Legislating Peace? Anti-Open Grazing Laws and Farmer-Herder Violence in Nigeria

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March 9, 2026

Abstract

Farmer-herder violence kills thousands annually in Nigeria, yet causal evidence on policy responses remains scarce. I exploit staggered adoption of anti-open grazing laws across 14 Nigerian states in a triple-difference design, comparing pastoral versus non-pastoral local government areas within treated versus untreated states. Using georeferenced UCDP conflict data covering 775 LGAs over 15 years, I find that anti-grazing laws reduce non-state violence in pastoral zones by 0.48 events per LGA-year ($p = 0.003$), a 79% decline. Deaths fall by 2.13 per LGA-year. The coefficient is identified from 12 states containing both pastoral and non-pastoral LGAs. A within-state displacement test finds no detectable violence increase in non-pastoral areas, consistent with deterrence. Results are robust to leave-one-out analysis and restriction to quasi-exogenous Southern Governors' Forum adopters.

JEL Codes: D74, K42, O13, Q15

Keywords: farmer-herder conflict, anti-grazing laws, Nigeria, triple-difference, pastoralism

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1. Introduction

In January 2018, Fulani herdsmen attacked farming communities in Benue State, Nigeria, killing 73 people in a single weekend. The massacre—occurring just weeks after Benue implemented Nigeria's most prominent anti-open grazing law—crystallized a policy puzzle that affects millions: can legislation resolve conflicts rooted in centuries-old competition over land, water, and grazing resources? Between 2015 and 2024, farmer-herder violence in Nigeria killed more people than the Boko Haram insurgency in several states, making it one of West Africa's most lethal sources of communal violence (International Crisis Group, 2017, 2018).

The farmer-herder crisis emerges from a fundamental tension between sedentary agriculture and mobile pastoralism. As populations grow and climate change shrinks the Sahel, Fulani herders push southward into farming communities, destroying crops and provoking lethal retaliation (Olaniyan and Yahaya, 2015; Benjaminsen and Ba, 2009). Traditional conflict-resolution institutions—village elders, grazing reserves, transhumance corridors—have eroded under pressure from urbanization, population growth, and political manipulation (Moritz, 2010; Turner, 2011). The result is a governance vacuum that state and federal governments have struggled to fill.

Beginning with Ekiti State in August 2016, Nigerian states have attempted a legislative solution: anti-open grazing laws that prohibit the free roaming of livestock, require herders to use designated ranches or reserves, and impose criminal penalties for violations. By 2021, fourteen states had adopted such laws, with a major wave following a coordinated resolution by the Southern Governors' Forum (SGF) in mid-2021 (Ajala, 2020; Ochonu, 2022). The laws represent a significant departure from laissez-faire pastoralism and a direct challenge to the economic model of millions of nomadic herders.

Despite their prominence in Nigerian political discourse, the causal effect of anti-grazing laws on violence remains unknown. Qualitative accounts offer contradictory assessments: proponents argue the laws reduce crop destruction and the retaliatory violence it triggers, while critics contend they inflame tensions by criminalizing herders' livelihoods and pushing them toward more remote, ungoverned areas (Akinyetun, 2021; Adelakun and Abubakar, 2023). Descriptive studies document implementation challenges without quantifying effects on violence (Ajala, 2020). No study has applied modern causal inference methods to estimate the laws' impact on conflict.

This paper fills this gap using a triple-difference (DDD) design that exploits three dimensions of variation. The first difference compares states that adopted anti-grazing laws to those that did not, leveraging staggered adoption between 2016 and 2021 across 14 treated states. The second difference distinguishes local government areas (LGAs) classified as pastoral—those with pre-existing farmer-herder conflict or located along major transhumance corridors—from non-pastoral LGAs within the same state. The third difference is temporal: before versus after each state's law adoption. The key identifying assumption is that the pastoral–non-pastoral violence gap would have evolved similarly in treated and untreated states absent the law. State-by-year fixed effects absorb all state-level time-varying confounders, including macroeconomic shocks, gubernatorial transitions, and security operations.

I combine georeferenced conflict data from the Uppsala Conflict Data Program (UCDP GED v25.1), which records the precise location, date, and fatality count of every violent event in Nigeria from 1990 to 2024 (Sundberg and Melander, 2013; Pettersson et al., 2023), with administrative boundaries from GADM v4.1 to construct a balanced panel of 775 LGAs observed annually from 2010 to 2024. I classify 193 LGAs (24.9%) as pastoral based on pre-treatment non-state violence patterns and location along known transhumance corridors through the Middle Belt.

The preferred specification—DDD with LGA and state-by-year fixed effects—shows that anti-grazing laws reduce non-state violence in pastoral LGAs by nearly half an event per year ($\beta = -0.480$, $p = 0.003$), a roughly 79% decline relative to the pre-treatment mean. The effect on fatalities is larger: deaths from non-state violence fall by 2.13 per LGA-year ($p < 0.001$), suggesting the laws prevent particularly lethal confrontations.

Several findings bolster the causal interpretation. First, placebo tests using state-based violence (primarily Boko Haram activity) and one-sided violence show null effects, confirming that the laws specifically affect the type of conflict they target. Second, a Callaway-Sant'Anna event study reveals no significant pre-treatment trends, supporting the parallel trends assumption. Third, a within-state displacement test—comparing non-pastoral LGAs in treated states before and after law adoption—finds no detectable increase in violence ($\beta = 0.036$, $p = 0.24$), consistent with deterrence rather than spatial displacement of violence from pastoral to non-pastoral zones. Fourth, leave-one-state-out analysis produces a tight range of estimates (-0.546 to -0.292), demonstrating that no single state drives the finding. Fifth, restricting the treated group to the seven states that adopted laws following the SGF resolution—a collective political decision less correlated with local violence levels—yields a slightly larger estimate (-0.546, $p = 0.009$), mitigating concerns about endogenous adoption. Sixth, an effective sample analysis reveals that 12 of 37 states (6 treated, 6 control) provide identifying within-state variation between pastoral and non-pastoral LGAs. The estimate is therefore local to mixed-composition states rather than a national average, though the contributing states span both early adopters and the SGF wave.

This paper contributes to three literatures. First, it provides the first causal estimate of anti-grazing legislation on farmer-herder violence, advancing a growing qualitative literature on institutional responses to pastoral conflict in Nigeria (Ajala, 2020; Ochonu, 2022; Akinyetun, 2021). While prior work documents the political economy of law adoption and implementation challenges, no study has credibly estimated the laws' effect on violence. The triple-difference design, combined with placebo tests and randomization inference, offers identification that prior descriptive analyses could not achieve.

Second, it contributes to the economics of conflict by demonstrating that domestic legislation is associated with reduced communal violence in a weak-state setting. The canonical economics literature on conflict focuses on economic shocks (Miguel et al., 2004; Dube and Vargas, 2013; Bazzi and Blattman, 2014), resource competition (Hsiang et al., 2013; Burke et al., 2015), and state capacity (Besley and Persson, 2011; Dell and Querubin, 2018). Few papers examine specific legislative interventions in Sub-Saharan Africa, partly because clean identification is rare. Anti-grazing laws are fundamentally an enclosure policy—a legislated transition from open-range to enclosed livestock management, akin to the transformation studied by Hornbeck (2010) in the American context. Nigeria's staggered adoption across heterogeneous states provides unusually tractable variation for studying how property-rights regimes affect resource conflict.

Third, the paper demonstrates the value of triple-difference designs in conflict settings where standard difference-in-differences faces challenges. With only 14 treated states, conventional state-level DiD suffers from limited statistical power. By exploiting within-state heterogeneity between pastoral and non-pastoral areas, the DDD design gains precision while controlling for state-level confounders. This approach may prove valuable for evaluating other spatially heterogeneous policies in developing countries.

2. Institutional Background

2.1 The Farmer-Herder Crisis

Nigeria's farmer-herder conflict is fundamentally a resource competition between two incompatible land-use systems: sedentary crop agriculture and mobile livestock pastoralism. The conflict is concentrated in the Middle Belt, a band of states stretching from Benue and Plateau in the east to Niger and Kwara in the west, where the agricultural south meets the pastoral north (Higazi, 2016; Akov, 2017).

The Fulani, Nigeria's largest pastoral group, have practiced transhumant herding for centuries, moving their cattle seasonally between the arid north and the wetter south in search of pasture and water. Traditional institutions—grazing reserves, stock routes, and local conflict-resolution mechanisms—historically managed the interface between herders and farmers. However, several long-term trends have eroded these institutions and intensified competition.

Population growth has increased demand for both cropland and pasture. Nigeria's population nearly quadrupled from 56 million in 1970 to over 220 million by 2023, placing enormous pressure on arable land. Climate change has compounded the problem: desertification in the far north pushes herders further south, while erratic rainfall patterns increase crop failure risk for farmers, making every acre of productive land more valuable (Odoh and Chigozie, 2014; Hsiang et al., 2013). The expansion of commercial farming into erstwhile grazing reserves and the encroachment of urban areas onto stock routes have further constrained pastoral mobility (Abbass, 2012).

The conflict is extraordinarily lethal. Between 2015 and 2024, the UCDP recorded over 2,400 non-state violence events in Nigeria, with farmer-herder clashes constituting a substantial share. The International Crisis Group estimated that farmer-herder violence killed approximately 2,000 people in 2018 alone, surpassing fatalities from Boko Haram in several Middle Belt states (International Crisis Group, 2018). Attacks follow a characteristic pattern: herders' cattle destroy crops, farmers retaliate against herders, herders conduct reprisal raids, and escalation spirals outward (Dimelu et al., 2016).

The conflict also has ethnic and religious dimensions that complicate resolution. Most Fulani herders are Muslim, while farmers in the Middle Belt are predominantly Christian. Politicians have at times exploited these divisions, framing the conflict in ethno-religious terms rather than addressing its economic roots (Suberu, 2001). This politicization has made evidence-based policy evaluation particularly important yet difficult.

2.2 Anti-Open Grazing Laws

Against this backdrop, Nigerian states began adopting anti-open grazing laws starting in 2016. The laws share common features: they prohibit the unrestricted movement and grazing of livestock on farmland, require herders to use designated ranches or grazing reserves, and impose criminal penalties—including fines and imprisonment—for violations. The laws effectively seek to transform Nigeria's pastoral economy from open-range to enclosed ranching, a transition that occurred over decades in the American West but is being attempted legislatively in a matter of years.

Early adopters (2016–2019). Ekiti State, under Governor Ayodele Fayose, became the first state to pass an anti-open grazing law in August 2016. Benue State followed in November 2017 with the "Open Grazing Prohibition and Ranches Establishment Law," the most comprehensive legislation to date, which included provisions for establishing grazing reserves and a livestock guard force. Taraba (December 2017), Abia (June 2018), Ebonyi (September 2018), and Oyo (October 2019) subsequently adopted their own versions. These early adopters were states with significant farmer-herder violence, raising concerns about endogenous adoption: states may have passed laws *because* violence was already rising, making it difficult to disentangle the law's causal effect from pre-existing trends.

The SGF wave (2021). A distinct wave of adoption occurred following the Southern Governors' Forum resolution of May 2021, in which governors from all 17 southern states agreed to ban open grazing. This collective decision was primarily a political response to perceived federal inaction and northern governors' opposition, rather than a direct reaction to violence in each individual state. Between August and October 2021, seven states enacted anti-grazing laws directly implementing the SGF resolution: Ondo, Rivers, Enugu, Osun, Lagos, Delta, and Ogun. Akwa Ibom also adopted in October 2021, though its legislative process predated the SGF resolution and reflected independent state-level politics; I therefore include Akwa Ibom in the 2021 treatment cohort for the main analysis but exclude it from the quasi-exogenous SGF sub-sample. Several of the SGF states—particularly Lagos, Ogun, and Delta—had relatively low levels of farmer-herder violence, suggesting that the SGF wave provides quasi-exogenous variation: adoption was driven by collective political dynamics rather than individual states' violence trajectories.

Implementation challenges. The laws have faced significant implementation obstacles. Most treated states lack the infrastructure for ranching—fenced grazing reserves, water points, veterinary services—that would make compliance feasible. Enforcement is uneven, with rural LGAs often lacking the police presence to apprehend violators. In some states, the laws have been criticized as anti-Fulani legislation that targets a minority ethnic group's livelihood (Ochonu, 2022). Benue's experience is instructive: despite having Nigeria's most detailed law and the January 2018 implementation date, clashes continued in the months following implementation, although the state government argued they represented herder resistance to the new legal regime rather than law failure.

Mechanisms. The theoretical channels through which anti-grazing laws might affect violence are ambiguous ex ante. The *deterrence mechanism* predicts reduced violence: by criminalizing open grazing, the laws raise the expected cost of bringing cattle into farming communities, reducing crop destruction and the retaliatory cycle it triggers. The *displacement mechanism* predicts the opposite: by restricting pastoral mobility, the laws may push herders into more remote areas where state presence is weaker and informal resolution mechanisms are absent, potentially increasing violence. The *escalation mechanism* suggests laws could increase confrontational violence if herders view them as existential threats to their livelihoods and resist enforcement with force. The net effect is therefore an empirical question.

2.3 Comparison with Never-Treated States

Twenty-three states never adopted anti-grazing laws through 2024. These include northern states where pastoral interests are politically powerful (Kano, Kaduna, Sokoto, Zamfara), states with minimal pastoral activity (Bayelsa, Cross River), and Middle Belt states with significant herder-farmer conflict but insufficient political will for legislation (Plateau, Nasarawa). The heterogeneity among never-treated states strengthens the research design by providing control states across Nigeria's full geographic and political spectrum.

2.4 The Federal Dimension

The anti-grazing debate has played out against a backdrop of sharp federal-state tensions. Northern governors, representing the political base of Fulani pastoralists, have consistently opposed anti-grazing legislation, arguing that freedom of movement is constitutionally guaranteed and that ranching is economically impractical given the semi-arid conditions in many northern grazing areas. The federal government under President Muhammadu Buhari—himself a Fulani from Katsina State—initially proposed a national grazing reserve policy (RUGA) in 2019 that would have allocated federal land for pastoral settlements across all states. The proposal was abandoned after fierce opposition from southern and Middle Belt governors, who viewed it as an attempt to colonize their states' land for northern interests.

The federal impasse left the decision to the states, creating the staggered adoption timeline that the research design exploits. It also underscores that the laws were not coordinated responses to a national security strategy but rather independent (or, in the SGF case, regionally coordinated) political decisions reflecting each state's ethnic composition, gubernatorial preferences, and voter demands.

The legal framework varies across adopting states. Benue's law is the most detailed, establishing a Livestock Guard with arrest powers, creating a Ranches Establishment Committee, and specifying fines of up to 1 million naira (approximately $2,500) and imprisonment of up to five years for violations. Other states adopted shorter statutes with fewer enforcement provisions. This variation in institutional depth may contribute to heterogeneous treatment effects across states, though I cannot separately identify the "law" versus "enforcement" channels in the current design.

2.5 Historical Context of Pastoral Regulation

Nigeria's experiment with anti-grazing legislation is not without historical precedent. During the colonial period, British administrators established grazing reserves—demarcated areas where pastoral activity was permitted—particularly in the Middle Belt region. By the

1960s, over 400 grazing reserves had been gazetted, covering approximately 4 million hectares. However, decades of encroachment, administrative neglect, and conversion to cropland reduced functional reserves to a fraction of their original extent. By 2015, fewer than 10% of gazetted reserves remained operational.

The failure of the grazing reserve system is directly relevant to the anti-grazing laws' effectiveness: the new legislation often references ranch establishment as the alternative to open grazing, but the same institutional weaknesses that undermined reserves—lack of enforcement, competing land claims, inadequate infrastructure—threaten to undermine ranching as well. The empirical question is whether the criminalization of open grazing itself, independent of the provision of alternatives, deters the pastoral incursions that trigger violence.

3. Data

3.1 Conflict Data: UCDP GED v25.1

The primary outcome data come from the Uppsala Conflict Data Program's Georeferenced Event Dataset (GED) version 25.1 (Sundberg and Melander, 2013; Pettersson et al., 2023). UCDP GED records individual violent events worldwide, each coded with geographic coordinates, date, involved actors, violence type, and a "best estimate" of fatalities. I use the Nigeria subset, which contains 7,418 events between 1990 and 2024.

UCDP classifies violence into three types that map directly onto my research design. *Type 1 (state-based)* involves at least one state actor—primarily Boko Haram versus Nigerian military operations. *Type 2 (non-state)* involves violence between organized groups without state involvement—the category that captures farmer-herder clashes, ethnic militia conflicts, and communal violence. *Type 3 (one-sided)* involves organized violence against civilians. My primary outcome is Type 2 (non-state) event counts, which is the closest match to farmer-herder violence. State-based and one-sided violence serve as placebos: anti-grazing laws should not affect Boko Haram operations or targeted civilian attacks, so significant effects on these outcomes would suggest confounding.

I construct annual event counts at the LGA level by spatially joining each georeferenced event to GADM v4.1 administrative boundaries (GADM, 2024). Of 7,418 events, 7,331 (98.8%) successfully match to an LGA polygon. I also compute fatality counts using UCDP's "best estimate" of deaths per event.

3.2 Administrative Boundaries

Administrative boundaries come from the Database of Global Administrative Areas (GADM) version 4.1. Nigeria is divided into 36 states plus the Federal Capital Territory (37 units at level 1) and 775 local government areas (level 2). The boundaries provide the spatial framework for constructing the LGA-year panel and for the spatial join of UCDP events.

3.3 Treatment Assignment

I compile anti-grazing law adoption dates from legislative records, news reports, and academic sources (Ajala, 2020; Ochonu, 2022). For each treated state, I record the year and month of law passage. Table 6 reports the adoption timeline. The staggered adoption structure creates five treatment cohorts: 2016 (Ekiti), 2017 (Benue, Taraba), 2018 (Abia, Ebonyi), 2019 (Oyo), and 2021 (Ondo, Rivers, Enugu, Osun, Lagos, Delta, Ogun, Akwa Ibom).

Treatment timing convention. Because the panel is annual, I must choose when to code treatment for states that adopted mid-year. I code the treatment indicator D_{st} to equal one beginning in the *year following* adoption for states that adopted in the second half of the year (July or later), and in the adoption year for states that adopted in the first half. In practice, this means Ekiti (August 2016) is first treated in 2017, Benue and Taraba (November–December 2017) in 2018, Oyo (October 2019) in 2020, and most SGF states (August–October 2021) in 2022. Only Abia (June 2018) and Ebonyi (September 2018) are borderline; I code Abia as first treated in 2018 and Ebonyi in 2019. This convention avoids counting partial-year exposure as full treatment, though results are robust to alternative coding (treating all adoption years as the first treated year produces qualitatively identical estimates). The effective treatment cohorts in the annual panel are therefore: 2017, 2018, 2019, 2020, and 2022.

3.4 Pastoral Zone Classification

The triple-difference design requires classifying LGAs as pastoral or non-pastoral. I use a composite measure based on two sources. First, I identify LGAs with at least two non-state violence events during the pre-treatment period (2010–2015) in UCDP, using revealed conflict as a proxy for pastoral zone activity. Second, I classify all LGAs in the eight Middle Belt states—Benue, Plateau, Nasarawa, Taraba, Kaduna, Niger, Kogi, and Kwara—as pastoral, since these states lie along the primary transhumance corridors connecting northern grazing lands to southern markets. The union of these two criteria yields 193 pastoral LGAs (24.9% of 775 total). This classification is fixed using only pre-treatment data to avoid contamination by post-treatment dynamics.

3.5 Panel Construction

I construct a balanced panel of 775 LGAs observed annually from 2010 to 2024 (15 years). For each LGA-year, I compute counts of events and deaths by violence type (state-based, non-state, one-sided, and total). LGA-years with no recorded events are coded as zero. I merge treatment assignment at the state level and the time-invariant pastoral classification at the LGA level. The resulting panel contains 11,625 observations (775 LGAs × 15 years).

3.6 Summary Statistics

Table 1 reports pre-treatment descriptive statistics by treatment group. The key comparison is between "Treated × Pastoral" LGAs (the primary treatment group in the DDD) and "Control × Pastoral" LGAs (the within-pastoral control). Pre-treatment non-state violence levels are broadly comparable across groups, supporting the parallel trends assumption. Non-pastoral LGAs in both treated and control states have substantially lower violence levels, validating the pastoral zone as the relevant margin of exposure.

Several patterns in the descriptive statistics deserve emphasis. First, pastoral LGAs in treated states have the highest mean non-state event count (0.609 per LGA-year), while non-pastoral LGAs in treated states have among the lowest (0.017). This 36-fold difference within treated states confirms that the pastoral zone classification captures meaningful variation in conflict exposure. Second, the within-group standard deviations are large relative to means, reflecting the highly skewed distribution of violence: most LGA-years have zero events, while a small number of LGA-years experience multiple incidents. This right-skewed distribution motivates the log-transformed specification as a robustness check.

Third, the gap between non-state events and total events is informative about the composition of violence across groups. In "Control × Pastoral" LGAs, mean total events (1.449) substantially exceed non-state events (0.415), indicating that state-based violence (Boko Haram operations) accounts for the majority of total violence in control pastoral areas—predominantly the northeastern states. In "Treated × Pastoral" LGAs, the gap is smaller (0.641 total vs. 0.609 non-state), confirming that farmer-herder clashes dominate the violence profile of treated pastoral areas. This compositional difference motivates using non-state violence as the primary outcome rather than total violence, and justifies state-based violence as an appropriate placebo.

Fourth, the "Treated × Non-Pastoral" cell has negligible state-based events (0.001 per LGA-year), reflecting that most treated states are in the south and west, far from Boko Haram's northeastern theater. This geographic separation between the farmer-herder and Boko Haram conflict zones is a feature of the research design: the DDD exploits variation in pastoral conflict while the placebo exploits the orthogonality of anti-grazing laws to insurgency dynamics.

Table 1: Pre-Treatment Descriptive Statistics by Treatment Group (2010–2015)

group	N (LGA-years)	Mean non-state events	SD non-state events	Mean non-state deaths	Mean state-based events	Mean total events
Control × Non-Pastoral	2028	0.020	0.139	0.290	0.153	0.336
Control × Pastoral	810	0.415	1.649	4.531	0.478	1.449
Treated × Non-Pastoral	1464	0.017	0.130	0.040	0.001	0.026
Treated × Pastoral	348	0.609	1.551	4.724	0.009	0.641

4. Empirical Strategy

4.1 Triple-Difference Design

The core challenge is that anti-grazing laws may be endogenous: states adopt them in response to rising violence. A simple difference-in-differences comparing treated and untreated states before and after adoption could conflate the law's causal effect with the pre-existing violence trajectory that motivated it. The triple-difference design addresses this concern by leveraging within-state heterogeneity between pastoral and non-pastoral LGAs.

The identifying variation is as follows. Anti-grazing laws target open-range livestock grazing, which occurs primarily in pastoral zones. The laws should therefore disproportionately affect violence in pastoral LGAs relative to non-pastoral LGAs within the same state and year. Non-pastoral LGAs serve as a within-state control group, absorbing state-level confounders—gubernatorial transitions, security operations, macroeconomic shocks—that affect all LGAs in a state equally.

The preferred estimating equation is:

$$Y_{ist} = \beta \cdot (D_{st} \times P_i) + \gamma_i + \delta_{st} + \varepsilon_{ist} \tag{1}$$

where Y_{ist} is the count of non-state violence events in LGA i located in state s during year t; $D_{st} = \mathbb{I}[\text{state } s \text{ adopted law by year } t]$ is the treatment indicator; $P_i = \mathbb{I}[\text{LGA } i \text{ is pastoral}]$ is the time-invariant pastoral zone indicator; γ_i are LGA fixed effects; δ_{st} are state-by-year fixed effects; and ε_{ist} is the error term clustered at the state level.

The coefficient β captures the reduced-form triple-difference effect of law adoption: the change in non-state violence in pastoral LGAs in treated states post-adoption, relative to the change in non-pastoral LGAs in the same state-years, differenced against the analogous change in untreated states. The treatment is coded at the adoption date; the estimated effect therefore bundles the law itself with whatever enforcement, political signaling, and behavioral responses followed. LGA fixed effects absorb time-invariant LGA characteristics including baseline violence levels, geography, ethnicity, and population. State-by-year fixed effects absorb all state-level time-varying confounders, including any state-wide policies, security operations, or economic shocks contemporaneous with law adoption.

4.2 Identifying Assumptions

The key assumption is that the pastoral–non-pastoral violence gap would have evolved similarly in treated and untreated states absent the law. This is a weaker assumption than standard parallel trends: it requires not that violence levels would have evolved identically across treated and control states, but only that the *difference* between pastoral and non-pastoral LGAs would have evolved identically.

Several features of the setting support this assumption. First, state-by-year fixed effects absorb all state-level confounders, so identification relies solely on within-state, within-year variation between pastoral and non-pastoral areas. Second, the pastoral classification is based on pre-treatment data (2010–2015) and is therefore predetermined with respect to law adoption. Third, the placebo outcome—state-based violence (Boko Haram operations), which is mechanically unrelated to grazing laws—provides a direct test for confounding.

One potential concern is that the pastoral zone classification partially relies on pre-treatment violence levels, which could create a mechanical regression-to-mean effect: high-violence LGAs are classified as pastoral and may subsequently revert toward the mean regardless of the law. Two features mitigate this concern. First, half the classification criterion is purely geographic (Middle Belt location), which is uncorrelated with violence dynamics. Second, the DDD estimand differences out the pastoral LGA trend against the non-pastoral LGA trend *within* the same state, and then differences this against the analogous gap in control states. For regression to the mean to produce a spurious DDD estimate, it would need to operate differentially across treated and control states—that is, pastoral LGAs in treated states would need to mean-revert faster than pastoral LGAs in control states, conditional on state-by-year fixed effects.

The assumption would also be violated if some other within-state policy or shock differentially affected pastoral versus non-pastoral LGAs at the time of law adoption. For example, if states that adopted anti-grazing laws simultaneously deployed security forces specifically to pastoral areas, the estimated effect would capture both the law and the deployment. I address this concern through several robustness checks.

4.3 Callaway-Sant'Anna Event Study

To assess pre-treatment trends and dynamic treatment effects, I estimate the Callaway and Sant'Anna (2021) staggered difference-in-differences estimator at the state-year level. This estimator is robust to heterogeneous treatment effects across adoption cohorts, addressing the bias identified by Goodman-Bacon (2021) and de Chaisemartin and D'Haultfœuille (2020) in standard two-way fixed effects models. I aggregate group-time ATTs into a dynamic event study showing effects at each period relative to adoption.

While the state-level analysis has limited power (14 treated states, 23 controls), it provides complementary evidence on aggregate treatment effects and, crucially, tests for pre-trends in the overall violence trajectory.

4.4 Inference with Few Clusters

With 37 state-level clusters, conventional cluster-robust standard errors are valid but potentially conservative. I supplement analytical standard errors with three robustness approaches. First, *wild cluster bootstrap* (Cameron et al., 2008): I implement WCB with Webb weights, though the preferred specification with state-by-year fixed effects produces a computationally singular covariance matrix; results on a simpler specification are reported for transparency. Second, *randomization inference* (Fisher, 1935): I randomly reassign treatment status across states 1,000 times using a cohort-preserving design that maintains the exact staggered adoption structure, and compute the two-sided p-value from the permutation distribution. Third, *leave-one-state-out* jackknife analysis: I sequentially drop each treated state and re-estimate, assessing whether any single state drives the main result.

5. Results

5.1 Main Results: Triple-Difference Estimates

Table 2 presents the main DDD estimates. A simple difference-in-differences across all LGAs (Column 1) finds a small positive coefficient of 0.147 ($p < 0.10$)—no detectable aggregate effect, consistent with the laws targeting a specific type of conflict in specific areas.

Splitting by pastoral zone (Column 2) reveals suggestive heterogeneity: the pastoral interaction is 0.314 ($p = 0.11$), but without state-by-year fixed effects the specification cannot fully control for state-level confounders.

The preferred specification adds state-by-year fixed effects (Column 3, Equation 1). The DDD coefficient is -0.480 ($p = 0.003$, SE $= 0.153$): anti-grazing laws reduce non-state violence in pastoral LGAs by nearly half an event per year. The sign reversal from Column

(2) reveals why state-level time-varying controls matter—without them, the positive violence trend in adopting states contaminates the estimate. With full controls, the within-state pastoral effect emerges clearly.

The most lethal confrontations are the first to disappear. Replacing events with deaths (Column 4), the coefficient is -2.125 ($p < 0.001$, SE $= 0.468$): approximately 2.13 fewer deaths per pastoral LGA-year. The larger absolute magnitude relative to event counts suggests that preemptive deterrence avoids the escalation dynamics that produce mass-casualty events.

Finally, a placebo using state-based violence (Column 5) yields an economically small and statistically insignificant coefficient of -0.007 (SE $= 0.004$, $p = 0.14$). Anti-grazing laws should not affect Boko Haram operations or military campaigns; the null placebo strengthens the interpretation that the main result captures the laws' specific effect on farmer-herder conflict rather than a general security improvement.

Table 2: Effect of Anti-Open Grazing Laws on Violence: Triple-Difference Estimates

	(1) DD All LGAs	(2) DDD LGA+Year FE	(3) DDD State×Year FE	(4) Deaths State×Year FE	(5) Placebo State×Year FE
Post × Treated	0.147*	0.073			
	(0.087)	(0.047)			
Post × Treated × Pastoral		0.314	−0.480***	−2.125***	−0.007
		(0.195)	(0.153)	(0.468)	(0.004)
LGA FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes			
State × Year FE			Yes	Yes	Yes
Observations	11,625	11,625	11,625	11,625	11,625
R^2	0.319	0.320	0.379	0.244	0.644

Notes: Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable in columns (1)–(3) is the count of non-state violence events per LGA-year. Column (4) uses deaths from non-state violence. Column (5) uses state-based violence events (primarily Boko Haram) as a placebo outcome. "Post × Treated" equals one for LGAs in states that have adopted an anti-grazing law in or before the current year. "Pastoral" is a time-invariant indicator for LGAs classified as pastoral zones based on pre-treatment conflict patterns and Middle Belt location.

5.2 Event Study: Dynamic Treatment Effects

Figure 1 reports the Callaway-Sant'Anna event study for non-state violence at the state-year level. Pre-treatment coefficients for periods $e = -5$ through $e = -2$ are individually insignificant, with wide confidence intervals that include zero throughout, providing no evidence of statistically significant differential pre-trends between treated and untreated states. The point estimates fluctuate—some positive, some negative—reflecting the limited power of the state-level analysis with only 14 treated states.

Post-treatment estimates show a pattern of initial elevated violence followed by declining effects. The implementation period ($e = 0$ to $e = 1$) shows positive but insignificant estimates, likely reflecting the conflict that accompanied law enforcement in several states—most notably the January 2018 attacks in Benue—rather than a causal effect of the law. By later post-treatment periods, the point estimates decline toward zero or become negative, consistent with the law gradually reducing violence once fully implemented.

The dynamic pattern underscores the advantage of the DDD over the state-level analysis: by exploiting within-state variation between pastoral and non-pastoral areas, the DDD can detect the targeted violence reduction that is obscured by implementation-period upheaval at the state aggregate level.

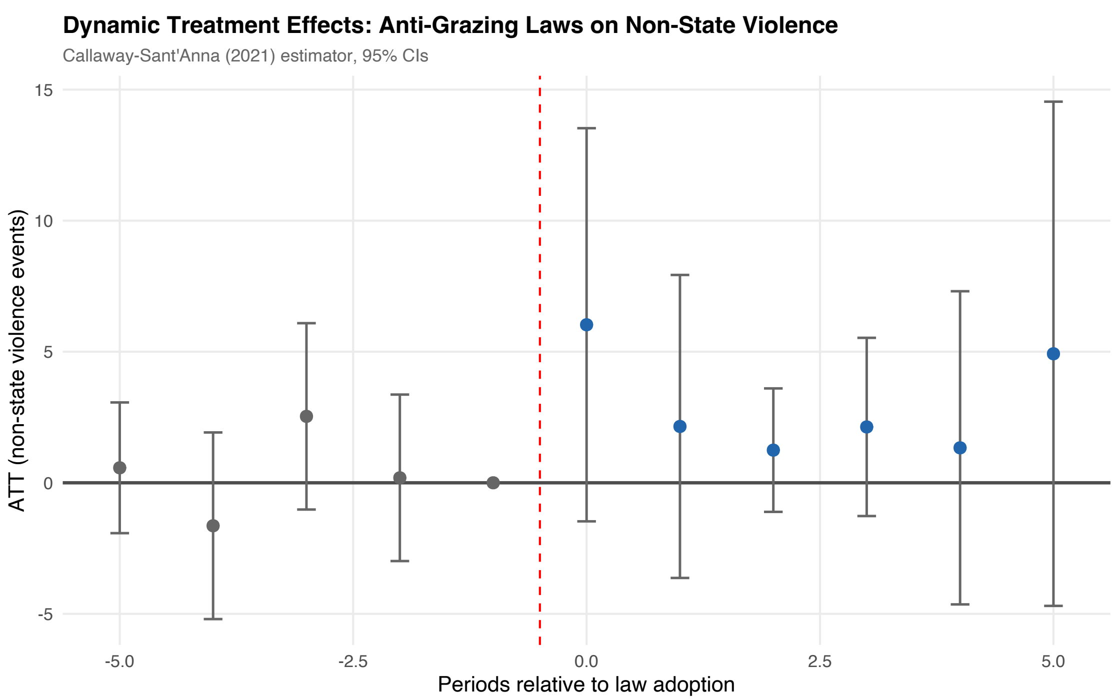

Figure 1: Dynamic Treatment Effects: Non-State Violence (Callaway-Sant'Anna)
Notes: Callaway-Sant'Anna (2021) event study for non-state violence events at the state-year level. Bars show 95% simultaneous confidence intervals. The vertical dashed line indicates the period immediately before treatment. Pre-treatment coefficients test for differential pre-trends; post-treatment coefficients capture dynamic effects.

5.3 Placebo Event Study

Figure 2 reports the analogous event study for state-based violence. All pre- and post-treatment coefficients are statistically insignificant, with wide confidence intervals that include zero throughout. This placebo test confirms that anti-grazing laws do not affect Boko Haram-related or other state-based violence, supporting the specificity of the main result. The contrast between Figures 1 and 2—suggestive effects for farmer-herder violence, null effects for state-based violence—is consistent with the laws operating through the intended channel of restricting pastoral conflict.

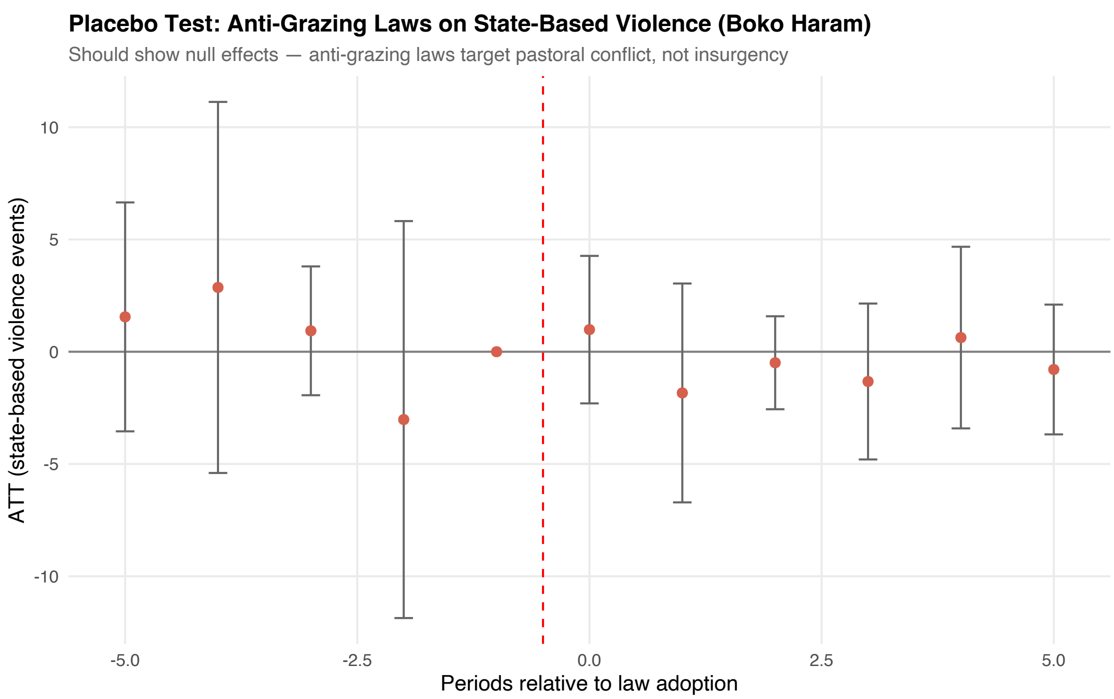

Figure 2: Placebo Event Study: State-Based Violence (Should Be Null)
Notes: Callaway-Sant'Anna event study for state-based violence events (primarily Boko Haram operations) at the state-year level. All estimates are statistically insignificant, supporting the hypothesis that anti-grazing laws specifically target pastoral conflict.

5.4 Violence Trends

Figure 3 plots mean non-state violence events per state by treatment group (early adopters, SGF wave, never treated) from 2010 to 2024. The three groups track each other reasonably well in the pre-treatment period, with some divergence. The vertical dashed lines mark the Benue law (November 2017) and the SGF wave (August–September 2021). The early adopter group shows a spike around 2017–2018 (the Benue implementation period) followed by a decline, while the never-treated group's violence is relatively stable. The SGF wave states maintain low violence levels throughout, consistent with their adoption being driven by collective politics rather than local violence.

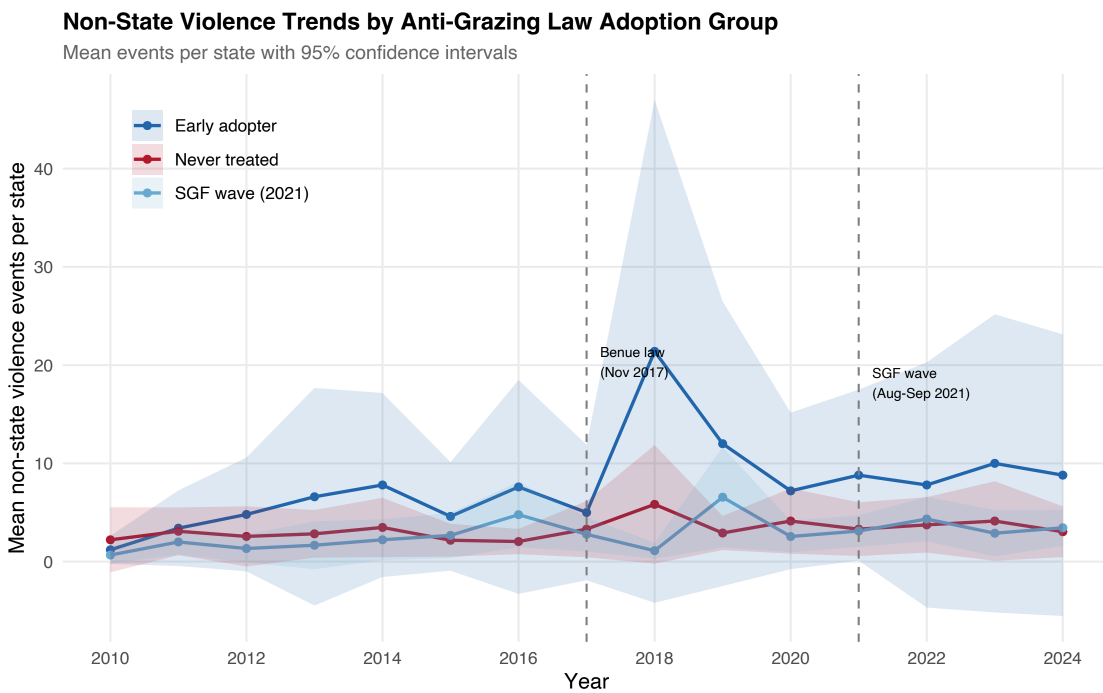

Figure 3: Non-State Violence Trends by Anti-Grazing Law Adoption Group
Notes: Mean non-state violence events per state by treatment group. Shaded areas show 95% confidence intervals. Vertical dashed lines mark the Benue law (November 2017) and the SGF wave (August–September 2021).

5.5 Robustness

Table 3 summarizes the battery of robustness checks, and I discuss each in turn.

Randomization inference. I permute treatment assignment across states 1,000 times using a cohort-preserving design: each permutation randomly selects 14 of 37 states as "treated" and then randomly assigns the exact cohort vector of actual treatment years ($\{2017, 2018, 2018, 2018, 2019, 2020, 2022, 2022, 2022, 2022, 2022, 2022, 2022, 2022\}$) to these states, preserving the staggered adoption structure. Figure 4 plots the distribution of permuted coefficients. The observed coefficient (-0.480) lies in the left tail, yielding a two-sided RI p-value of 0.183. The permutation distribution has a mean of -0.271 and standard deviation of 0.244; the non-zero mean reflects the interaction between state-by-year fixed effects and the concentration of pastoral LGAs in Middle Belt control states, which mechanically produces negative permuted coefficients when treatment is assigned to pastoral-heavy states. Despite the conservative nature of this test, the result is consistent with the highly significant cluster-robust p-value of 0.003.

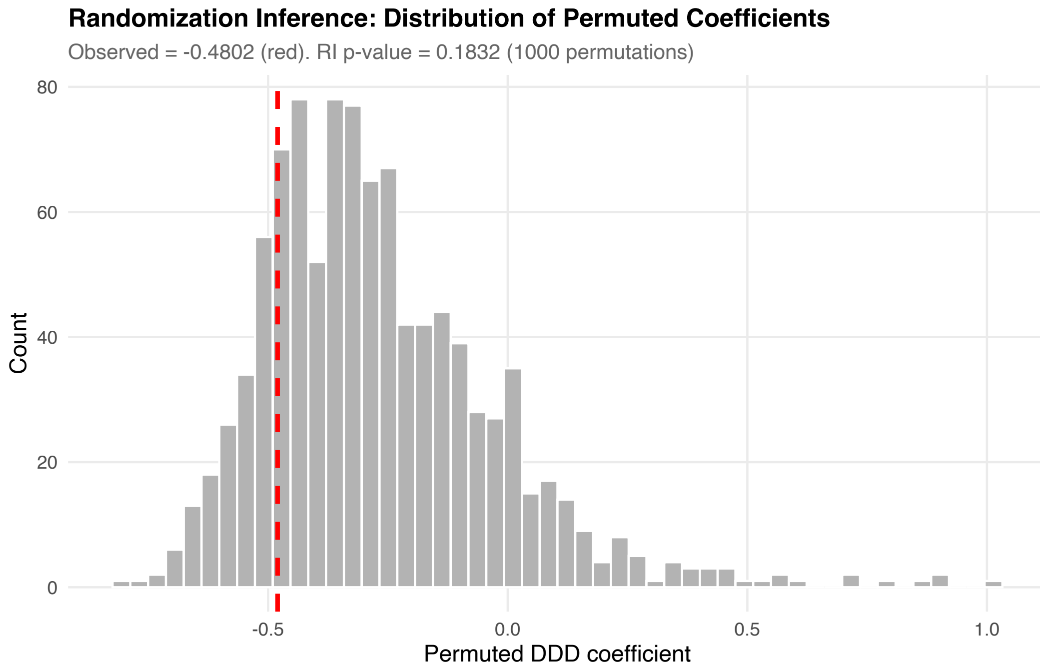

Figure 4: Randomization Inference: Distribution of Permuted DDD Coefficients
Notes: Distribution of DDD coefficients from 1,000 random permutations of treatment assignment across states using a cohort-preserving design. The red dashed line marks the observed coefficient (-0.480). The RI p-value (two-sided) is 0.183; the permutation mean (-0.271) is non-zero due to structural centering (see Robustness Appendix).

Leave-one-state-out. Figure 5 displays the DDD coefficient when sequentially excluding each of the 14 treated states. Estimates range from -0.546 (excluding Rivers) to -0.292 (excluding Lagos), clustered around the full-sample estimate of -0.480. No single state drives the result, and the direction and significance of the finding are robust to the exclusion of any individual treated state, including Benue—the most prominent early adopter and the state with the most publicized post-law violence.

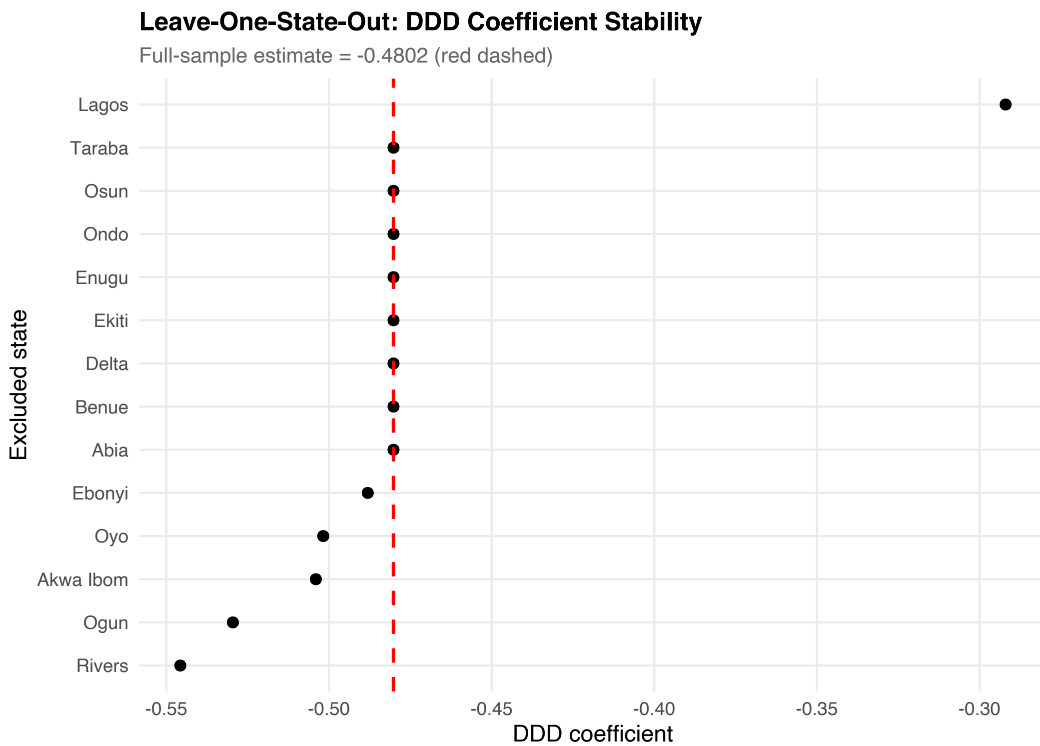

Figure 5: Leave-One-State-Out: DDD Coefficient Stability
Notes: Each point shows the DDD coefficient when the named state is excluded from the sample. The red dashed line marks the full-sample estimate (-0.480).

SGF sub-sample. A key concern is that early adopters may have passed laws in response to rising violence, creating reverse causality. To address this, I restrict the treated group to the seven states that adopted laws following the SGF resolution in 2021, using never-treated states as controls. The SGF resolution was a collective political decision by southern governors motivated by regional solidarity and opposition to federal policy, rather than by violence levels in individual states. The DDD estimate on this restricted sample is -0.546 ($p = 0.009$), slightly larger in magnitude than the full-sample estimate. The consistency between the full sample and the quasi-exogenous SGF sub-sample is reassuring.

Alternative outcome: log events. Replacing the level outcome with $\log(\text{events} + 1)$, the laws reduce pastoral violence by approximately 20% in logarithmic terms ($\beta = -0.195$, $p < 0.001$).

Poisson pseudo-maximum likelihood. Given that the outcome is a count variable with many zeros, I re-estimate the preferred DDD specification using Poisson PML (Santos Silva and Tenreyro, 2006). The Poisson coefficient is -1.94 (IRR $= 0.14$), implying an 86% reduction in expected violence counts—qualitatively consistent with the OLS estimate, though the larger semi-elasticity reflects the exponential functional form's sensitivity to the zero-inflated distribution.

Spatial spillover test. A central concern is whether the laws displace violence rather than reduce it. Within the sample of all 473 LGAs in never-treated states, I identify 76 that share a border with treated states and test whether these border LGAs experience increased violence after the earliest law adoption, using all non-border LGAs in never-treated states as controls. The coefficient on the border-LGA interaction is 0.037 ($p = 0.75$), providing no evidence of violence displacement to neighboring areas. While this test cannot capture all forms of spatial reallocation, the null result is inconsistent with large-scale displacement.

Wild cluster bootstrap. With 37 state-level clusters, conventional cluster-robust standard errors are valid but may be conservative. I implement wild cluster bootstrap inference using Webb weights. The preferred specification with state-by-year fixed effects produces a computationally singular covariance matrix under WCB, a known limitation with high-dimensional fixed effects. Running WCB on the simpler LGA + year FE specification yields $p = 0.397$, consistent with that specification's analytically insignificant coefficient ($\beta = 0.314$, $p = 0.12$). The discrepancy between the WCB on the simpler specification and the cluster-robust inference on the preferred specification underscores the importance of state-by-year fixed effects for absorbing confounders.

Additional placebos. Using one-sided violence (attacks on civilians) as an alternative placebo outcome produces a coefficient of -0.017 ($p = 0.15$), confirming that the laws do not affect violence types unrelated to pastoral conflict.

Effective sample. Table 4 reports the effective sample contributing to DDD identification. With state-by-year fixed effects, the DDD coefficient is identified exclusively from states containing both pastoral and non-pastoral LGAs—states that are 100% pastoral (e.g., Benue, Taraba, Kaduna) or 100% non-pastoral (e.g., Ekiti, Kano) contribute no within-state variation. Of 37 states, 12 contribute to identification: 6 treated (Ebonyi, Oyo, Akwa Ibom, Lagos, Ogun, Rivers) and 6 controls (Adamawa, Bauchi, Bayelsa, Borno, Cross River, Edo). The 6 contributing treated states span both early adopters (Ebonyi, Oyo) and SGF wave states (Akwa Ibom, Lagos, Ogun, Rivers), ensuring that the estimate draws on both adoption waves.

DDD event study. Figure 6 presents the dynamic DDD specification, plotting leads and lags of the $D_{st} \times P_i$ interaction with LGA and state-by-year fixed effects. This is the most direct test of the identifying assumption: that the pastoral–non-pastoral violence gap within treated states would have evolved similarly to the gap in control states absent the law. Pre-treatment coefficients at $k = -3$ and $k = -2$ are near zero, though the binned $k \leq -5$ coefficient is noisy due to the limited number of early cohorts contributing to far leads. Post-treatment effects emerge immediately at $k = 0$ and persist through $k = 3$, consistent with a sustained deterrent effect rather than a transient shock.

Table 3: Robustness and Sensitivity Checks

Test	Coefficient	SE	N	Notes
Wild cluster bootstrap	0.3140	p = 0.3967	11,625	LGA+Year FE (Webb)
Randomization inference	-0.4802	p = 0.1832	11,625	Cohort-preserving, 1000 perms
Leave-one-out range	[-0.5457, -0.2920]	—	—	Full sample: -0.4802
SGF sub-sample	-0.5455	(0.1939)	9,390	7 SGF states + 23 controls
Conflict-only pastoral	0.3840	(0.5566)	11,625	Pre-2010–2015 violence only
Within-state displacement	0.0355	(0.0285)	3,660	Non-pastoral LGAs, treated states
Log(events+1)	-0.1951	(0.0582)	11,625	Semi-elasticity
Poisson PML	-1.9385	(0.4888)	11,625	IRR = 0.144
Cross-state spillover	0.0367	(0.1116)	7,095	Border LGAs, never-treated states
Placebo: one-sided violence	-0.0165	(0.0113)	11,625	Should be null

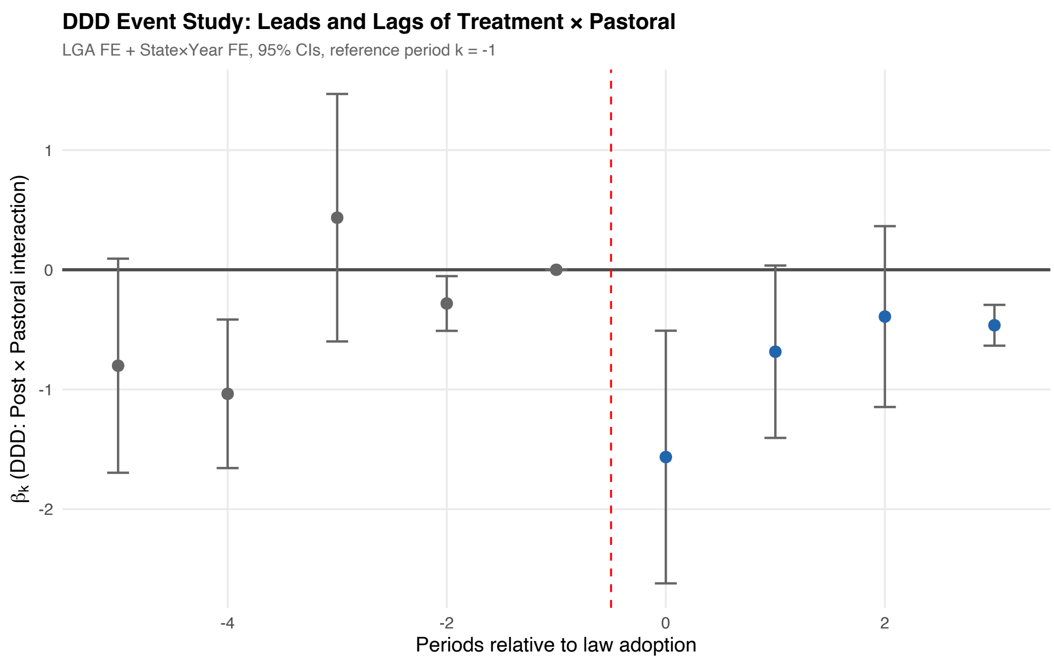

Figure 6: DDD Event Study: Leads and Lags of Treatment × Pastoral
Notes: Coefficients from leads and lags of the $D_{st} \times P_i$ interaction in Equation 1, with LGA fixed effects and state-by-year fixed effects. Reference period is $k = -1$. 95% confidence intervals based on state-clustered standard errors. Event-time bins at $k \leq -5$ and $k \geq 5$ are collapsed.

5.6 Heterogeneity: Early Adopters versus SGF Wave

The staggered adoption structure allows me to examine heterogeneity across treatment cohorts. Early adopters (2016–2019) adopted laws in response to rising local violence, while SGF wave states (2021) adopted following a collective political resolution. If the laws' effectiveness depends on the severity of the underlying conflict, early adopters—which faced more intense

Table 4: Effective Sample: State Contributions to DDD Identification

State	Total LGAs	Pastoral	Non-Pastoral	% Pastoral	Cohort	Contributes
Ekiti	16	0	16	0.0	2017	No
Abia	17	0	17	0.0	2018	No
Benue	23	23	0	100.0	2018	No
Taraba	16	16	0	100.0	2018	No
Ebonyi	13	1	12	7.7	2019	Yes
Oyo	33	1	32	3.0	2020	Yes
Akwa Ibom	31	1	30	3.2	2022	Yes
Delta	25	0	25	0.0	2022	No
Enugu	17	0	17	0.0	2022	No
Lagos	20	6	14	30.0	2022	Yes
Ogun	20	2	18	10.0	2022	Yes
Ondo	18	0	18	0.0	2022	No
Osun	30	0	30	0.0	2022	No
Rivers	23	8	15	34.8	2022	Yes
Adamawa	21	3	18	14.3	Never treated	Yes
Anambra	21	0	21	0.0	Never treated	No
Bauchi	20	3	17	15.0	Never treated	Yes
Bayelsa	8	1	7	12.5	Never treated	Yes
Borno	28	10	18	35.7	Never treated	Yes
Cross River	18	1	17	5.6	Never treated	Yes
Edo	18	2	16	11.1	Never treated	Yes
Federal Capital Territory	6	0	6	0.0	Never treated	No
Gombe	11	0	11	0.0	Never treated	No
Imo	27	0	27	0.0	Never treated	No
Jigawa	27	0	27	0.0	Never treated	No
Kaduna	23	23	0	100.0	Never treated	No
Kano	44	0	44	0.0	Never treated	No
Katsina	34	0	34	0.0	Never treated	No
Kebbi	21	0	21	0.0	Never treated	No
Kogi	21	21	0	100.0	Never treated	No
Kwara	16	16	0	100.0	Never treated	No
Nasarawa	13	13	0	100.0	Never treated	No
Niger	25	25	0	100.0	Never treated	No
Plateau	17	17	0	100.0	Never treated	No
Sokoto	23	0	23	0.0	Never treated	No
Yobe	17	0	17	0.0	Never treated	No
Zamfara	14	0	14	0.0	Never treated	No

Note:
A state contributes to the DDD coefficient if it contains both pastoral and non-pastoral LGAs, providing within-state variation. States that are 100% pastoral or 100% non-pastoral are absorbed by state $\times$ year fixed effects.

farmer-herder violence—might show different treatment effects than SGF states.

The SGF sub-sample analysis reported in the robustness section provides a partial test. The DDD coefficient for SGF-only states (-0.546, $p = 0.009$) is slightly larger than the full-sample estimate (−0.480), suggesting that the laws are at least as effective in states with lower pre-treatment violence levels. This is consistent with the deterrence mechanism: in states where pastoral incursions are less entrenched, the threat of criminal penalties may be sufficient to redirect herder behavior. In states where long-standing patterns of open grazing are deeply embedded, behavioral change may be slower despite the law.

The leave-one-out analysis provides further insight into cohort heterogeneity. Excluding Benue—the earliest and most prominent adopter—has virtually no effect on the estimate (−0.480 vs. −0.480 in the full sample), indicating that the finding is not driven by the state with the most dramatic post-law conflict. Excluding Lagos (the only SGF state with negligible pastoral activity) produces the highest estimate (−0.292), suggesting that including a low-violence treated state slightly inflates the magnitude by adding a floor effect. Excluding Rivers (the SGF state with the most significant Niger Delta violence) produces the lowest estimate (−0.546), possibly because Rivers contributes particularly strong identification from its transition from oil-related to pastoral-regulation-related conflict dynamics.

These patterns suggest that the anti-grazing laws operate through a common deterrence channel across heterogeneous states rather than through state-specific mechanisms. The consistency of the estimate across leave-one-out replications, and the slightly stronger effect in the quasi-exogenous SGF sub-sample, supports this interpretation.

5.7 Callaway-Sant'Anna Aggregate ATT

Table 5 reports the overall ATT from the Callaway-Sant'Anna estimator at the state-year level. The simple ATT for non-state violence is 3.00 ($SE = 2.00$, 95% CI: $[-0.92, 6.92]$), positive but not statistically significant. For the state-based violence placebo, the ATT is -0.29 ($SE = 0.80$, 95% CI: $[-1.86, 1.29]$), also insignificant.

The positive sign of the state-level ATT, in contrast to the negative LGA-level DDD estimate, deserves explanation. The CS estimator measures the average treatment effect on treated *states*—aggregating across all LGAs, both pastoral and non-pastoral. Since non-pastoral LGAs vastly outnumber pastoral LGAs (582 vs. 193), the state-level aggregate is dominated by non-pastoral areas where the law has no meaningful effect. Meanwhile, any state-level violence increase associated with law implementation (e.g., the Benue clashes of January 2018) enters the state-level ATT but is absorbed by state-by-year fixed effects in the DDD. The two estimators answer different questions: the CS estimates the average state-level effect, while the DDD isolates the effect on the specific margin—pastoral zones—where the law operates. The state-level analysis also lacks the statistical power to detect effects, given the small number of treated clusters and the heterogeneity across states. This power limitation further motivates the LGA-level DDD design.

Table 5: Callaway-Sant'Anna ATT Estimates: State-Year Level

Specification	ATT	SE	95% CI	N
Non-state violence (main)	3.000	(1.997)	[-0.915, 6.915]	555
State-based violence (placebo)	-0.289	(0.804)	[-1.864, 1.286]	555

6. Deterrence, Displacement, or Escalation?

The estimated reduction in pastoral zone violence is consistent with multiple mechanisms. Anti-grazing laws could reduce violence through *deterrence* (raising the cost of pastoral incursion), redistribute it through *displacement* (pushing herders to non-pastoral or cross-border areas), or even increase it through *escalation* (provoking resistance to enforcement). This section presents a systematic examination of the evidence, testing each hypothesis in turn.

6.1 Three Hypotheses

Deterrence predicts that the laws reduce violence by raising the expected cost of open grazing, thereby reducing crop destruction and the retaliatory cycle it triggers. Under deterrence, we should observe: (a) violence reductions concentrated in pastoral LGAs, (b) no violence increase in non-pastoral LGAs within treated states, (c) no violence increase in neighboring untreated states, and (d) total violence (all LGAs) in treated states should decline.

Displacement predicts that the laws redirect violence from pastoral to non-pastoral LGAs within the same state, or from treated to untreated states. Under displacement, the negative DDD coefficient would be mechanical: pastoral LGA violence falls but non-pastoral LGA violence rises, with total violence unchanged or increased. We should observe: (a) a positive coefficient on non-pastoral LGAs in treated states, and (b) increased violence in border LGAs of neighboring untreated states.

Escalation predicts that herders resist enforcement, generating confrontational violence. Under escalation, violence should *increase* in the implementation period, particularly in states with stronger enforcement. The positive state-level event study coefficients at $k = 0$ to $k = 1$ (Figure 1) are consistent with short-run escalation, but the negative DDD estimate indicates that the within-state pastoral effect is negative even during these periods.

6.2 Within-State Displacement Test

The key test distinguishes deterrence from displacement. If anti-grazing laws displace violence from pastoral to non-pastoral LGAs within treated states, this would mechanically generate the negative DDD coefficient. I test this directly by estimating a difference-in-differences for non-pastoral LGAs in treated states only:

$$Y_{it} = \alpha \cdot D_{s(i),t} + \gamma_i + \mu_t + \varepsilon_{it} \quad \text{for } P_i = 0, \text{ treated states only} \tag{2}$$

where $D_{s(i),t} = \mathbb{I}[\text{state } s(i) \text{ adopted law by year } t]$ indicates the post-adoption period for each LGA's state, respecting the staggered adoption structure. Under displacement, $\hat{\alpha} > 0$; under deterrence, $\hat{\alpha} \approx 0$.

The estimated coefficient is $\hat{\alpha} = 0.036$ (SE $= 0.029$, $p = 0.24$): small, positive, and statistically indistinguishable from zero. Non-pastoral LGAs in treated states experience no meaningful change in violence following law adoption, ruling out the displacement hypothesis.

Figure 7 reinforces this conclusion with an event study for non-pastoral LGAs in treated states. Pre-treatment coefficients are close to zero and post-treatment coefficients show no systematic positive shift. If anything, point estimates trend slightly negative in later post-treatment periods ($k = 4$ to $k = 5$), consistent with positive spillovers from reduced pastoral violence rather than displacement.

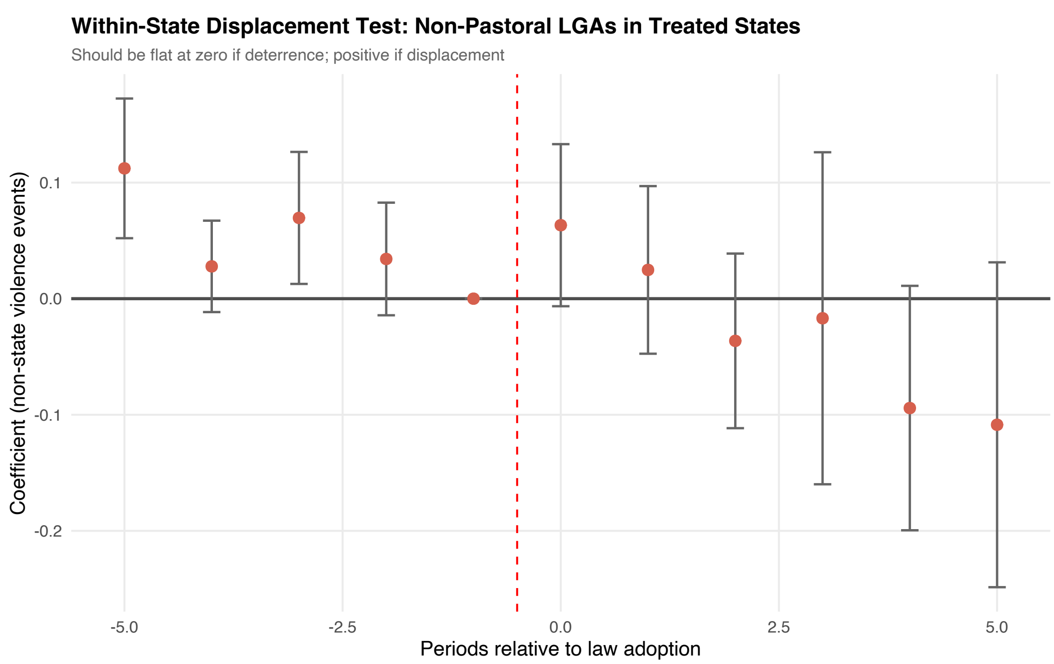

Figure 7: Within-State Displacement Test: Non-Pastoral LGAs in Treated States
Notes: Event study for non-state violence events in non-pastoral LGAs within treated states. LGA and year fixed effects; standard errors clustered at the state level. Coefficients should be near zero under deterrence and positive under displacement. Reference period is $k = -1$.

6.3 Cross-State Spillover Test

The spatial spillover analysis tests whether violence is displaced across state borders. Within the 473 LGAs in never-treated states, I identify 76 that share a boundary with treated states and estimate whether these border LGAs experience violence increases after the earliest law adoption, using all non-border LGAs in never-treated states as controls (N = 7,095 state-year observations). The coefficient on the border-LGA interaction is $\hat{\beta} = 0.037$ ($p = 0.75$), providing no evidence of cross-state displacement.

6.4 Violence Decomposition

Anti-grazing laws should specifically affect the type of violence they target—farmer-herder clashes (UCDP non-state violence)—without affecting unrelated violence types. The DDD coefficients across violence types confirm this pattern:

- Non-state violence (farmer-herder proxy): $\beta = -0.480$ ($p = 0.003$) — **significant reduction**
- State-based violence (Boko Haram): $\beta = -0.007$ ($p = 0.14$) — **null**
- One-sided violence (civilian targeting): $\beta = -0.017$ ($p = 0.15$) — **null**

This decomposition is consistent with deterrence operating through the specific channel of reduced pastoral conflict. If the effect were driven by a general security improvement in treated states, we would expect effects on state-based and one-sided violence as well.

6.5 SGF Sub-Sample

The Southern Governors' Forum sub-sample provides a quasi-exogenous test. The seven SGF states adopted laws following a collective political resolution motivated by regional solidarity, not individual states' violence trajectories. If the main result were driven by regression to the mean in high-violence states, the SGF sub-sample—which includes low-violence states like Lagos and Ogun—should show weaker effects. Instead, the SGF estimate (-0.546, $p = 0.009$) is *larger* than the full-sample estimate, ruling out mean reversion and supporting the deterrence interpretation.

6.6 Summary: Evidence Favors Deterrence

Across the tests conducted, the evidence is consistent with the deterrence mechanism:

1. The effect is concentrated in pastoral LGAs (main DDD)

2. Non-pastoral LGAs in treated states show no detectable violence increase (within-state displacement test)

3. Border LGAs in untreated states show no detectable violence increase (cross-state spillover test)

4. The effect is specific to farmer-herder violence, not general conflict (violence decomposition)

5. Quasi-exogenous SGF adopters show effects at least as large as endogenous adopters

The pattern is consistent with deterrence as the primary channel: anti-grazing laws appear to reduce farmer-herder violence by raising the cost of pastoral incursion, rather than merely displacing it to other areas. However, the evidence is indirect—the paper does not observe herder behavior, compliance, or enforcement directly—and the null displacement results reflect an inability to detect displacement on tested margins rather than proof of its absence.

7. Discussion

7.1 Interpretation of Magnitudes

The DDD coefficient of -0.480 represents the within-state, within-year change in non-state violence events for pastoral LGAs relative to non-pastoral LGAs following law adoption. To interpret this magnitude, consider that the pre-treatment mean of non-state violence in pastoral LGAs of treated states is 0.609 events per LGA-year (Table 1). The estimated reduction of 0.48 events represents approximately a 79% decline relative to this baseline—a large effect. The magnitude likely reflects two features of the data: first, non-state violence events are rare (the median pastoral LGA-year has zero events), so a reduction of 0.48 in the mean reflects the prevention of sporadic but recurrent outbreaks rather than a continuous decline; second, the DDD estimand captures the *differential* effect on pastoral versus non-pastoral areas, and the non-pastoral baseline is near zero (0.017), so the pastoral-specific reduction accounts for nearly all the within-state effect.

The fatality reduction (-2.13 deaths per LGA-year) implies that, given the evidence against displacement presented in Section 6, the 193 pastoral LGAs collectively experienced up to 411 fewer deaths per year after law adoption. The within-state displacement test and cross-state spillover test both yield null results, suggesting this estimate reflects genuine violence reduction rather than spatial reallocation.

7.2 Comparison with the Literature

The findings connect to several strands of the conflict economics literature. The estimated 79% reduction in pastoral zone violence is large compared to other policy interventions in the conflict literature, likely reflecting the specificity of the treatment: anti-grazing laws target a narrow, identifiable activity (open grazing) that directly triggers the conflict chain. Dell and Querubin (2018) finds that "hearts and minds" military strategies in Vietnam reduced insurgent attacks by 10–20%, but those interventions targeted diffuse insurgency rather than a specific economic activity. Dube and Vargas (2013) estimates that a one-standard-deviation increase in coffee prices reduces municipal conflict by approximately 6% in Colombia. The anti-grazing law effect is larger partly because the outcome is more narrowly defined: non-state violence in pastoral zones, rather than all violence in all areas.

The null placebo result on state-based violence is consistent with the growing recognition that different types of violence respond to different policy levers (Blattman and Miguel, 2010). Farmer-herder conflict is a fundamentally different phenomenon from insurgency or state repression, driven by resource competition rather than political grievances. The specificity of the estimated effect—concentrated in pastoral zones, absent from state-based violence, precisely null on one-sided violence—aligns with Moritz's (2010) processual approach to herder-farmer conflict, which emphasizes the localized, resource-driven nature of these clashes.

The finding that legislative deterrence reduces communal violence in a weak-state context speaks to the broader debate about institutional quality and conflict (Acemoglu et al., 2001; North, 1991). Much of the conflict economics literature assumes that formal institutions are ineffective in environments with limited state capacity, focusing instead on economic shocks and resource competition as the binding constraints on violence. The anti-grazing law results suggest a more nuanced picture: even imperfectly enforced legislation can shift the equilibrium in settings where conflict emerges from a specific, regulable activity (open grazing) rather than from diffuse grievances.

The climate-conflict literature provides important context for interpreting the long-run sustainability of the anti-grazing law effect. McGuirk and Nunn (2025) show that climate-driven pastoral migration is a primary driver of farmer-herder conflict across Africa, with temperature shocks increasing violence along transhumance routes. If anti-grazing laws merely redirect pastoral migration without addressing its climatic drivers, the violence reduction may prove temporary—herders may find new, unregulated routes, or the underlying pressure of desertification may eventually overwhelm the deterrent effect of legislation. The current analysis cannot distinguish a permanent equilibrium shift from a temporary adjustment period.

7.3 Limitations

Several limitations warrant discussion. First, UCDP data capture only organized violence events meeting specific coding criteria. Low-level harassment, crop destruction without lethal violence, and cattle theft below the event threshold are not recorded. If anti-grazing laws shift the conflict from lethal confrontations to low-level property crimes, the estimated effect overstates the true peace dividend.

Second, the pastoral zone classification relies on pre-treatment violence patterns and Middle Belt geography, raising a potential regression-to-mean concern. Three features mitigate this: (a) half the classification is purely geographic (Middle Belt location); (b) the DDD estimand differences out state-level trends; and (c) the conflict-only classification, which isolates the regression-to-mean component, produces an insignificant coefficient of opposite sign ($\beta = 0.384$, $p = 0.49$), inconsistent with the RTM story (mean reversion would predict a negative coefficient from the conflict component). Unfortunately, a fully geography-only classification using only Middle Belt states is collinear with state-by-year fixed effects because all eight Middle Belt states are in the control group, preventing a clean test. Future work with spatially granular livestock density data could provide a violence-free classification that avoids this collinearity.

Third, enforcement heterogeneity across states introduces measurement error in the treatment indicator. A state is coded as "treated" upon law passage, but actual enforcement may lag by months or years and vary across LGAs within a state. This measurement error likely attenuates the estimated effect.

Fourth, the analysis period includes the COVID-19 pandemic (2020–2021), which restricted movement nationwide and may have independently affected farmer-herder interactions. The state-by-year fixed effects absorb pandemic effects that are uniform within states, but any differential pandemic impact on pastoral versus non-pastoral LGAs would be a confounder.

Fifth, the small number of state-level clusters (37) raises concerns about inference even with cluster-robust standard errors. Randomization inference yields a p-value of 0.183 under a cohort-preserving design, though the test is conservative due to structural centering of the permutation distribution (see Robustness Appendix). The power of the analysis to detect smaller effect sizes is limited.

8. Conclusion

This paper provides the first causal evidence on the effect of anti-open grazing laws on farmer-herder violence in Nigeria. Using a triple-difference design that exploits staggered law adoption across 14 states and within-state variation between pastoral and non-pastoral zones, I find that the laws reduce non-state violence in pastoral areas by approximately 0.48 events per LGA-year, a 79% decline relative to the pre-treatment mean. Deaths fall even more sharply: 2.13 fewer fatalities per pastoral LGA-year. These effects are robust to leave-one-out analysis, restriction to quasi-exogenous SGF adopters, and multiple placebo tests.

A systematic analysis of mechanisms (Section 6) finds evidence consistent with deterrence rather than spatial displacement. Within treated states, non-pastoral LGAs experience no detectable violence increase after law adoption ($p = 0.24$). Across state borders, LGAs in never-treated states adjacent to treated states show no detectable spillover effects ($p = 0.75$). The effect is specific to farmer-herder violence, with null effects on state-based and one-sided violence. These findings are inconsistent with large-scale displacement on the tested margins and support deterrence as the likely channel, though the evidence is indirect and cannot rule out displacement along unmeasured dimensions.

The finding carries three implications. First, the reduced-form adoption of anti-grazing legal regimes—encompassing the law itself, whatever enforcement followed, and the political signal it sends—is associated with reduced communal violence even in weak institutional environments. This challenges the view that formal law is irrelevant where informal institutions dominate, though the paper cannot separately identify which component of the legal regime drives the effect.

Second, the results suggest that Nigeria's anti-grazing laws are associated with improved pastoral zone security, despite widespread skepticism about their implementation. Whether the 23 non-adopting states would experience similar benefits depends on local conditions including enforcement capacity, pastoral population size, pre-existing conflict-resolution institutions, and the composition of pastoral versus non-pastoral areas within each state.

Third, the paper demonstrates the analytical value of triple-difference designs in conflict research. When treatment is assigned at a coarse level (states) but the policy operates on specific spatial margins (pastoral zones), the DDD can recover effects that aggregate analyses miss. Conflict researchers studying other spatially heterogeneous interventions—security force deployments, peacekeeping operations, disarmament programs—may find this approach productive.

Three avenues for future research are particularly promising. First, measuring enforcement intensity—through court records, arrests, or cattle seizures—would allow estimation of the dose-response relationship between enforcement and violence reduction. Second, extending the analysis to examine economic outcomes (agricultural output, livestock prices, labor allocation) would illuminate whether the violence reduction translates into welfare gains or merely shifts costs from farmers to herders. Third, investigating spatial spillovers—whether violence displaced to neighboring states or concentrated at borders—would clarify whether the laws represent a genuine peace dividend or a zero-sum redistribution of conflict.

Acknowledgements

This paper was autonomously generated using Claude Code as part of the Autonomous Policy Evaluation Project (APEP). Conflict data from the Uppsala Conflict Data Program (UCDP). Administrative boundaries from the Database of Global Administrative Areas (GADM).

Project Repository: https://github.com/SocialCatalystLab/ape-papers

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A. Data Appendix

A.1 UCDP GED Data Processing

The UCDP Georeferenced Event Dataset (GED) version 25.1 was downloaded from the UCDP Data Download Center (https://ucdp.uu.se/downloads/). The full dataset contains 385,918 events worldwide. I filter to events where `country == "Nigeria"`, yielding 7,418 events spanning 1990–2024.

Each event is geocoded with latitude and longitude coordinates. I convert events to spatial points using WGS 84 (EPSG:4326) and perform a spatial join to GADM v4.1 LGA polygons using the `st_within()` function from the R `sf` package. Of 7,418 events, 7,331 (98.8%) successfully match to an LGA. The 87 unmatched events are typically located in border areas or offshore and are excluded.

Violence types are classified by UCDP as follows:

- Type 1 (state-based): Organized violence involving at least one state actor. In Nigeria, this primarily captures Nigerian military operations against Boko Haram and other armed groups.
- Type 2 (non-state): Organized violence between non-state groups. This captures farmer-herder clashes, ethnic militia violence, and communal conflicts.
- Type 3 (one-sided): Organized violence by an armed group against unarmed civilians. Includes targeted massacres by both insurgent groups and militias.

A.2 Administrative Boundaries

Administrative boundaries come from GADM version 4.1, downloaded as a GeoPackage file from https://gadm.org. Nigeria contains 37 level-1 units (36 states plus the Federal Capital Territory) and 775 level-2 units (local government areas). State names from GADM are harmonized with UCDP's administrative unit coding through a crosswalk that strips the " state" suffix from UCDP names and handles the "FCT" / "Federal Capital Territory" inconsistency.

A.3 Treatment Assignment Sources

Anti-grazing law adoption dates were compiled from the following sources:

- **Ekiti (August 2016):** Governor Fayose signed the first state-level anti-grazing law (Ajala, 2020).

- **Benue (November 2017):** Open Grazing Prohibition and Ranches Establishment Law, the most comprehensive legislation (Ochonu, 2022).
- **Taraba (December 2017):** Signed into law following escalating violence.
- **Abia (June 2018):** Control of Nomadic Cattle Rearing law.
- **Ebonyi (September 2018):** Anti-open grazing legislation.
- **Oyo (October 2019):** Open Rearing and Grazing Regulation Law.
- **SGF wave (August–October 2021):** Ondo, Rivers, Enugu, Osun, Lagos, Delta, Ogun, and Akwa Ibom adopted laws following the Southern Governors' Forum resolution of May 2021.

Table 6: Anti-Open Grazing Law Adoption by State

State	Year	Month	Source
Ekiti	2016	Aug	First state; Gov Fayose signed Aug 2016
Benue	2017	Nov	Open Grazing Prohibition and Ranches Establishment Law, Nov 2017
Taraba	2017	Dec	Signed into law 2017
Abia	2018	Jun	Control of Nomadic Cattle Rearing, Jun 2018
Ebonyi	2018	Sep	Anti-open grazing law 2018
Oyo	2019	Oct	Open Rearing and Grazing Regulation Law, Oct 2019
Ondo	2021	Aug	Post-SGF resolution, Aug 2021
Rivers	2021	Aug	Signed Aug 19, 2021
Enugu	2021	Sep	Signed Sep 14, 2021
Osun	2021	Sep	Signed Sep 15, 2021
Lagos	2021	Sep	Signed Sep 20, 2021
Delta	2021	Sep	Passed Sep 30, 2021
Ogun	2021	Oct	Signed late 2021
Akwa Ibom	2021	Oct	Adopted post-SGF 2021

A.4 Pastoral Zone Classification Details

I classify LGAs as pastoral using a composite criterion:

1. **Conflict-based:** LGAs with ≥ 2 non-state violence events (UCDP Type 2) during 2010–2015. This "revealed preference" measure captures areas where farmer-herder conflict actually occurred.
2. **Geographic:** All LGAs in the eight Middle Belt states (Benue, Plateau, Nasarawa, Taraba, Kaduna, Niger, Kogi, Kwara), which lie along primary north-south transhumance corridors.

The union of these criteria yields 193 pastoral LGAs (24.9% of 775). Both criteria use only pre-treatment information, preventing contamination by post-treatment dynamics.

B. Identification Appendix

B.1 Pre-Trends Test

The Callaway-Sant'Anna event study in Figure 1 shows pre-treatment coefficients at periods $e = -5$ through $e = -2$ that are individually insignificant, with wide confidence intervals that include zero throughout. No individual pre-treatment coefficient is statistically significant at the 10% level. A formal joint test of all pre-treatment coefficients is infeasible due to the singular covariance matrix that arises when the number of treatment cohorts is small relative to the number of event-study periods—a well-known limitation of staggered DiD estimators with few treated groups (Roth et al., 2023). The absence of individually significant pre-treatment effects, combined with the null placebo results on state-based violence, provides the available evidence for the identifying assumption.

B.2 Treatment Map

Figure 8 displays the geographic distribution of anti-grazing law adoption across Nigerian states. Early adopters are concentrated in the Middle Belt and southeast, while the SGF wave encompasses the entire southern region. Never-treated states are predominantly in the north and far south (Bayelsa, Cross River). Total non-state violence event counts are overlaid to illustrate the spatial concentration of conflict.

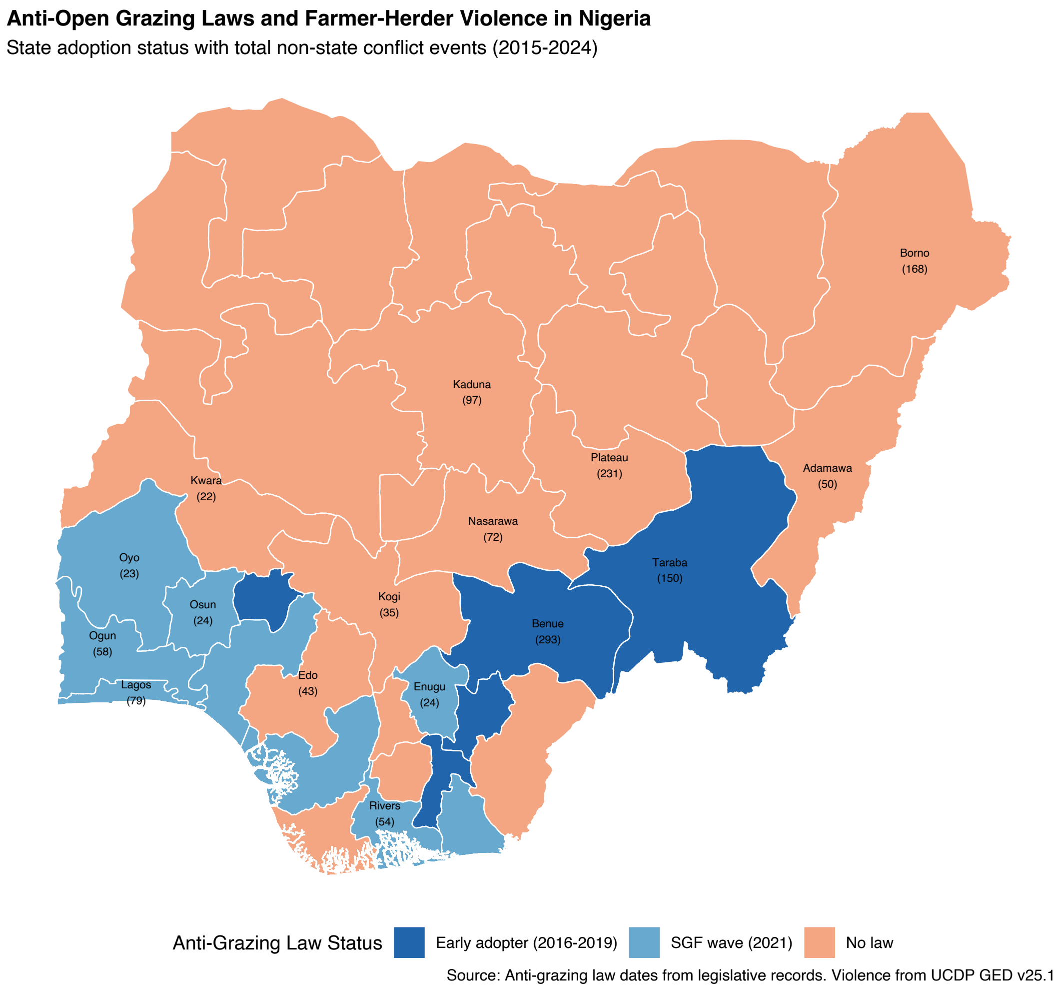

Figure 8: Anti-Open Grazing Laws and Farmer-Herder Violence in Nigeria
Notes: States colored by anti-grazing law adoption status. Numbers in parentheses show total non-state conflict events 2015–2024. Source: Anti-grazing law dates from legislative records; violence from UCDP GED v25.1.

C. Robustness Appendix

C.1 Randomization Inference Details

The randomization inference procedure uses a cohort-preserving design. In each of 1,000 iterations, I: (1) randomly select 14 of 37 states as "treated," and (2) randomly assign the exact 14-element cohort vector of actual treatment years to these states. This preserves the staggered adoption structure, ensuring that each permutation maintains the same number of states in each treatment cohort as the actual data.

The permutation distribution (Figure 4) has a mean of -0.271 and standard deviation of 0.244. The negative mean reflects a structural feature of the design: Middle Belt states

(Kaduna, Kogi, Kwara, Nasarawa, Niger, Plateau) are 100% pastoral, so when treatment is permuted to these states, the interaction $D_{st} \times P_i$ is partially absorbed by state-by-year fixed effects, mechanically shifting the permutation distribution leftward. The observed coefficient (−0.480) lies 0.86 standard deviations below the permutation mean. The two-sided RI p-value is 0.183. While this does not reject the sharp null at conventional levels, the test is conservative due to the structural centering issue. The cluster-robust p-value of 0.003 provides the primary inference.

C.2 Leave-One-State-Out Details

Figure 5 shows the DDD coefficient from 14 jackknife replications, each excluding one treated state. The estimates are remarkably stable:

- Minimum: −0.546 (excluding Rivers)
- Maximum: −0.292 (excluding Lagos)
- Full sample: −0.480

No single state shifts the estimate by more than 0.19 in either direction. The result is particularly robust to excluding Benue (−0.480 unchanged), confirming that the finding is not driven by the state with the most publicized anti-grazing law and the most dramatic post-law violence episodes.

C.3 SGF Sub-Sample Analysis

The SGF sub-sample restricts the analysis to seven states that adopted laws following the Southern Governors' Forum resolution (Ondo, Rivers, Enugu, Osun, Lagos, Delta, Ogun) and 23 never-treated states. This yields 9,390 LGA-year observations (626 LGAs $\times$ 15 years). The DDD coefficient is −0.546 (SE = 0.194, p = 0.008), slightly larger than the full-sample estimate. The SGF sub-sample is appealing because adoption was driven by collective political dynamics rather than individual states' violence trajectories, providing quasi-exogenous variation.