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Criminal Order, Ports, and Coca Booms: The Onset, Amplification, and Persistence of Ecuador's Territorial Homicide Divide (2014–2025)

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Abstract

This article examines homicidal violence in Ecuador by distinguishing three moments of a trajectory—onset, amplification, and persistence—and by asking how a transnational supply shock becomes territorially differentiated. Using administrative microdata (2014–2025; N = 39,819), we estimate difference-in-differences event studies on four spatiotemporal panels that combine provinces and cantons with monthly, quarterly, and annual frequencies. With 2019 as the reference year, joint Wald tests of all pre-treatment coefficients do not reject parallel trends in any panel. The Coast–interior divide opens in the last quarter of 2020—before the assassination of the leader of Los Choneros and before the change of government in May 2021—amplifies through 2023, and persists without reconvergence through December 2025; using 2020 as the reference, a robustness check, delays the dated onset to April 2021 because it absorbs the late-2020 rise into the baseline. Interacting coastal exposure with the log of Colombian coca cultivation shows that Colombia's two coca booms are not equivalent: during the first (2014–2019) the Coast's homicide trajectory is unrelated to coca supply, whereas during the second (2020–2025) it tracks it closely (1.8–2.2 log points per unit of ln coca across scales). Proximity to the nearest seaport reproduces the pattern at the cantonal scale, but coastal membership absorbs most of the effect. The results are consistent with a supply shock whose territorial transmission depended on the prior collapse of the criminal order.

Keywords: Homicide; Organized crime; Cocaine supply; Ports; Subnational transnationality; Ecuador

Between 2010 and 2018, Ecuador reduced its homicide rate to approximately 5.8 per 100,000 inhabitants, one of the lowest in South America. Five years later, the rate exceeded 45 homicides per 100,000 inhabitants. The transformation was accompanied by recurrent prison massacres, the assassination of a presidential candidate during the 2023 campaign, and the armed takeover of a television channel in January 2024 (Dressler & Wolff, 2024; International Crisis Group, 2025; Pontón Cevallos, 2022). Yet speaking of a "national crisis" conceals considerable subnational variation: violence did not rise in the same way across the country. Its expansion was uneven both territorially and temporally, with a growing concentration on the Coast. Rather than a homogeneous escalation, Ecuador experienced a divide in its subnational trajectories of violence.

This distinction matters for the study of illegal economies and organized crime. The Latin American literature has shown that the presence or expansion of illicit markets does not mechanically translate into higher levels of violence. Their effects depend on how those markets are organized territorially, on the relationships among criminal organizations, and on the state capacities that regulate, tolerate, or contest those orders (Díaz-Román & Laverde-Rodríguez, 2023; Snyder & Durán-Martínez, 2009; Trejo & Ley, 2020; Viscardi & Tenenbaum, 2023; Yashar, 2018). From this perspective, homicidal violence is not merely an indicator of the size of an illegal economy. It can also be the manifestation of the breakdown of a previously stabilized criminal order and of the competition that follows that breakdown. This shift in perspective is pertinent for Ecuador, where the country's insertion into transnational drug-trafficking

chains produced territorially differentiated effects and turned several coastal provinces into strategic nodes of transit and export (Pino-Uribe et al., 2025). The recent rise in homicides also coincided with Ecuador's consolidation as a platform for international cocaine trafficking, the fragmentation of criminal organizations, and the deterioration of the prison system (Carrión Mena & Enríquez Bermeo, 2022; Pontón Cevallos, 2022; Rivera-Rhon & Bravo-Grijalva, 2020).

The problem is that much of this literature identifies plausible mechanisms but is less precise about their sequence. Available explanations have emphasized the expansion of drug trafficking, the December 2020 assassination of Jorge Luis Zambrano (alias "Rasquiña"), leader of Los Choneros, the ensuing disputes among criminal organizations, the prison crisis, and the weakening of state capacities. Several studies, however, place the break only loosely "around 2021." This temporal aggregation renders simultaneous processes that were not, and makes observationally compatible explanations that carry different causal implications. Zambrano was murdered in December 2020, the first major prison massacre occurred in February 2021, and Guillermo Lasso took office as president in May of that year. With annual data, all of these events fall within a single observation: homicides rose in 2021.

Strikingly, several of the structural conditions that made the Coast especially exposed to the subnational transnationality of illicit economies—port infrastructure, connections to international cocaine routes, and weak state control—existed well before 2020. Why, then, did a relatively persistent geography of exposure produce an abrupt homicidal break at that particular moment? Explaining where violence concentrates is not the same as explaining when a territorial trajectory breaks. Likewise, identifying the conditions that favor the expansion of an illegal economy does not, by itself, establish which event triggers an escalation of violence.

This precision allows us to distinguish three moments that tend to appear aggregated in accounts of Ecuador's security crisis: the onset of the divide, its amplification, and its persistence. The fragmentation of a criminal order can generate a divergence; state capacities can subsequently contain or amplify it; and additional processes can explain why a dynamic endures for several years. Onset, escalation, and persistence are therefore distinct outcomes, and they need not respond to the same mechanism. Before explaining why violence reached the levels observed in 2023, or why it continued in the following years, a prior fact must be established: when did the Coast's homicide trajectory begin to separate from that of the rest of the country?

The temporal sequence makes it possible to discriminate among some of the available explanations. If the divergence begins after Guillermo Lasso took office in May 2021, an explanation that assigns a decisive role to the governmental transition remains plausible. If the divide was already observable earlier, that family of explanations is weakened by the sequence of onset, and those that locate the break in the reorganization of the criminal order during the second half of 2020 gain plausibility. This does not mean that the sequence alone causally identifies alternative mechanisms. Nonetheless, dating the break makes it possible to rule out explanations whose timing is incompatible with the data and to establish which remain consistent with the evidence.

Answering this question also requires attention to territorial variation. Annual aggregation can show that Ecuador changed between 2020 and 2021, but it cannot establish whether the break preceded or followed events separated by only a few months. An equivalent problem arises in space. A provincial pattern can hide the concentration of violence in a few cities. Guayaquil, for example, accounts for about one-third (32.7%) of coastal homicides, and its province, Guayas, for just over half (51.4%), so an

apparent Coast–interior divide could in fact be a metropolitan phenomenon amplified by aggregation. The modifiable areal unit problem (Openshaw, 1984) is therefore not merely a statistical threat: it directly affects which phenomenon we believe we are explaining. A national crisis, a regional divergence, and an urban concentration are different empirical objects.

A further question concerns the source of the divide's timing and magnitude. Ecuador is a transit and export platform for cocaine produced mainly in Colombia, and Colombian coca cultivation has gone through two booms in the period studied: an expansion from about 69,000 hectares in 2014 to a first peak of about 171,000 in 2017, followed by a decline to about 143,000 in 2020, and a second, larger expansion to about 261,000 in 2024. If the Coast's violence responded mechanically to the volume of cocaine moving through its ports, both booms should leave a footprint. If instead the effect of supply depends on the stability of the criminal order that governs the territories through which it flows, the two booms should differ. We test this contrast by interacting coastal exposure—and, alternatively, cantonal proximity to the nearest seaport—with the log of Colombian coca cultivation.

To discriminate among these problems, we use administrative microdata on intentional homicides from Ecuador's Ministry of the Interior for January 2014 through December 2025, which contain 39,819 individual georeferenced records. We estimate a difference-in-differences event study (Callaway & Sant'Anna, 2021; Roth et al., 2023) on four panels that deliberately vary spatial and temporal resolution: province by month, province by year, canton by quarter, and canton by year. The seven coastal provinces constitute the treated group and the remaining seventeen the comparison group. All models use 2019 as the reference year, the last full year before the criminal order began to break down, and we test the joint null that all pre-treatment coefficients are zero; 2020 is used as an alternative reference in robustness checks. The province-by-month panel allows us to date the break; the cantonal models establish whether the divide extends beyond the main urban centers; and the lower-frequency specifications verify that the result does not depend on a particular way of aggregating time. We then add the Colombian coca series and a continuous measure of port proximity to the design.

The results identify a precise sequence. With 2019 as the reference, the divide opens in the last quarter of 2020: the canton-by-quarter panel is significant from Q4 2020 ($\delta = +0.22$; $p = 0.001$) in every subsequent quarter, and in the province-by-month panel the first significant differentials appear in October and November 2020 ($\delta = +0.42$ and $+0.50$), becoming continuous from March 2021. The onset therefore precedes both the assassination of Jorge Luis Zambrano at the end of December 2020 and Guillermo Lasso's inauguration in May 2021, which excludes the governmental transition as its origin and suggests that the assassination was part of a reorganization of the criminal order already under way rather than its starting point. Once opened, the gap grows between 2021 and 2023, with peak coefficients between +1.22 and +1.81 log points depending on the specification, and remains through December 2025. The onset also precedes the second Colombian coca boom, which starts in 2021: the coca interaction shows that the first boom (2014–2019) is unrelated to the Coast's trajectory, while during the second (2020–2025) the Coast's homicides move with Colombian coca supply. Port proximity reproduces this pattern at the cantonal scale but largely acts as a proxy for coastal membership. Using 2020 as the reference delays the first significant month to April 2021 without altering the rest of the trajectory.

The article shows that Ecuador's current crisis should be understood as a territorial divide whose onset, amplification, and persistence need to be distinguished, and in which the transmission of a transnational

supply shock depends on the state of the local criminal order. The design identifies the moment at which the divide opens and restricts the set of explanations compatible with its sequence, without attributing the totality of its escalation to a single mechanism. Methodologically, we show that varying temporal and spatial scales can be used as an inferential strategy, and that the choice of reference period, assessed through joint pre-trend tests, can shift the dated onset by several months.

1. Criminal violence and territorial trajectories: from exposure risk to persistence

The relationship between illegal economies and lethal violence is not automatic. Illicit markets may resort to coercion as a regulatory mechanism precisely because they operate outside legal dispute-resolution mechanisms, but their existence does not necessarily lead to equivalent levels of homicidal violence (Goldstein, 1985; Reuter, 2009; Durán-Martínez, 2018). Evidence on illegal markets in Latin America has also shown that supply shocks, prices, and the expansion of drug trafficking produce different effects depending on territorial conditions and on the structure of competition among criminal organizations (Angrist & Kugler, 2008; Castillo et al., 2020). The question, therefore, is not only how much an illicit economy grows, but under what conditions its operation becomes a source of persistent violence.

A second line of research has shown that this relationship depends on how the criminal order is organized. Large illicit markets can coexist with contained or even low levels of violence when stable agreements exist among criminal organizations or when protection arrangements exist with state actors; when those equilibria break down, competition can take far more violent forms (Durán-Martínez, 2018; Snyder & Durán-Martínez, 2009). The fragmentation of dominant organizations, the multiplication of competitors, and the transformation of protection networks can alter the incentives to use violence (Trejo & Ley, 2020). Likewise, state responses do not operate on empty terrain: certain coercive interventions can alter criminal equilibria, displace organizations, or intensify confrontations that were previously contained (Lessing, 2017).

This literature helps explain why violence associated with illicit economies has a territorial dimension. State capacities, available infrastructure, routes of commodity circulation, and local modalities of criminal competition generate different configurations within a single country (Díaz-Román & Laverde-Rodríguez, 2023; Viscardi & Tenenbaum, 2023; Yashar, 2018). In the Ecuadorian case, the expansion of drug trafficking is linked to territories strategically connected to transnational corridors, port infrastructure, and international markets (Carrión Mena & Enríquez Bermeo, 2022; Rivera-Rhon & Bravo-Grijalva, 2020). This territorial dimension can also be observed through patterns of geographic clustering. Trejo and Ley (2021) show that criminal violence concentrates in nearby regions and municipalities, and that attacks in adjacent municipalities increase the probability of new attacks in neighboring units. This pattern is consistent with processes of territorial expansion and the establishment of subnational forms of criminal governance.

Geographic clustering characterizes the spatial proximity of violence, whereas subnational transnationality emphasizes the connection between specific territories and criminal circuits that cross borders. The latter is more pertinent for the Ecuadorian case because it allows us to consider the role of strategic infrastructure, such as ports and logistical corridors, that links subnational territories to the transnational chains of drug trafficking. Within this geography, the roads connecting these nodes to other territories also become relevant: the Observatorio Ecuatoriano de Crimen Organizado (2025)

identifies a concentration of cantons with high homicide rates around the key highways for cargo traffic to the ports, including the E25, or Troncal de la Costa. However, recognizing that certain territories are more exposed to illegal economies does not resolve an important temporal problem: relatively persistent structural conditions can coexist for years with contained levels of violence and subsequently produce abrupt ruptures.

This tension suggests approaching criminal violence as a trajectory rather than as a difference in levels. A trajectory implies a sequence: certain conditions precede the change, an event or set of events alters the prior course, and the consequences generated by that rupture can modify the conditions under which the process subsequently unfolds. The literature on historical processes has shown that the mechanisms responsible for the origin of a trajectory do not necessarily coincide with those that explain its reproduction, and that outcomes produced during one phase can become causal conditions for the next (Mahoney, 2000; Pierson, 2000). This perspective does not imply transposing historical institutionalism to the study of organized crime in Ecuador, at least not in this article. Its usefulness is more limited: it allows us to treat sequence, accumulation, and feedback as dimensions of explanation.

From this perspective, a trajectory of violence linked to illegal economies can be examined by distinguishing three moments: onset, amplification, and persistence. These are neither necessary stages nor a new typology of criminal violence. The distinction makes it possible to formulate different causal questions about a single process and prevents structural conditions, organizational shocks, and mechanisms of reproduction from being treated as equivalent explanations.

Onset corresponds to the moment at which a trajectory begins to separate from its prior course. The presence of an illegal economy constitutes a condition of exposure but does not determine when that rupture occurs. A relatively stable equilibrium can be disturbed when an actor capable of regulating relations among organizations disappears, a dominant structure fragments, protection agreements collapse, or the relationship between criminal and state actors changes (Durán-Martínez, 2018; Snyder & Durán-Martínez, 2009; Trejo & Ley, 2020). From this perspective, violence may arise not from a sudden expansion of the illegal market but from the transformation of the structure that previously regulated its operation. The difference is important: the existence of the market helps identify exposed territories, whereas the rupture of the criminal order may help explain the moment at which that exposure begins to produce a distinct homicidal trajectory.

Amplification is a subsequent problem: an initial rupture may remain limited or become a large-scale escalation. The proliferation of rival organizations, disputes over routes and rents, the availability of illicit resources, control of prisons and territories, and the state's capacity to contain competition can all affect the intensity that violence acquires once the break has begun (Durán-Martínez, 2018; Lessing, 2017; Yashar, 2018). The administrative and legal capacities of the state also condition the possibilities of containing violent markets and limiting the territorial expansion of criminal disputes (Jaramillo-Ramón & Curvale, 2026). Consequently, an institutional condition may have little power to explain the exact moment of onset and, at the same time, be central to understanding why an initial rupture subsequently reaches a given magnitude.

This distinction allows a more precise treatment of the Ecuadorian case. The conditions that increase the Coast's exposure to drug trafficking—its port infrastructure, its connection to international routes, and its logistical importance—predate the escalation that began in late 2020 (Carrión Mena & Enríquez Bermeo, 2022; Rivera-Rhon & Bravo-Grijalva, 2020). The fragmentation of the criminal order organized

around Los Choneros, which culminated in the assassination of its leader, Jorge Luis Zambrano, in December 2020, is a mechanism compatible with the onset of a new dynamic of competition, while the prison crisis, the multiplication of organizations, and limits on state capacity offer plausible mechanisms for its subsequent amplification (Arévalo Rueda & Maldonado Ruiz, 2022; International Crisis Group, 2025; Pontón Cevallos, 2022). Distinguishing between the two moments avoids attributing the whole escalation to a single mechanism.

Persistence raises a third question: why a trajectory does not return to its previous levels once the event that contributed to initiating it has disappeared. A crisis can reach high levels and then reconverge, but it can also generate mechanisms that favor its own reproduction. The formation of new organizations, the consolidation of territorial positions, investments in protection networks, cycles of retaliation, and adaptive responses to state intervention can transform the initial conditions of the conflict (Durán-Martínez, 2018; Lessing, 2017; Trejo & Ley, 2020). Once that change has occurred, the original shock is no longer necessary for the trajectory to continue. Outcomes accumulated during the process itself modify the opportunities, resources, and expectations of actors and can generate feedback mechanisms that hinder a return to the earlier equilibrium (Mahoney, 2000; Pierson, 2000).

For this reason, persistence does not mean the prolonged existence of high homicide levels. It refers to the absence of reconvergence with respect to the previous trajectory. Conceptually, this makes it possible to separate two questions that are often conflated: what initially produced the rupture, and what causes that rupture to reproduce itself afterward. The mechanism that explains the origin of a trajectory may disappear, yet its organizational, territorial, and institutional consequences continue to generate violence.

The distinction among onset, amplification, and persistence has a direct consequence for the analysis of illegal economies. A single condition should not be presumed capable of explaining all three dimensions indiscriminately. Infrastructure and transnational routes may explain territorial exposure; the fragmentation of a criminal order may help explain a break; competition among organizations and state capacities may condition its amplification; and the dynamics generated during the crisis itself may contribute to its persistence. These relationships must be established empirically and not deduced solely from the presence of an illegal market.

2. Sequence and scale as an observational strategy

A processual perspective requires attention both to how much territories differ and to how that difference develops over time. Data aggregated annually can show that violence increased during a given year, but they compress into a single observation events separated by several months and potentially linked to different mechanisms. A temporal perspective is therefore useful because it allows us to establish the first moment at which a divergence becomes statistically observable, to follow its subsequent growth, and to determine whether the gap converges again or remains open. Fig. 1 summarizes the framework that guides the analysis, showing the relevance we assign to each mechanism at each of the three moments.

Fig. 1 *Differential activation of mechanisms in Ecuador's homicide divide (2014–2025)*



Note: Rows are stylized mechanisms; shading indicates the relevance assigned to each in each moment. The coca row summarizes the results of Section 5.4 and is interpretive. Source: Authors' own elaboration.

Territorial scale raises a complementary problem: the results of a subnational analysis can depend on how spatial units are delimited, particularly when a few urban centers concentrate a large share of the observed events (Openshaw, 1984). A difference estimated between regions can conceal a metropolitan phenomenon, while an excessively aggregated analysis can turn local heterogeneity into an apparently common trajectory. Deliberately varying the territorial unit makes it possible to distinguish a localized concentration from a more extensive territorial divide.

Combining both dimensions makes it possible to observe the three moments of a trajectory without assuming that the design causally identifies the mechanisms responsible for each. Temporal disaggregation allows us to locate the observable emergence of the divergence; the subsequent evolution of the differential allows us to study its amplification; and its permanence allows us to establish whether or not there is reconvergence within the analytical window. Spatial variation, in turn, allows us to establish the extent to which the trajectory is territorially distributed or depends on a few hotspots of violence.

This strategy identifies a sequence, though it cannot establish causally all of the mechanisms that produce it. Our argument is that the homicide crisis should be understood as a trajectory composed of three distinct moments—onset, amplification, and persistence—that need not respond to the same causes. The temporal sequence allows us to rule out explanations incompatible with the timing of the break and to establish which are plausible for its onset; the subsequent evolution of the gap allows us to

distinguish the processes that may have contributed to amplifying and sustaining it. Identifying those mechanisms requires additional strategies. The contribution of this approach therefore lies in separating what must be explained at each moment before attributing the entire trajectory of violence to a single cause.

3. Supply shocks and territorial exposure: expectations

The literature on subnational transnationality suggests that transnational drug markets are anchored in specific territories—ports, corridors, border areas—whose institutional conditions shape how supply shocks are absorbed (Pino-Uribe et al., 2025; Jaramillo-Ramón, 2026). Two implications follow for Ecuador. The first is a spatial one: the effect of a given increase in the supply of cocaine should be larger where the territory is more tightly integrated into export circuits, which we operationalize through the Coast and, more finely, through proximity to the nearest seaport. The second is a temporal one: because the same territories were exposed to Colombian supply during both coca booms, a mechanical supply account predicts a similar association in both.

The processual argument advanced above predicts otherwise. Until 2019 a relatively stable criminal order could absorb larger flows without generating a territorial differential in lethal violence (Durán-Martínez, 2018; Snyder & Durán-Martínez, 2009). After the rupture of that order, the same exposure channel becomes a channel for competition. We therefore state two hypotheses. H1 (differential booms): the association between coastal exposure and Colombian coca cultivation is statistically indistinguishable from zero during the first boom (2014–2019) and positive during the second (2020–2025). H2 (port gradient): within the second window, the association increases with a canton's proximity to a seaport, so that a continuous port-proximity measure reproduces, and refines, the Coast–interior contrast.

Both hypotheses are tests of association between two time series and a territorial exposure, not of the causal effect of Colombian supply. We return to this limit in the discussion.

4. Data and empirical strategy

4.1 Data

The primary source is the individual administrative records of intentional homicides from Ecuador's Ministry of the Interior between January 2014 and December 2025 (data update of May 15, 2026), comprising 39,819 cases georeferenced at the canton level and dated to the day. The microdata make it possible to reconstruct the trajectory of violence by combining different territorial units and temporal frequencies. Intentional homicides also offer greater temporal and territorial comparability and less underreporting than other indicators of lethal violence (Mejía & Restrepo, 2013; Trejo & Ley, 2020). The treated group comprises the seven provinces of the Coast; the comparison group comprises the seventeen remaining provinces of the Highlands, the Amazon, and Galápagos.

4.2 Four scales of aggregation

Every subnational analysis faces the modifiable areal unit problem: results can vary according to how territory is aggregated (Openshaw, 1984). In Ecuador, the threat is concrete. The canton of Guayaquil alone accounts for about one-third of coastal homicides (32.7%), and the province of Guayas for just

over half (51.4%), so a provincial effect could conceal a metropolitan phenomenon. Temporal frequency poses an equivalent problem: annual aggregates can obscure the moment of the break, while monthly data, with small cells and abundant zeros, introduce more noise (Chen & Roth, 2024).

Difference-in-differences event studies make it possible to use this variation in scale to reconstruct the trajectory of the divergence. Explicit reference periods allow us to assess pre-trends (Roth et al., 2023), while simultaneous treatment avoids the problems of staggered designs (Callaway & Sant'Anna, 2021; de Chaisemartin & D'Haultfœuille, 2023). Deliberately varying spatial and temporal resolution serves an additional function here: it allows us to distinguish the onset of the divide, to follow its amplification, and to establish whether it persists beyond a few territorial hotspots.

We construct four balanced panels that combine two geographic units—province and canton—with monthly, quarterly, and annual frequencies. Each specification serves a different function: the province-by-month panel allows us to date the onset more precisely; the cantonal panels establish the territorial extent of the divergence; and the quarterly and annual frequencies allow us to follow its amplification and persistence under different levels of aggregation (Table 1). Convergence among them allows us to assess whether the observed trajectory depends on a particular scale.

Table 1 *The four event-study specifications*

Specification	Unit	Frequency	Units (N)	Periods (T)	Observations
A — Province × month	Province	Monthly	24	144	3,456
B — Province × year	Province	Annual	24	12	288
C — Canton × quarter	Canton	Quarterly	222	48	10,656
D — Canton × year	Canton	Annual	222	12	2,664

Note: Treated group: the 86 cantons of the seven coastal provinces (Esmeraldas, Manabí, Santa Elena, Guayas, El Oro, Los Ríos, and Santo Domingo de los Tsáchilas). Control group: the remaining 17 provinces (136 cantons). The cantonal panels include six cantons with no recorded homicide in 2014–2025 (one coastal). Exposure models exclude the three Galápagos cantons (219 cantons; 23 provinces). Source: Authors' own elaboration based on Ministry of the Interior records.

4.3 Model and dependent variable

For each specification we estimate the event study

$$\log(1 + H_{it}) = \alpha + \beta \cdot \text{Coast}_i + \Sigma\tau\ \gamma\tau \cdot 1(t = \tau) + \Sigma\tau\ \delta\tau \cdot 1(t = \tau) \times \text{Coast}_i + \lambda_i + \varepsilon_{it} \qquad (1)$$

where H_{it} is the homicide count of unit i in period t; λ_i are unit fixed effects, which absorb all stable differences among provinces or cantons (size, history of violence, port exposure); and the $\gamma\tau$ capture common temporal shocks. The coefficients of interest are the $\delta\tau$, which measure how much more (or less) violent the Coast was in each period τ relative to the control group and to the reference period. If trends were parallel before the break, the pre-treatment $\delta\tau$ should be small and statistically insignificant; if the Coast diverges afterward, the post-treatment $\delta\tau$ will be positive and significant. The virtue of the design is that it indicates not only whether there was a difference but exactly when that difference ceased to be attributable to chance. Standard errors are heteroskedasticity-robust, using the

HC3 variant, which is appropriate for cells with small counts. In the port-exposure models $Coast_i$ is replaced by a standardized proximity measure (Section 4.5).

The dependent variable is $\log(1 + H_{it})$ rather than the simple logarithm, for a structural reason in the data: the share of cells with zero homicides is 25% in the province-by-month panel and 56% in the canton-by-quarter panel. The logarithm of zero is undefined, and discarding those cells would bias the estimation toward the most violent places. The log(1 + H) transformation retains all observations at the cost, documented by Chen and Roth (2024), that the coefficients do not admit an exact elasticity interpretation. We therefore interpret them as approximations of proportional change, also report the implied factor $\exp(\delta)$, and verify that the qualitative pattern—date of the break, trajectory, absence of reversal—is identical across the four specifications, whose shares of zeros differ drastically.

4.4 The choice of reference period

The most important design decision is the reference period: the "zero" against which all coefficients are measured. A poorly chosen reference period—one that is itself abnormally violent or abnormally calm—produces a spurious pattern that falsely suggests (or conceals) a violation of parallel trends (Roth et al., 2023). Rather than fixing it by convention, we subjected it to an empirical test. For each candidate, we estimate the event study and test the joint null hypothesis that all pre-treatment coefficients are equal to zero, using a Wald test with the HC3 covariance matrix. A joint test is preferable to counting individually significant coefficients because it accounts for the covariance among the estimates and avoids the multiple-testing problem of inspecting dozens of coefficients one by one. The same joint test is reported for every specification in the article. A rejection signals a violation of parallel trends; a failure to reject is not proof that the assumption holds, since pre-tests can have low power (Roth, 2022).

Four candidates were evaluated (Table 2). The candidates were chosen to test whether the change of government in May 2021 was relevant for the dating of the divide, since a baseline that includes the months around Lasso's inauguration could either hide or mimic a governmental effect. Omitting a single period (May 2021, the month Lasso took office)—the most widespread convention—rejects the null of parallel pre-trends in the province-by-month specification ($\chi^2(84) = 110.1$, $p = 0.030$), because any individual month is too noisy a baseline. Omitting the January–May 2021 block, the months leading up to and including the transition, also rejects it ($\chi^2(84) = 115.6$, $p = 0.013$), because those months were already abnormally violent and shift the "zero" upward. Treating the change of government as the reference point therefore produces a baseline that is itself contaminated by the process we want to date. Omitting a whole year does not reject the null, either 2019 ($\chi^2(60) = 59.6$, $p = 0.490$) or 2020 ($\chi^2(72) = 72.8$, $p = 0.451$). Both are full years that end before the inauguration, so they allow us to ask whether the divide was already under way before the governmental transition rather than assuming that it began with it. We adopt 2019 as the reference year for all models. It is the last full year before the criminal order began to break down, it passes the joint test in all four panels (Table 3), and it does not place in the baseline any month of the period whose dating is at stake. Using 2020 would put the last quarter of that year—already elevated on the Coast—into the "zero," and the cantonal panels reject or nearly reject parallel trends under it. We therefore report 2020 only as a robustness check (Section 5.6 and Appendix).

Table 2 *Joint test of parallel pre-trends by reference-period candidate (specification A)*

Reference-period candidate	Wald χ^2	df	p-value	Parallel trends rejected at 5%?
Single month (May 2021)	110.1	84	0.030	Yes
January–May 2021 block	115.6	84	0.013	Yes
Full year 2019	59.6	60	0.490	No
Full year 2020	72.8	72	0.451	No

Note: Joint Wald test (HC3 covariance matrix) of the null that all pre-treatment coefficients, i.e., those preceding the omitted period(s), are zero. Only omitting a full year (2019 or 2020) fails to reject the null; 2019 is adopted as the reference year and 2020 is used as a robustness check. Source: Authors' own elaboration.

4.5 Coca-shift and port-exposure designs

Colombian coca cultivation is taken from the UNODC–SIMCI annual census for 2014–2024 and from the SIIMA–Policía Nacional figure for December 2025, which is not strictly comparable with the earlier series (Fig. 4a). Let Coca_t denote hectares in year t. Because Coca_t varies only over time, its main effect is absorbed by period fixed effects, and the parameter of interest is the coefficient on its interaction with territorial exposure:

$$\log(1 + H_{it}) = \lambda_i + \gamma_t + \theta \cdot (E_i \times \ln \text{Coca}_t) + \varepsilon_{it} \qquad (2)$$

where E_i is either the Coast dummy or a standardized measure of port proximity, and the model is estimated on annual data (specifications B and D) or on the annual coca series assigned to every month or quarter within the year (A and C). Pooling both booms in one regression would conflate the slope with the level shift that coincides with the 2020 break, so we estimate (2) separately within Window 1 (2014–2019) and Window 2 (2020–2025), and additionally in Window 2 restricted to 2021–2025. Period fixed effects remove any common national shock, so the coefficient is identified from whether territories with higher exposure move differentially with coca within each window.

Port proximity is built from the minimum distance between each canton and eight seaports (San Lorenzo, Esmeraldas, Bahía de Caráquez, Manta, La Libertad, Guayaquil, Posorja, and Puerto Bolívar). We define E_i as the negative of the standardized distance, or of the standardized log-distance, so that a positive coefficient means that closer cantons are more affected. Provincial exposure is the mean of cantonal minimum distances. The Galápagos cantons, about 1,000 km from the nearest port, are excluded from exposure models because they would dominate the standardization; this leaves 219 cantons (86 coastal, 133 interior) with mean minimum distances of 58 and 163 km, respectively. For the Coast dummy models all 222 cantons are used. The event-study model (1) is estimated with E_i in place of the Coast dummy for the port-exposure analysis.

The cantonal panel includes the six cantons that recorded no homicide in the whole period (one coastal) so that the sample covers all 222 cantons; adding them has a negligible effect on every estimate.

4.6 Inference

Event-study coefficients use HC3 standard errors and are evaluated jointly with a Wald test of all pre-treatment coefficients. Coca-shift models have only 24 provinces as clusters of exposure, and the coca

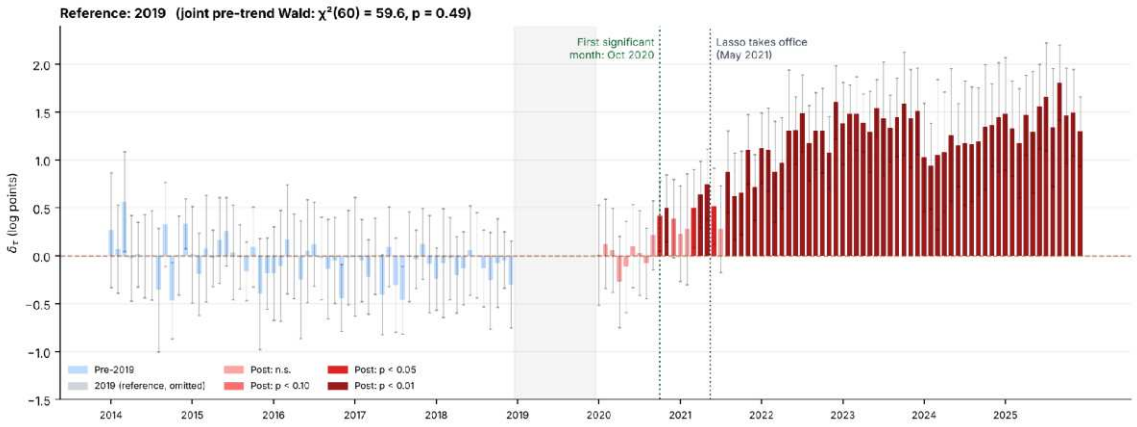
series contains six annual observations per window, so we do not rely on conventional standard errors. We report province-clustered standard errors with a t(G−1) reference distribution, p-values from the Webb (2014) wild cluster bootstrap-t with the null imposed (Cameron et al., 2008), and, for the annual panels, permutation p-values obtained by reassigning the years of the coca series within each window (6! = 720 orderings). We also report a specification with the lagged coca series, one with an exposure-specific linear trend in Window 2, and a horse-race of the Coast dummy against port proximity.

5. Results

5.1 The break, month by month

Fig. 2 presents the specification with the highest temporal resolution—province by month (specification A). Before 2019 the coefficients are small and distributed around zero, with no systematic anticipatory trend, and the joint Wald test does not reject parallel trends ($\chi^2(60) = 59.6$, $p = 0.49$). The coefficients for the first nine months of 2020 are also indistinguishable from zero. From the last quarter of 2020 onward they become positive, increasing, and statistically significant.

Fig. 2 *Monthly event study at the province level (specification A), reference year 2019*



Note: δτ coefficients relative to 2019; error bars are 95% confidence intervals with HC3 standard errors. The first significant month is October 2020. The same specification with 2020 as the reference is shown in Fig. A1. Source: Authors' own elaboration based on Ministry of the Interior records.

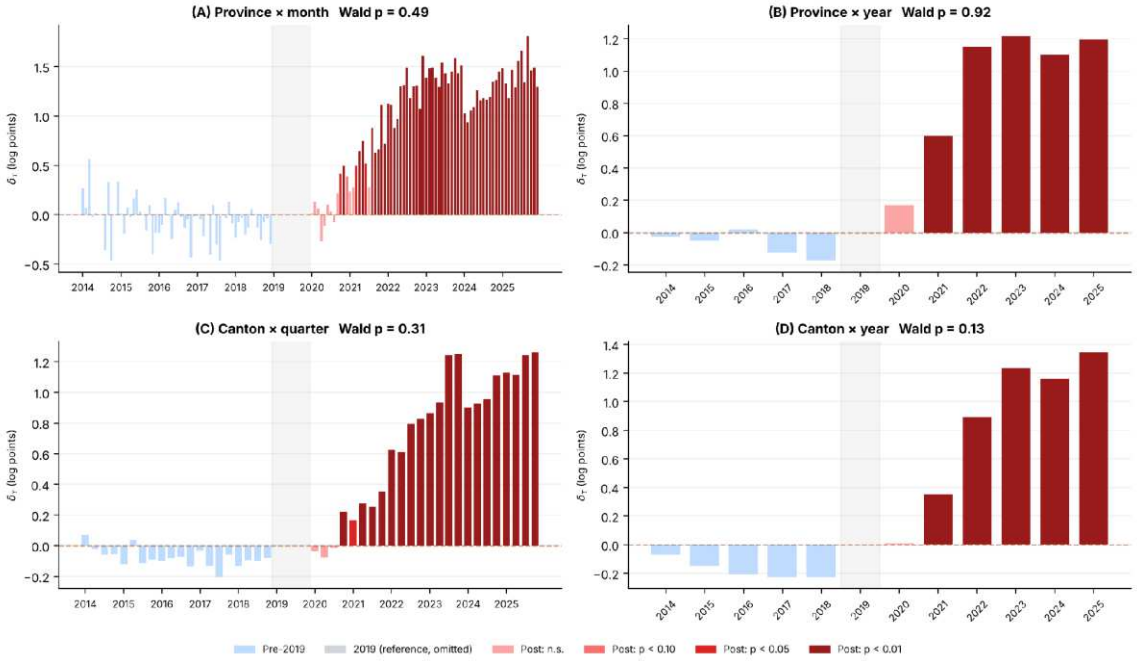
The divide opens in the last quarter of 2020. The first significantly positive months are October 2020 (δ = +0.42; p = 0.025) and November 2020 (δ = +0.50; p = 0.005); December is positive and marginal (δ = +0.39; p = 0.062), and the first two months of 2021 are positive but imprecise (δ = +0.23 and +0.28). From March 2021 the coefficients are significant in every month except July 2021. The quarterly cantonal panel, which is less noisy, dates the onset unambiguously: the coefficient for Q4 2020 is +0.22 (p = 0.001), the three previous quarters of 2020 are close to zero, and every subsequent quarter is significant (Section 5.2). The onset thus precedes Lasso's inauguration in May 2021 by six to seven months, so the governmental transition cannot explain the beginning of the divide. It also precedes the assassination of Zambrano at the end of December 2020 and the first major prison massacre in February 2021. These events are better read as part of a reorganization of the criminal order that was already producing a territorial differential in lethal violence, and as accelerants of the divergence in early 2021,

than as its starting point. The design does not identify the causal effect of any of these events, but it restricts the plausible explanations for the timing of the onset.

5.2 Robustness across scales

Fig. 3 presents the four specifications and Table 3 summarizes their main indicators. All show the same general trajectory: coefficients close to zero before the break; an observable divergence from the last quarter of 2020 (October 2020 in A and Q4 2020 in C) or, in the annual panels, from 2021; sustained amplification through 2023; and no reconvergence through December 2025. The annual panels date the break to 2021 because the four months of divergence in late 2020 are averaged with eight months without it (2020 coefficients: +0.17 in B and +0.01 in D, both insignificant). Varying the scale thus makes it possible to observe different dimensions of a single process without making the finding depend on a particular spatial or temporal aggregation. The joint pre-trend tests do not reject parallel trends in any of the four panels (p = 0.49, 0.92, 0.31, and 0.13).

Fig. 3 *The four event-study specifications (reference year 2019)*



Note: (A) province × month; (B) province × year; (C) canton × quarter; (D) canton × year. Titles report the p-value of the joint Wald test of all pre-treatment coefficients. The pattern is the same across scales and frequencies. The same specifications with 2020 as the reference are shown in Fig. A2. Source: Authors' own elaboration.

Table 3 *Comparative summary of the four specifications: main reference year (2019) and robustness (2020)*

Indicator	A: Prov. × month	B: Prov. × year	C: Canton × qtr.	D: Canton × year
Main specification (reference 2019)				

Indicator	A: Prov. × month	B: Prov. × year	C: Canton × qtr.	D: Canton × year
Observations	3,456	288	10,656	2,664
R^2	0.852	0.947	0.764	0.861
Pre-trends: Wald χ^2 (df)	59.6 (60)	1.4 (5)	22.6 (20)	8.6 (5)
Pre-trends: p-value	0.490	0.925	0.309	0.126
First significant period	Oct 2020	2021	Q4 2020	2021
Peak coefficient δ	+1.81	+1.22	+1.26	+1.34
Implied factor exp(δ)	6.1×	3.4×	3.5×	3.8×
Port proximity: Wald p	0.365	0.555	0.960	0.673
Port proximity: first significant period	Dec 2020	2021	Q4 2020	2021
Port proximity: peak δ per SD	+0.71	+0.50	+0.54	+0.53
Robustness (reference 2020)				
Pre-trends: Wald χ^2 (df)	72.8 (72)	3.6 (6)	35.0 (24)	13.4 (6)
Pre-trends: p-value	0.451	0.729	0.068	0.037
First significant period	Apr 2021	2021	Q2 2021	2021
Peak coefficient δ	+1.69	+1.05	+1.23	+1.33
Port proximity: Wald p	0.105	0.400	0.468	0.076

Note: HC3 standard errors in all specifications; all models include unit and period fixed effects. Wald rows report the joint test of the null that all pre-treatment coefficients are zero. With the 2019 reference no panel rejects parallel trends; with the 2020 reference, D rejects at 5% and C is borderline. Port-proximity rows use −z(ln distance) as exposure on the mainland sample. R^2 refers to the Coast-dummy model. Source: Authors' own elaboration.

The comparison across scales makes it possible to specify the territorial extent and temporal sequence of the divide. Spatially, if the divergence were driven exclusively by Guayaquil—which accounts for about one-third of coastal homicides—the effect should dilute in moving from the provincial to the cantonal level. This is not the case: the canton-by-year specification reaches a peak coefficient of +1.34 and the canton-by-quarter specification +1.26. The reduction relative to the province-by-month peak (+1.81) reflects the greater heterogeneity among cantons but shows that the divide extends beyond its main urban hotspot. Temporally, the annual specifications identify the year of the break, while the higher-frequency ones allow us to date its onset and follow its subsequent amplification.

5.3 Magnitude and persistence: 2021–2025

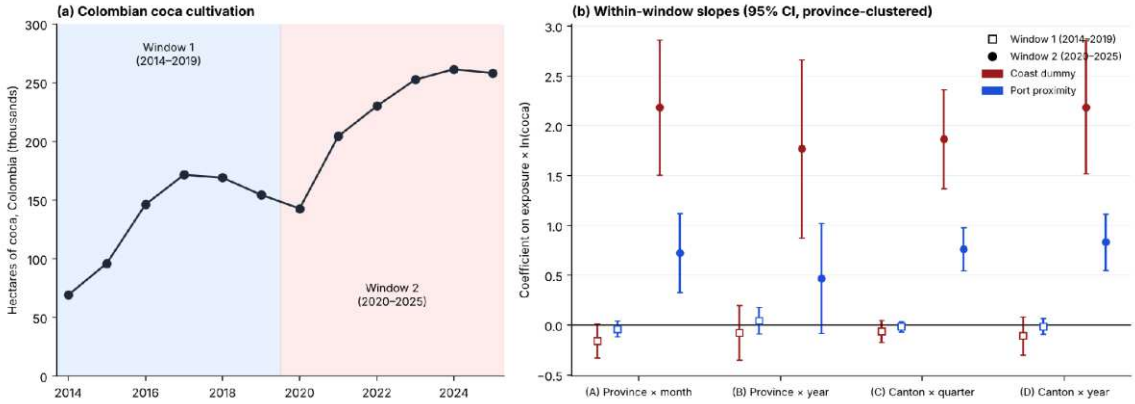
In the highest-frequency specification, the peak coefficient reaches δ = +1.81 in September 2025, equivalent to a factor of approximately 6.1 relative to the interior's trajectory. Even in the more conservative province-by-year specification, the estimated factor is 3.4. The magnitude of the gap shows that the divide that opened in late 2020 not only persisted but amplified considerably over the following years.

Persistence is particularly clear after 2023. Daniel Noboa took office in November of that year and adopted a strategy based on states of exception, military deployment, and the declaration of an "internal armed conflict" in January 2024 (Dressler & Wolff, 2024). The coefficients decline during 2024, but in 2025 they return to 2023 levels or exceed them. In the annual specifications the Coast–interior gap is statistically significant in every year from 2021 onward, and in the monthly specification every month from 2022 onward is significant. The design does not allow us to estimate the causal effect of the policies implemented during this period, but it does establish that, as of December 2025, the trajectory had not reconverged.

5.4 The coca shift: two booms, one effect

Fig. 4 and Table 4 report the within-window coefficients on exposure × ln(coca). In Window 1, which contains the first coca boom, the Coast's interaction with coca is close to zero and statistically insignificant in all four panels (from −0.16 in A to −0.06 in C; wild-bootstrap p between 0.10 and 0.55). In Window 2 it is large, positive, and significant at the 1% level by the wild bootstrap in all four panels, ranging from +1.77 (B) to +2.19 (D). Since ln(Coca) rose by about 0.60 between 2020 and 2024, these estimates imply a Coast differential of roughly 1.1–1.3 log points attributable to the co-movement with coca, which is of the same order as the peak event-study coefficients. Restricting Window 2 to 2021–2025 increases the coefficients (from +2.14 in B to +3.72 in D), as expected if the 2020 trough is a low baseline.

Fig. 4 *Colombian coca cultivation and the Coast's homicide association with it, by window*



Note: Panel (a): hectares of coca in Colombia (UNODC–SIMCI; 2025: SIIMA–Policía Nacional). Panel (b): coefficients on exposure × ln(coca) estimated separately within Window 1 and Window 2, with 95% confidence intervals based on province-clustered standard errors (t with 23 d.f.). Source: Authors' own elaboration.

Table 4 *Exposure × ln(Colombian coca hectares), by window*

Exposure and window	A: Prov. × month	B: Prov. × year	C: Canton × qtr.	D: Canton × year
Coast × ln(coca): Window 1 (2014–2019)	−0.16 (0.08)	−0.08 (0.13)	−0.06 (0.05)	−0.11 (0.09)
Coast × ln(coca): Window 2 (2020–2025)	2.18*** (0.33)	1.77*** (0.43)	1.87*** (0.24)	2.19*** (0.32)
Coast × ln(coca): Window 2, 2021–2025	2.92*** (0.88)	2.14** (1.04)	3.42*** (0.58)	3.72*** (0.78)
Port proximity × ln(coca): Window 1 (2014–2019)	−0.04 (0.04)	0.05 (0.06)	−0.01 (0.02)	−0.01 (0.04)
Port proximity × ln(coca): Window 2 (2020–2025)	0.72** (0.19)	0.47 (0.27)	0.76*** (0.10)	0.83*** (0.14)
Port proximity × ln(coca): Window 2, 2021–2025	0.90** (0.43)	0.58 (0.54)	1.35*** (0.24)	1.42*** (0.30)
Observations (W1 / W2)	1,728 / 1,728	144 / 144	5,328 / 5,328	1,332 / 1,332

Note: Coefficients with province-clustered standard errors in parentheses (24 clusters for the Coast dummy; 23 for port proximity, which excludes Galápagos). Stars: wild cluster bootstrap-t p-values (Webb weights, null imposed) where computed, otherwise clustered p-values (2021–2025 rows). Port proximity is −z(ln distance). * p < 0.10, ** p < 0.05, *** p < 0.01. All models include unit and period fixed effects. Source: Authors' own elaboration.

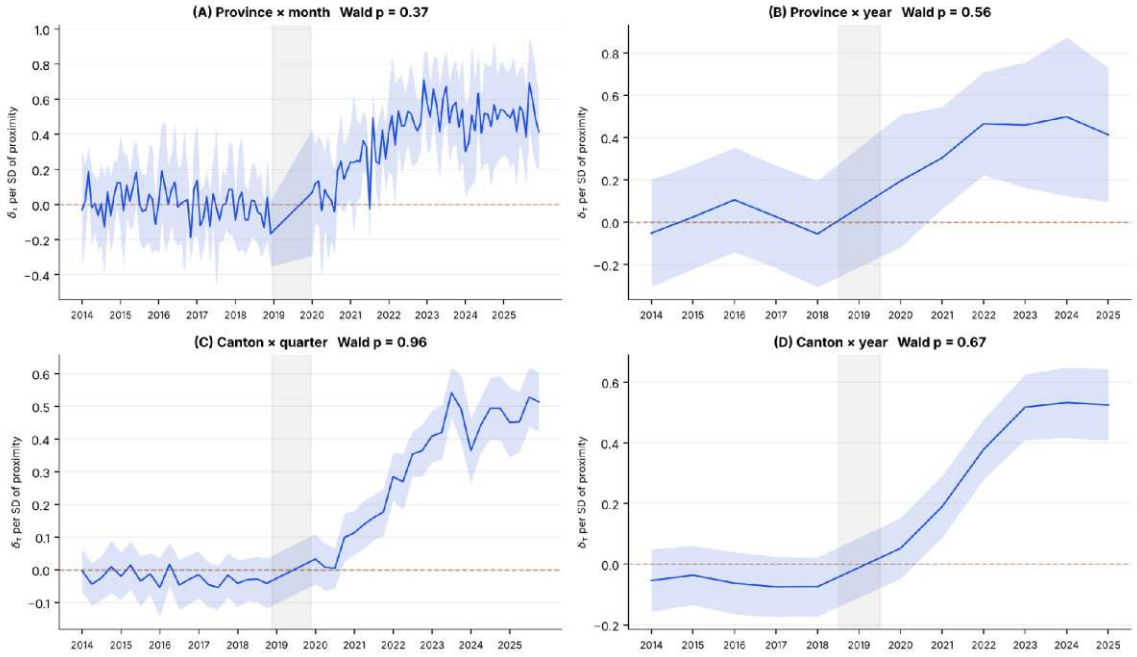
Permutation tests for the annual panels agree with the clustered and bootstrap results: reassigning coca years within Window 2 yields two-sided p = 0.017 (B) and p = 0.006 (D) for the Coast interaction, versus 0.48 (B) and 0.44 (D) in Window 1. The pattern is also unchanged when the lagged coca series is used and when an exposure-specific linear trend is added in Window 2 (Coast coefficient between +0.82 and +1.88; Table 5). Hypothesis H1 is therefore supported: the Coast's association with Colombian coca is present in the second boom and absent in the first.

5.5 Port proximity

Fig. 5 shows the event study with port proximity (log-distance) in place of the Coast dummy. The pre-treatment coefficients are jointly indistinguishable from zero in all panels (Wald p = 0.37, 0.56, 0.96, and 0.67 for A–D). The timing matches that of the Coast dummy: in the canton-by-quarter panel the proximity coefficient first becomes significant in Q4 2020 (+0.10 per standard deviation; p = 0.008) and remains so thereafter. Post-2020 coefficients peak at 0.71, 0.50, 0.54, and 0.53 log points per standard deviation of proximity (Table 3), which corresponds to factors of about 2.0, 1.6, 1.7, and 1.7. The proximity gradient is visible at the cantonal scale: the interaction with coca in Window 2 is +0.76 (C) and

+0.83 (D) per standard deviation (wild-bootstrap p = 0.002), and indistinguishable from zero in Window 1. At the provincial scale it is imprecise (A: +0.72, wild p = 0.036; B: +0.47, wild p = 0.27).

Fig. 5 *Event study with port proximity as exposure (reference year 2019)*



Note: Coefficients on the interaction of period dummies with −z(ln distance to the nearest seaport); shaded bands are 95% HC3 confidence intervals; the gray band marks the reference year. Galápagos excluded. Source: Authors' own elaboration.

The horse-race specification, however, qualifies H2. When the Coast dummy and port proximity enter together (Table 5), the Coast coefficient in Window 2 remains large and significant (+2.15 in A, +1.92 in B, +1.54 in C, +1.95 in D), whereas the proximity coefficient falls to between −0.20 and +0.22 and is insignificant in every panel. In Window 1 the horse-race Coast coefficient is, if anything, slightly negative (−0.28 to −0.08; significant at 5% only in A), so there is no positive association during the first boom under this specification either. Port proximity thus carries information about where the divide is concentrated, but within the Coast it does not explain additional variation in the response to coca once regional membership is controlled. The most conservative reading is that proximity to ports is a good descriptor of exposure but that, in this sample, coastal membership captures a broader set of characteristics—transport infrastructure, urban agglomeration, prior criminal organization—that proximity alone does not.

Table 5 *Robustness of the coca interaction and horse race between Coast and port proximity*

Specification	A: Prov. × month	B: Prov. × year	C: Canton × qtr.	D: Canton × year
Coast × ln(coca), lag-1 coca, W2	1.82*** (0.35)	1.43*** (0.42)	1.75*** (0.25)	1.98*** (0.34)

Specification	A: Prov. × month	B: Prov. × year	C: Canton × qtr.	D: Canton × year
Coast × ln(coca), + Coast linear trend, W2	1.88*** (0.40)	1.77*** (0.51)	0.82*** (0.17)	1.21*** (0.23)
Horse race, W2: Coast × ln(coca)	2.15*** (0.66)	1.92*** (0.75)	1.54** (0.53)	1.95** (0.59)
Horse race, W2: Proximity × ln(coca)	−0.02 (0.32)	−0.20 (0.45)	0.22 (0.21)	0.14 (0.24)
Horse race, W1: Coast × ln(coca)	−0.21** (0.09)	−0.28** (0.14)	−0.08** (0.05)	−0.18** (0.10)
Horse race, W1: Proximity × ln(coca)	0.04 (0.03)	0.14** (0.07)	0.01 (0.02)	0.05 (0.03)

Note: Province-clustered standard errors in parentheses. Horse-race models include both interactions simultaneously on the mainland sample (23 provinces; 219 cantons). Stars use wild cluster bootstrap p-values where computed (horse race, W2) and clustered p-values otherwise. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Source: Authors' own elaboration.

5.6 Robustness: 2020 as the reference year

Using 2020 as the reference year leaves the trajectory intact but shifts its dating (Table 3; Figs. A1 and A2 in the Appendix). In the province-by-month panel the first significant month becomes April 2021 (δ = +0.53; $p = 0.003$), and in the canton-by-quarter panel Q2 2021. The shift is mechanical: the 2020 baseline contains the elevated last quarter of that year, which lowers all subsequent coefficients and removes from the post-treatment period the months in which the divergence began. Peak coefficients are slightly smaller (+1.69 versus +1.81 in A; +1.05 versus +1.22 in B) and the annual panels are unchanged in their dating (2021). Parallel trends are not rejected in the provincial panels ($p = 0.45$ and 0.73) or with port proximity in A–C ($p = 0.10$, 0.40, 0.47), but are rejected in D ($p = 0.037$; 0.077 with port proximity) and borderline in C ($p = 0.068$), which reinforces the choice of 2019 as the main reference. The coca-shift estimates do not depend on the reference year because they use within-window variation. One caution applies to the main specification: under the 2019 reference, specification A shows a small negative linear pre-trend in the Coast–interior gap over the pre-reference years 2014–2018 (−0.05 log points per year; province-clustered $p = 0.007$), which is not detectable in the other panels ($p = 0.065$–0.20) and is not detected by the joint test. We read it as a mild convergence before the break that, if anything, makes the post-2020 divergence conservative.

6. Discussion

The observed trajectory allows us to return to the article's argument: the mechanisms associated with violence can acquire different relevance at different moments of a single process. The starting point is territorial. Drug trafficking operates through a logic of subnational transnationality: networks that cross borders but whose expansion depends on specific territories, strategic infrastructure, and local institutional conditions (Pino-Uribe et al., 2025). This heterogeneity coincides with work showing that

the relationship among illegal markets, homicide, and territory is neither mechanical nor spatially uniform (Díaz-Román & Laverde-Rodríguez, 2023; Viscardi & Tenenbaum, 2023). However, these conditions predated 2020. They help explain where violence could concentrate, but not why the Coast's trajectory broke precisely at that moment.

The first problem is therefore the onset of the divide, which the evidence places in the last quarter of 2020. This date excludes the governmental transition of May 2021 as an origin. It also precedes the assassination of Zambrano at the end of December 2020, which is usually treated as the starting point of the crisis. The sequence is compatible with a rupture of the criminal order that was already under way in the second half of 2020—a period in which the hegemony of Los Choneros was being contested and its leader's position was in question—and of which the assassination would be a culminating episode rather than the trigger. The prison massacre of February 2021 and the dispute among factions such as Los Lobos, Los Tiguerones, and Los Chone Killers then accompanied the consolidation of the divergence in 2021 (Arévalo Rueda & Maldonado Ruiz, 2022; International Crisis Group, 2025). This interpretation coincides with a literature showing that large illegal markets can coexist with contained levels of violence as long as relatively stable arrangements persist, and that their erosion can alter the incentives for violent competition before any single event makes the rupture visible (Durán-Martínez, 2018; Schedler, 2014; Snyder & Durán-Martínez, 2009). Two other features of the onset are relevant. It does not coincide with an increase in Colombian supply: 2020 was the trough between the two coca booms, and the second expansion began in 2021. And the late-2020 differential emerges after the end of the national pandemic restrictions, so part of it could reflect a differential rebound of criminal activity on the Coast; the period fixed effects absorb the national component of that rebound, but not its regional variation. The design does not causally identify the effect of any of these factors, but it restricts the plausible explanations for the timing of the break.

Amplification poses a different problem. Once the divide opened, the gap grew steadily between 2021 and 2023. During those years, the expansion of criminal competition coincided with severe limits on state capacity: the Ministry of the Interior executed 0% of its investment budget in 2022 and 13.75% in 2023, in a context also marked by institutional and prison crises (International Crisis Group, 2025). This reading is in dialogue with work that situates territorial control, governance, and state capacity in the functioning of illegal economies (Laverde Rodríguez et al., 2025) and is consistent with a role for state capacities in amplification rather than necessarily in onset (Jaramillo-Ramón & Curvale, 2026). The design allows us to observe the growth of the gap, but it does not establish how much corresponds to criminal competition and how much to limits on state containment.

Persistence poses a third problem. The measures adopted from late 2023 coincided with a temporary reduction of the gap during 2024, but not with its closure: in 2025 it widened again. This evolution is in dialogue with recent analyses of the limits of predominantly coercive responses to the transformation of Ecuador's security situation (Buckley, 2026). The design does not allow a causal evaluation of those interventions, but it does show that the mechanisms associated with onset are no longer sufficient to explain the continuity of the trajectory. The territorial consolidation of new organizations, cycles of competition and adaptation, and persistent limits on state containment offer plausible mechanisms of reproduction. A consequence of amplification can thus become a condition of persistence: organizations, territorial positions, and networks created during the escalation may subsequently contribute to reproducing it.

The coca and port analyses refine this reading. Because the divide opens before the second coca boom, Colombian supply cannot explain the onset; its role, if any, lies in amplification. The two coca booms are not equivalent for the Coast: during the first, the expansion of Colombian supply did not translate into a differential in Coast homicides, consistent with a criminal order able to absorb larger flows without lethal competition; during the second, the same exposure channel becomes a gradient. This is what the processual argument predicts, and it is difficult to reconcile with a purely mechanical account in which homicide follows the volume of cocaine passing through the ports. The port-proximity results add that the territorial gradient is visible within the cantonal scale but is subsumed by coastal membership, which suggests that the relevant exposure is a bundle of features rather than distance to a port in itself.

Three limits deserve emphasis. First, the coca interaction is identified from six annual observations per window and from a series that is correlated with time within each window; we mitigate this with wild-bootstrap, permutation, lagged, and trend-augmented specifications, but the evidence remains an association between two time series and a territorial exposure. Second, the Colombian series stops being strictly comparable in 2025 and, as a national total, does not measure flows that actually reach Ecuador. Third, although the onset precedes the second boom, the boom overlaps entirely with the amplification of the divide, so the design cannot separate the effect of supply from that of the competition unleashed by the rupture; the first-window null shows that supply alone was not sufficient, not that it is irrelevant to the second window.

The sequence allows us to sharpen the processual argument: conditions of exposure, mechanisms associated with the break, and those that may subsequently reproduce violence occupy different positions within the trajectory, and their explanatory relevance may change over time. Varying scales makes it possible to observe these moments without making the result depend on a single spatial or temporal aggregation. The causal scope remains limited: the design identifies the divergence and its sequence, not the mechanisms responsible for each moment. In addition, the Coast–interior treatment averages over heterogeneity among cantons (Molina Ramón, 2025), the log(1 + H) transformation limits the exact interpretation of the coefficients (Chen & Roth, 2024), and administrative records may contain misclassification errors. These constraints limit the causal inferences that can be drawn, but they do not prevent the identification of the trajectory of onset, amplification, and persistence.

Conclusions

This article reconstructs the onset, amplification, and persistence of the subnational divergence in homicidal violence in Ecuador between 2014 and 2025, and relates it to Colombian coca supply and to proximity to ports. Four complementary event studies, which combine different territorial scales and temporal frequencies on 39,819 administrative records, converge on a single trajectory. The divergence between the Coast and the interior opens in the last quarter of 2020—before the assassination of the leader of Los Choneros and before Lasso took office—widens through 2023, and remains open through December 2025; using 2020 as the reference year delays the dated onset to April 2021 without altering the trajectory. The crisis is therefore not a single episode but a process whose distinct moments require differentiated explanations.

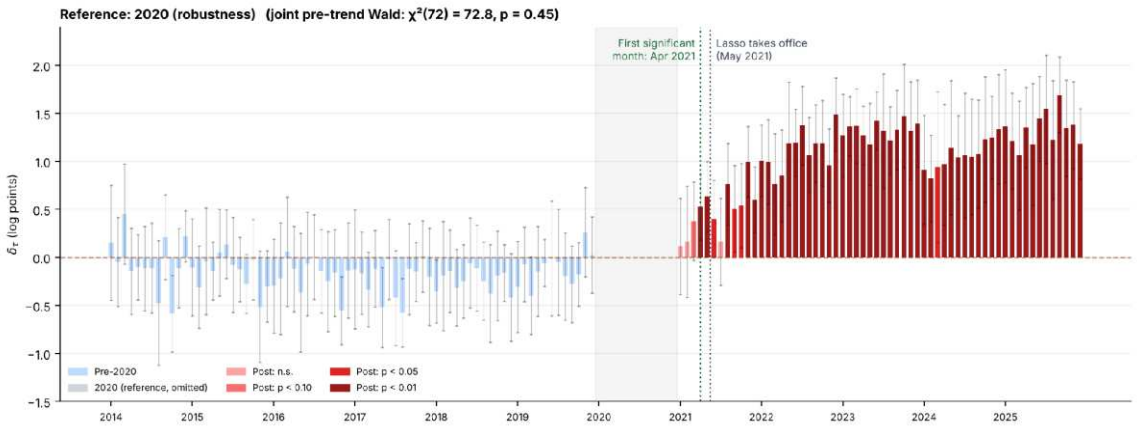
The main argument is that the relevance of mechanisms changes along that trajectory. The onset precedes both the best-known criminal event of the period and the second Colombian coca boom, which points to an erosion of the criminal order already under way in 2020. Colombia's two coca booms

did not have the same relationship with the Coast's homicides: the first was unrelated to it, while the second moves with it during the amplification of the divide, which suggests that the territorial transmission of supply depends on the state of the criminal order. Proximity to ports reproduces this pattern at the cantonal scale but is largely absorbed by coastal membership. The design does not causally identify each of these mechanisms, but it allows us to establish when they become plausible and which are incompatible with the observed sequence.

Open questions remain about which cantonal conditions explain the heterogeneity within the Coast, what role state capacities and criminal structures play during amplification, and which mechanisms subsequently prevent reconvergence. It will also be necessary to explore whether conventional regional categories (Coast, Highlands, etc.) adequately capture this heterogeneity, or whether it would be more appropriate to construct territorial classifications based on cantons' proximity and connectivity to ports, road corridors, borders, and other strategic nodes of the transnational circuits of drug trafficking. Answering these questions will require designs oriented toward identifying specific mechanisms. The starting point of this study is more modest: Ecuador's crisis cannot be attributed to a single cause, because what explains where violence concentrates does not necessarily explain when it begins, how it is amplified, or why it persists.

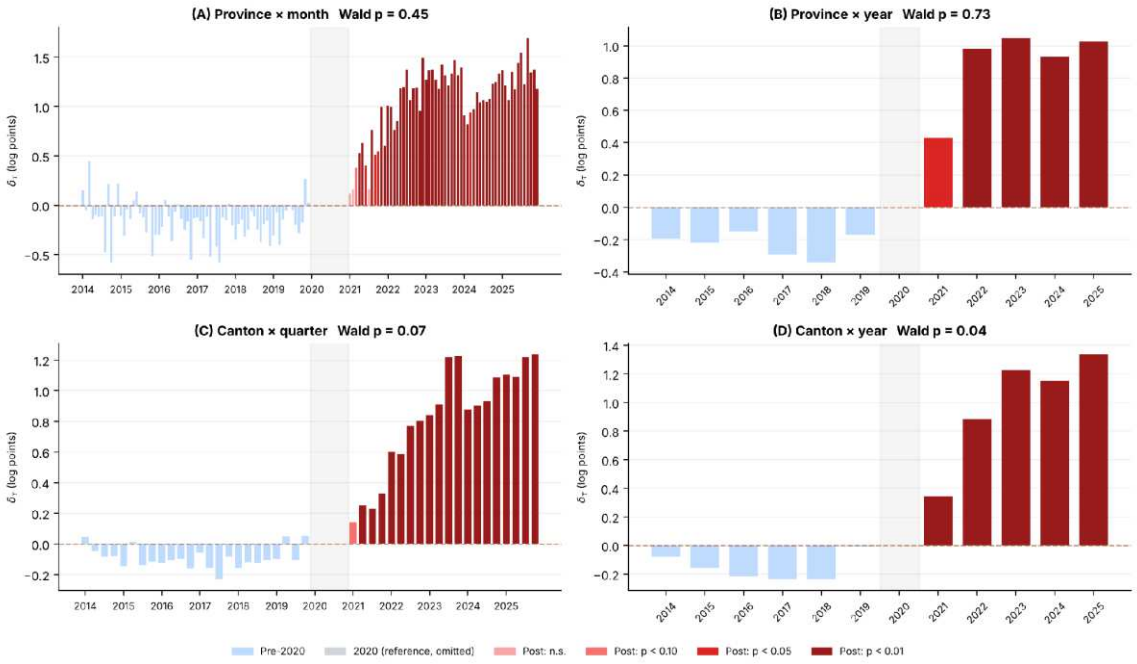
Appendix: robustness with 2020 as the reference year

Fig. A1 *Monthly event study at the province level (specification A), reference year 2020*



Note: δτ coefficients relative to 2020; error bars are 95% confidence intervals with HC3 standard errors. Joint pre-trend Wald test: $\chi^2(72) = 72.8$, $p = 0.45$. The first significant month is April 2021. Source: Authors' own elaboration.

Fig. A2 *The four event-study specifications (reference year 2020)*



Note: Same specifications as Fig. 3 with 2020 as the omitted reference year. Titles report the p-value of the joint Wald test of all pre-treatment coefficients. Source: Authors' own elaboration.

Replication note

The data, code, and outputs needed to reproduce every table, figure, and number cited in the text are provided as Online Resource 1 (a Google Colab notebook with the three raw data files and a README). The notebook includes the event studies with 2019 and 2020 as reference years, the joint pre-trend tests, the coca-shift models with wild cluster bootstrap and permutation inference, and the port-proximity analysis.

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