0.770	(0.570)	**-0.031**	(0.110)
Dynamic Snapshots				
Year $t = 1$	-0.635	(0.523)	-0.030	(0.032)
Year $t = 5$	-0.467	(0.563)	-0.030	(0.105)
Year $t = 10$	-0.923	(0.913)	0.013	(0.157)
Control Group	Restricted: Non-Neighbors Only (17 Neighbors Dropped)			
Covariates	Log GDP per Capita, War Duration			

Notes: Estimates derived from a "Donut" Difference-in-Differences estimator using Doubly Robust Inverse
Probability Weighting (DRIPW). All immediate geographic neighbors of treated units are excluded from
the control pool to satisfy SUTVA. Panel A confirms the trade-off (Peace/Crime) under impunity. Panel B
confirms the absence of a crime effect under accountability. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

References

Alvarez, G. (2025). The Economic Spoiler's Incentive: How Consent-Based Counternarcotics Policy Fueled Violence in Post-Conflict Colombia. *Studies in Conflict & Terrorism, Forthcoming.* https://doi.org/10.1080/1057610X.2025.2580952

Ansorg, N., & Kurtenbach, S. (2023). Promoting Peace and Impunity? Amnesty Laws After War in El Salvador and Beyond. *Journal of Global Security Studies, 8*(2). https://doi.org/10.1093/jogss/ogad010

Archer, D., & Gartner, R. (1976). Violent Acts and Violent Times: A Comparative Approach to Postwar Homicide Rates. *American Sociological Review, 41*(6), 937–963. https://doi.org/10.2307/2094796

Archer, D., & Gartner, R. (1984). *Violence and crime in cross-national perspective.* Yale University Press.

Autesserre, S. (2010). *The Trouble with the Congo: Local Violence and the Failure of International Peacebuilding.* Cambridge University Press. https://doi.org/10.1017/CBO9780511761034

Autesserre, S. (2014). *Peaceland: Conflict Resolution and the Everyday Politics of International Intervention.* Cambridge University Press. https://doi.org/10.1017/CBO9781107280366

Balcells, L., & Kalyvas, S. N. (2025). Ideology and Revolution in Civil Wars: The "Marxist Paradox". *American Political Science Review,* 1–19. https://doi.org/10.1017/S0003055425000188

Bank, W. (2025). World Bank Open Data.

Bara, C., Deglow, A., & van Baalen, S. (2021). Civil war recurrence and postwar violence: Toward an integrated research agenda. *European Journal of International Relations, 27*(3), 913–935. https://doi.org/10.1177/13540661211006443

Belgorodski, E. (2024). Situating Reparations for Ukraine within a Broader Transitional Justice Process. *International Journal of Transitional Justice*, *18*(3), 405–421. https://doi.org/10.1093/ijtj/ijae028

Bell, C., & Pospisil, J. (2017). Navigating Inclusion in Transitions from Conflict: The Formalised Political Unsettlement. *Journal of International Development*, *29*(5), 576–593. https://doi.org/10.1002/jid.3283

Bellamy, A. J. (2012, September). Introduction. In A. J. Bellamy (Ed.), *Massacres and Morality: Mass Atrocities in an Age of Civilian Immunity* (p. 0). Oxford University Press. https://doi.org/10.1093/acprof:oso/9780199288427.003.0001

Binningsbø, H. M., & Nordås, R. (2022). Conflict-Related Sexual Violence and the Perils of Impunity. *Journal of Conflict Resolution*, *66*(6), 1066–1090. https://doi.org/10.1177/00220027221078330

Bourgois, P. (2001). The Power of Violence in War and Peace: Post-Cold War Lessons from El Salvador. *Ethnography*, *2*(1), 5–34. https://doi.org/10.1177/14661380122230803

Bunselmeyer, E., & Schulz, P. (2020). Quasi-Experimental Research Designs as a Tool for Assessing the Impact of Transitional Justice Instruments. *International Journal of Human Rights*, *24*(5), 688–709.

Butts, K. (2023a). JUE Insight: Difference-in-differences with geocoded microdata. *Journal of Urban Economics*, *133*, 103493. https://doi.org/10.1016/j.jue.2022.103493

Butts, K. (2023b, June). Difference-in-Differences Estimation with Spatial Spillovers. https://doi.org/10.48550/arXiv.2105.03737

Call, C., & Wyeth, V. (2008). *Building states to build peace.* Lynne Rienner Publishers.

Callaway, B., & Sant'Anna, P. H. C. (2021). Difference-in-Differences with multiple time periods. *Journal of Econometrics*, *225*(2), 200–230. https://doi.org/10.1016/j.jeconom.2020.12.001

Campbell, S., & Spilker, G. (2021). Aiding War or Peace? The insiders' view on aid to post-conflict transitions. https://doi.org/10.2139/ssrn.3576116

Collier, P., Havard, H., Elliott, V. L., & Hegre. (2003). *Breaking the conflict trap : Civil war and development policy.* World Bank.

Collier, P., & Hoeffler, A. (2004). Greed and Grievance in Civil War. *Oxford Economic Papers, 56*(4), 563–595.

Collier, P., Hoeffler, A., & Rohner, D. (2009). Beyond greed and grievance: Feasibility and civil war. *Oxford Economic Papers, 61*(1), 1–27. https://doi.org/10.1093/oep/gpn029

Collier, P., Hoeffler, A., & Söderbom, M. (2008). Post-Conflict Risks. *Journal of Peace Research, 45*(4), 461–478. https://doi.org/10.1177/0022343308091356

Cruz, J. M. (2011). Criminal Violence and Democratization in Central America: The Survival of the Violent State. *Latin American Politics and Society, 53*(4), 1–33. https://doi.org/10.1111/j.1548-2456.2011.00132.x

Dancy, G. (2018). Deals with the Devil? Conflict Amnesties, Civil War, and Sustainable Peace. *International Organization, 72*(2), 387–421.

Dancy, G., Marchesi, B. E., Olsen, T. D., Payne, L. A., Reiter, A. G., & Sikkink, K. (2019). Behind Bars and Bargains: New Findings on Transitional Justice in Emerging Democracies. *International Studies Quarterly, 63*(1), 99–110.

Dancy, G., Thoms, O. T., Pham, P., Sikkink, K., & Vinck, P. (2025). Introducing the Transitional Justice Evaluation Tools (TJET) database. *Journal of Peace Research, 62*(7), 2481–2491. https://doi.org/10.1177/00223433251360200

Dancy, G., & Wiebelhaus-Brahm, E. (2018). The impact of criminal prosecutions during intrastate conflict. *Journal of Peace Research, 55*(1), 47–61. https://doi.org/10.1177/0022343317732614

Davies, S., Pettersson, T., Sollenberg, M., & Öberg, M. (2025). Organized violence 1989–2024, and the challenges of identifying civilian victims. *Journal of Peace Research, 62*(4).

Davies, S., Engström, G., Pettersson, T., & Öberg, M. (2024). Organized violence 1989–2023, and the prevalence of organized crime groups. *Journal of Peace Research*, *61*(4), 673–693. https://doi.org/10.1177/00223433241262912

De Greiff, P. (Ed.). (2006, March). *Then Handbook of Reparations*. Oxford University Press. https://doi.org/10.1093/0199291926.003.0001

De Greiff, P. (2012). *Report of the Special Rapporteur on the Promotion of Truth, Justice, Reparation and Guarantees of Non-Recurrence* (tech. rep. No. A_HRC_21_46-EN).

Elster, J. (2004). *Closing the Books: Transitional Justice in Historical Perspective*. Cambridge University Press. https://doi.org/10.1017/CBO9780511607011

Fearon, J. D., & Laitin, D. D. (2003). Ethnicity, Insurgency, and Civil War. *American Political Science Review*, *97*(1), 75–90. https://doi.org/10.1017/S0003055403000534

Figari Layus, R., & Vargas Trujillo, J. (2023). The 'Domino Effect' of Ongoing Violence on Transitional Justice: The Case of Colombia's Special Jurisdiction for Peace. *International Journal of Transitional Justice*, *17*(3), 435–452. https://doi.org/10.1093/ijtj/ijad023

Finkelstein, C. O., Fuller, C. J., Ohlin, J. D., & Regan, M. C. (2023). *Between crime and war : Hybrid legal frameworks for asymmetric conflict*. Oxford University Press.

Galtung, J. (1969). Violence, Peace, and Peace Research. *Journal of Peace Research*, *6*(3), 167–191. https://doi.org/10.1177/002234336900600301

Geoff, D., Pham, P., Sikkink, K., & Vinck, P. (2024). Database of Transitional Justice Mechanisms, 1970–2020.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, *225*(2), 254–277. https://doi.org/10.1016/j.jeconom.2021.03.014

Government-of-Colombia & FARC-EP. (2016, November). Final Agreement to End the Armed Conflict and Build a Stable and Lasting Peace.

Haidar, L. (2022). The Emergence of the Mafia in Post-War Syria: The Terror-Crime Continuum. *Studies in Conflict & Terrorism*, *45*(5-6), 429–444. https://doi.org/10.1080/1057610X.2019.1678869

Hayner, P. B. (2010). *Unspeakable Truths: Transitional Justice and the Challenge of Truth Commissions*. Taylor & Francis Group.

Hendrix, C. S. (2010). Measuring state capacity: Theoretical and empirical implications for the study of civil conflict. *Journal of Peace Research*, *47*(3), 273–285. https://doi.org/10.1177/0022343310361838

IHME. (2025). GBD Results.

JEP. (2025, September). Macrocaso Caso 001.

Kalyvas, S. N. (2006). *The Logic of Violence in Civil War*. Cambridge University Press.

Kao, K., & Revkin, M. R. (2023). Retribution or Reconciliation? Post-Conflict Attitudes toward Enemy Collaborators. *American Journal of Political Science*, *67*(2), 358–373. https://doi.org/10.1111/ajps.12673

Loyle, C. E. (2025). *Escaping Justice. Impunity for State Crimes in the Age of Accountability*. Cambridge University Press & Assessment.

Loyle, C. E., & Binningsbø, H. M. (2018). Justice during Armed Conflict: A New Dataset on Government and Rebel Strategies. *The Journal of Conflict Resolution*, *62*(2), 442–466.

Mac Ginty, R., & Richmond, O. P. (2013). The Local Turn in Peace Building: A critical agenda for peace. *Third World Quarterly*, *34*(5), 763–783. https://doi.org/10.1080/01436597.2013.800750

Mallinder, L. (2007). Can Amnesties and International Justice be Reconciled? *International Journal of Transitional Justice*, *1*(2), 208–230. https://doi.org/10.1093/ijtj/ijm020

Mallinder, L. (2008). *Amnesty, human rights and political transitions : Bridging the peace and justice divide*. Hart Pub.

Meernik, J. (2005). Justice and Peace? How the International Criminal Tribunal Affects Societal Peace in Bosnia. *Journal of Peace Research, 42*(3), 271–289. https://doi.org/10.1177/0022343305052012

Moe, T. M. (2005). Power and Political Institutions. *Perspectives on Politics, 3*(2), 215–233. https://doi.org/10.1017/S1537592705050176

Muggah, R., & Krause, K. (2009). Closing the Gap Between Peace Operations and Post-Conflict Insecurity: Towards a Violence Reduction Agenda. *International Peacekeeping, 16*(1), 136–150. https://doi.org/10.1080/13533310802485617

Neumann, J. v., & Morgenstern, O. (2007, March). *Theory of Games and Economic Behavior: 60th Anniversary Commemorative Edition.* Princeton University Press. https://doi.org/10.1515/9781400829460

North, D. C., Wallis, J. J., & Weingast, B. R. (2009). *Violence and Social Orders: A Conceptual Framework for Interpreting Recorded Human History.* Cambridge University Press. https://doi.org/10.1017/CBO9780511575839

Olsen, T. D., Payne, L. A., & Reiter, A. G. (2010). *Transitional justice in balance : Comparing processes, weighing efficacy.* Washington, DC : U.S. Institute of Peace.

Pearce, J. V., & Dietrich, W. (2019). Many violences, many peaces: Wolfgang Dietrich and Jenny Pearce in conversation. *Peacebuilding, 7*(3), 268–282. https://doi.org/10.1080/21647259.2019.1632056

Pérez, O. J. (2015). *Civil-military relations in post-conflict societies : Transforming the role of the military in Central America.* Routledge.

Pinker, S. (2018). *Enlightenment now : The case for reason, science, humanism, and progress.* Viking, an imprint of Penguin Random House LLC.

Porter, H. (2016). *After Rape: Violence, Justice, and Social Harmony in Uganda.* Cambridge University Press. https://doi.org/10.1017/9781316841297

Quinn, J. R. (2020). Tractionless Transitional Justice in Uganda: The Potential for Thin Sympathetic Interventions as Ameliorating Factor. In *Transitional Justice in Com-*

parative Perspective: Preconditions for Success. Springer International Publishing AG.

Roht-Arriaza, N. (2016, March). Measures of Non-Repetition in Transitional Justice: The Missing Link? https://doi.org/10.2139/ssrn.2746226

Roth, J., Sant'Anna, P. H. C., Bilinski, A., & Poe, J. (2023). What's trending in difference-in-differences? A synthesis of the recent econometrics literature. *Journal of Econometrics*, *235*(2), 2218–2244. https://doi.org/10.1016/j.jeconom.2023.03.008

Sanín, F. G. (2004). Criminal Rebels? A Discussion of Civil War and Criminality from the Colombian Experience. *Politics & Society*, *32*(2), 257–285. https://doi.org/10.1177/0032329204263074

Sant'Anna, P. H. C., & Zhao, J. (2020). Doubly robust difference-in-differences estimators. *Journal of Econometrics*, *219*(1), 101–122. https://doi.org/10.1016/j.jeconom.2020.06.003

Sikkink, K. (2011). *The justice cascade : How human rights prosecutions are changing world politics.* W. W. Norton & Co.

Skaar, E. (2011). *Judicial Independence and Human Rights in Latin America.* Palgrave Macmillan US. https://doi.org/10.1057/9780230117693

Snyder, J., & Vinjamuri, L. (2003). Trials and Errors: Principle and Pragmatism in Strategies of International Justice. *International Security*, *28*(3), 5–44.

Sriram, C., García-Godos, J., Herman, J., & Martin-Ortega, O. (2012). *Transitional Justice and Peacebuilding on the Ground: Victims and Ex-Combatants.* Taylor & Francis Group.

Steenkamp, C. (2005). The Legacy of War: Conceptualizing a 'Culture of Violence' to Explain Violence after Peace Accords. *The Round Table*, *94*(379), 253–267. https://doi.org/10.1080/00358530500082775

Steenkamp, C. (2022). Post-accord Crime and Violence. In R. Mac Ginty & A. Wanis-St. John (Eds.), *Contemporary Peacemaking: Peace Processes, Peacebuilding and*

Conflict (pp. 533–562). Springer International Publishing. https://doi.org/10.1007/978-3-030-82962-9_25

Teitel, R. G. (2000). *Transitional Justice.* Oxford University Press, Incorporated.

Teitel, R. G. (2014, May). *Globalizing Transitional Justice: Contemporary Essays.* Oxford University Press. https://doi.org/10.1093/acprof:oso/9780195394948.001.0001

Tiscornia, L. (2024). Police reform in the aftermath of armed conflict: How militarization and accountability affect police violence. *Journal of Peace Research, 61*(3), 383–397. https://doi.org/10.1177/00223433221128846

Trejo, G., Albarracín, J., & Tiscornia, L. (2018). Breaking state impunity in post-authoritarian regimes: Why transitional justice processes deter criminal violence in new democracies. *Journal of Peace Research, 55*(6), 787–809. https://doi.org/10.1177/0022343318793480

Tyler, T. R. (2021). *Why People Obey the Law.* Princeton University Press.

UNSC. (2004). *The Rule of Law and Transitional Justice in Conflict and post-Conflict Societies* (Report of the Secretary-General No. S/2004/616). United Nations Security Council.

USDS. (2023). *Country Reports on terrorism 2022* (tech. rep.). U.S. Department of State.

Voyvodic, C. (2021). The legacy of rebel order: Local (in)security in Colombia. *Conflict, Security & Development, 21*(2), 177–197. https://doi.org/10.1080/14678802.2021.1911065

Walter, B. F. (2015). Why Bad Governance Leads to Repeat Civil War. *Journal of Conflict Resolution, 59*(7), 1242–1272. https://doi.org/10.1177/0022002714528006

Watch, H. R. (2024, August). Nepal: New Transitional Justice Law a Flawed Step Forward.

Weber, M., 1864-1920. (1965). *Politics as a vocation.* Fortress Press.

Wood, E. J. (2009). Armed Groups and Sexual Violence: When Is Wartime Rape Rare? *Politics & Society, 37*(1), 131–161. https://doi.org/10.1177/0032329208329755