Too Far to Matter? The Urban Reach of Rural Territorial Conflict

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Can persistent territorial conflict affect nearby urban economies even when conflict events occur outside the city? I examine the Mapuche self-determination conflict in southern Chile, a recurrent and predominantly rural conflict rooted in historical land dispossession and territorial claims. I link geocoded attacks, coercive actions, and land invasions to a panel of 104 fixed urban areas and measure annual external exposure from events occurring within 25 kilometers but outside the city boundaries. Greater external conflict exposure is associated with weaker formal business activity in municipal tax records linked to the receiving areas. Moving from one to ten exposed events corresponds to about 2.2 percent fewer registered enterprises and 5.3 percent lower tax-reported sales. I complement the analysis with measures of nighttime lights, newly built area and fiscal appraisals. I find no comparable contemporaneous decline in nighttime lights or newly built area, while fiscal appraisal shows a negative association at medium lags, potentially reflecting the timing of administrative assessments. To explore whether these associations vary with historical land claims and Indigenous settlement, I examine heterogeneity by Títulos de Merced coverage and historical Mapuche population shares, but find no systematic evidence of stronger associations along either dimension. The findings highlight the importance of considering nearby urban markets, rather than only directly affected rural areas, when assessing the economic consequences of territorial conflict and designing policy responses.

Keywords: Indigenous conflict; political violence; local economic activity; spatial spillovers; Chile.
JEL codes: D74; O18; R11.

1 Introduction

Rural conflicts can affect more than the places where violence occurs. Towns and small cities provide markets, services, and economic opportunities for the surrounding countryside. When territorial conflict persists in these areas, firms in nearby towns may face disruptions to their activities or reconsider their investment and location decisions, even when the towns themselves are not directly affected by violence. These consequences may be particularly relevant in regions that already

face persistent economic disadvantages. This paper asks whether recurrent territorial conflict is associated with weaker economic activity in nearby urban areas, and whether this relationship varies with the historical geography of land claims and Indigenous settlement.

A growing literature documents how conflict and insecurity affect firms, investment, and local economic activity. Several studies focus on firms operating in conflict-affected locations (Camacho and Rodriguez, 2013; Del Prete et al., 2023; Neri-Lainé, 2026; Utar, 2025; Brancati et al., 2026), while others show that the economic consequences of violence can extend beyond the places where it occurs. For instance, Alfano and Cornelissen (2026) show how conflict along transport routes in Somalia affects prices in distant markets, while Abadie and Dermisi (2008) document higher office vacancy around prominent buildings in Chicago following the September 11 attacks, consistent with responses to perceived insecurity. I examine a related but distinct spatial relationship: whether conflict concentrated in rural territories is associated with weaker economic activity in nearby towns and cities, even when the conflict occurs outside their urban boundaries. I also investigate whether this relationship is reflected in physical urban development and how it varies across locations within the same city.

The Mapuche self-determination conflict in southern Chile provides an interesting setting for studying these questions. Unlike conventional civil wars or large-scale armed conflicts, it centers on territorial claims, land restitution, and political recognition. Its events include attacks, coercive actions, and land invasions that recur over time and occur predominantly outside urban areas. At the same time, the conflict unfolds in a country where formal property markets, tax administration, Indigenous land protections, and public restitution programmes remain operative. This combination provides an opportunity to examine the economic reach of persistent territorial conflict without the generalized institutional breakdown often associated with conventional armed conflict.

The regional context adds another dimension to this question. Much of the longstanding conflict is concentrated in and around La Araucanía, historically one of Chile's poorest regions. In 1992, its GDP per capita represented approximately 43% of the national average, increasing to 56% by 2017. Despite this partial convergence, La Araucanía ranked last nationally in GDP per capita in 1992, 2002, and 2017.[1] Its persistent economic disadvantage raises questions about the factors that may constrain development in the region, including the potential role of territorial conflict. Larroulet Vignau and Sepúlveda (2025) examine the economic consequences of violence at the regional level. I approach the question from a different spatial perspective, focusing on urban areas exposed to conflict in their surrounding territories.

The conflict is also closely connected to historical land dispossession and changes in property rights. Following the incorporation of Mapuche territory into Chile in the late nineteenth century, the State issued *Títulos de Merced* (TDM), establishing a legally recognized geography of Indigenous reservations. Subsequent land transfers, subdivision, and the reversal of agrarian reform substantially changed the distribution of land ownership (Jaimovich and Jordán, 2025a; Jaimovich

[1]The regional comparisons combine Banco Central GDP and census population data harmonized to the same regional boundaries within each year.

and Toledo-Concha, 2026). As a result, historical claims and contemporary property rights do not necessarily coincide. Since 1993, the Chilean State has also operated a land restitution programme through the Corporación Nacional de Desarrollo Indígena (CONADI), providing an institutional mechanism for addressing territorial disputes (Gobierno de Chile, 1993).

This historical and institutional context raises several questions about how firms might respond to nearby conflict. One possibility is that territorial disputes increase uncertainty about property rights and future land use, discouraging economic activity in places close to historically recognized claims. Another possibility concerns how firms perceive locations associated with Mapuche communities. If ethnic composition becomes a signal of perceived territorial risk, economic responses may also vary across places with different historical Mapuche populations, potentially reflecting territorial stigma or statistical discrimination.

Land restitution introduces an additional institutional dimension. Resolving historical claims could reduce uncertainty and improve economic expectations, but restitution may also be concentrated in territories where disputes are particularly persistent. I examine these questions through the historical geography of land claims, Mapuche population shares, and the relationship between conflict and subsequent land acquisitions by CONADI.

To investigate these questions, I combine geocoded conflict events from the Mapuche Conflict Event and Data Archive (MACEDA) (Cayul et al., 2022) with economic and spatial data for 104 fixed urban areas in Biobío, La Araucanía, Los Ríos, and Los Lagos. The main measure captures attacks, coercive actions, and land invasions occurring outside each receiving urban area but within 25 kilometers of its boundary. Conflict occurring inside the receiving area enters separately. I use within-area variation in nearby conflict exposure over time, accounting for time-invariant differences across locations, region-specific annual shocks, and trends associated with predetermined characteristics. The analysis combines administrative information on registered enterprises and sales with nighttime lights, fiscal appraisal, cadastral records, and satellite measures of urban development. Enterprises and sales are assigned to municipalities through tax or headquarters addresses, and therefore do not necessarily represent economic activity taking place within the fixed urban boundaries. I complement this analysis with a spatial comparison of grid cells within the same urban area and year.

The results show that greater exposure to nearby conflict is associated with weaker formal business activity. Moving from one to ten external events per year, corresponding to the interquartile range among area-years with positive exposure, is associated with approximately 2.2% fewer registered enterprises and 5.3% lower tax-reported sales. The relationship is more precisely estimated for enterprises, while the confidence interval for sales includes zero under the preferred area-clustered inference (Table 1). The analysis of leads and lags also shows negative enterprise associations over subsequent years, although the timing evidence is less clear for sales. Importantly, these patterns do not extend uniformly to other outcomes. Nighttime lights, fiscal appraisal, and newly built area do not exhibit a common contemporaneous decline. Fiscal appraisal becomes negatively associated with conflict at medium lags, although its administrative revaluation process complicates the

interpretation of these adjustments.

The spatial analysis provides a complementary perspective. Within the same urban area and year, grid cells with greater nearby conflict exposure show fewer selected legal entities at short exposure radii and lower shares of commercial cadastral use across several radii. However, nighttime lights, assessed property values, and new construction do not display a consistent negative pattern (Figure 4). These findings suggest that the relationship between nearby conflict and urban economic activity varies across outcomes and locations. The spatial comparisons also reveal local differences that may not be visible in aggregate municipal records.

I further examine whether these relationships vary with the historical geography of Indigenous land claims and population composition. At the municipal level, I find no systematic evidence that the negative associations with formal business activity are stronger in places with greater historical TDM coverage or larger 1992 urban Mapuche population shares (Figure E.1). I also examine the relationship between conflict and the State's institutional response. Greater external conflict exposure is associated with more CONADI land acquisitions two years later, based on combined records from Articles 20(a) and 20(b) (Appendix F). This pattern may reflect an institutional response to persistent territorial disputes, but it does not establish whether these interventions subsequently reduce conflict or economic uncertainty.

This paper contributes to the literature by examining an economic dimension of territorial conflict that may be overlooked when analysis focuses exclusively on the locations of violent events or on regional aggregates. The findings suggest that recurrent rural conflict is associated with weaker formal business activity in nearby urban markets, even when the conflict occurs outside their boundaries. At the same time, the absence of consistent negative associations in nighttime lights and measures of physical urban development suggests that these relationships do not extend uniformly across different dimensions of local economic activity. One possible interpretation is that firms adjust their activities and decisions before these changes become visible in the physical urban environment, although the available evidence does not establish this sequence. The evidence remains associational, but it points to an economic margin that may be missed when studying conflict only where violent events occur.

More broadly, the findings speak to the relationship between territorial conflict and regional development. In regions such as La Araucanía, where economic disadvantages have persisted, the potential consequences of rural conflict for nearby towns and cities are particularly relevant. Assessments of territorial disputes and the policies designed to address them may therefore need to consider these urban markets, rather than focusing exclusively on the rural territories directly affected. Whether resolving historical land claims can improve local economic conditions remains an important question for future research.

2 Institutional Setting and Conflict Geography

2.1 Territorial self-determination and historical claims

The Mapuche are an Indigenous group of south-central Chile whose historical territory encompasses much of the study corridor. Mapuche communities retained substantial autonomy through most of the colonial period and were incorporated into the Chilean State only after the military occupation of Araucanía in the late nineteenth century. Between 1884 and 1929, the State confined Mapuche communities to nearly 3,000 reservations covering roughly 550,000 hectares, a small share of their ancestral territory (Jaimovich and Jordán, 2025b; Jaimovich and Toledo-Concha, 2026). The State formalized these reservations through *Títulos de Merced* (TDM). The TDM therefore did not restore ancestral territory; they legally recognized the much smaller areas left to Mapuche communities after the military occupation. They created an important State-recognized geography of historical territorial claims that remains relevant to later land protection, restitution, and conflict. Importantly, TDM boundaries do not describe current Mapuche ownership. Through subsequent allotment, land sales, encroachment, weak enforcement, and later deregulated transfers, Mapuche communities lost ownership of substantial amounts of land within the historical reservations (Jaimovich and Jordán, 2025b,a). Contemporary claims can therefore concern land that lies within a historically recognized TDM but is no longer under Mapuche ownership. In the empirical analysis, TDM are used to locate this historical geography of recognized claims, not to identify the current owner of a parcel or the ethnic identity of a nearby firm.

Agrarian reform extended the contested geography beyond the reservations. The reform began in 1962 and accelerated between 1970 and 1973, when Mapuche communities recovered or gained access to land through the expropriation of large estates. After the 1973 military coup, the dictatorship returned or reassigned much of that land, promoted forestry plantations, and divided remaining collective reservations into individual parcels. Contemporary invasions and attacks are more common on plots touched by that reform and its reversal, linking current conflict to grievances both within and beyond TDM boundaries (Jaimovich and Jordán, 2025a; Jaimovich and Toledo-Concha, 2026).

2.2 Formal land institutions and restitution

Chile's 1993 Indigenous Law defines which lands qualify for Indigenous status, including land recognized through *Títulos de Merced*, certain historical occupation, and later State transfers. Articles 13–15 restrict transfers, encumbrances, and use arrangements, with different provisions for community and individually held land. Registration in the Public Registry of Indigenous Lands attests to that legal status (Gobierno de Chile, 1993). In Temuco property records, registered parcels had fewer post-inheritance sales and smaller losses of Mapuche ownership, while registry coverage declined over time (Jordán et al., 2025).

The law also created the Fund for Indigenous Land and Water, administered by CONADI. Article 20(a) subsidizes land acquisition where existing holdings are insufficient. Article 20(b) fi-

nances the resolution of land disputes, including purchases related to historically recognized claims (Gobierno de Chile, 1993). The Presidential Commission for Peace and Understanding reports that CONADI acquired 230,563 hectares under Article 20(b) in the four study regions through 2024 (Comisión Presidencial para la Paz y el Entendimiento, 2025, pp. 60–61, 72–73). This represents approximately 58 percent of the 396,825 hectares estimated for the Commission's Group 1 of land restitution demand, although this comparison does not measure the share of historical claims already resolved.[2]

Land restitution continued during the study period. Appendix F examines how recorded acquisitions under both articles vary with conflict exposure.

2.3 Conflict geography and recurrence

The Mapuche Conflict Event and Data Archive (MACEDA) records geocoded events from local-media reports in the Mapuche self-determination conflict (Cayul et al., 2022). The main event family contains attacks, coercive actions, and land invasions. Events recur over time rather than defining a single absorbing transition into conflict.

Most main events occur outside the study's urban areas. Of the 2,775 main events in the four-region corridor, 2,288 (82.5%) lie outside all 104 fixed urban areas. La Araucanía contains 66.6 percent of the corridor's main events despite accounting for 25.7 percent of the corridor's 2017 population; conflict also occurs in Biobío, Los Ríos, and Los Lagos. Figure 1 maps the events and the locations of the receiving urban areas.

Annual event counts rise and fall throughout the period (Figure 2). Nearby urban areas may therefore face repeated changes in external exposure even when events occur beyond their own urban footprint.

3 Data

3.1 Spatial units and outcomes

The analysis covers 104 fixed urban areas in Biobío, La Araucanía, Los Ríos, and Los Lagos, four regions encompassing much of the historical Mapuche territory in Chile. Figure A.1 in the Appendix illustrates the historical territorial context and the contemporary study corridor. Each urban area is constructed from the 2017 INE urban census blocks within a municipality, with boundaries held fixed throughout the study period. Some urban areas contain spatially disconnected components. These are geographic urban areas rather than functional labor-market areas. Their median area is 4.59 km^2, although their size varies considerably, ranging from 0.61 to 63.91 km^2.

Annual records from Chile's national tax authority (Servicio de Impuestos Internos, SII) measure registered enterprises and tax-reported sales. Tax-reported sales are sales declared by firms that

[2]Group 1 includes applicants with a favorable applicability decision or an application already submitted and allows up to 5 percent growth in beneficiary families. The Commission's broader estimate, including potential future demand, is 486,008 hectares.

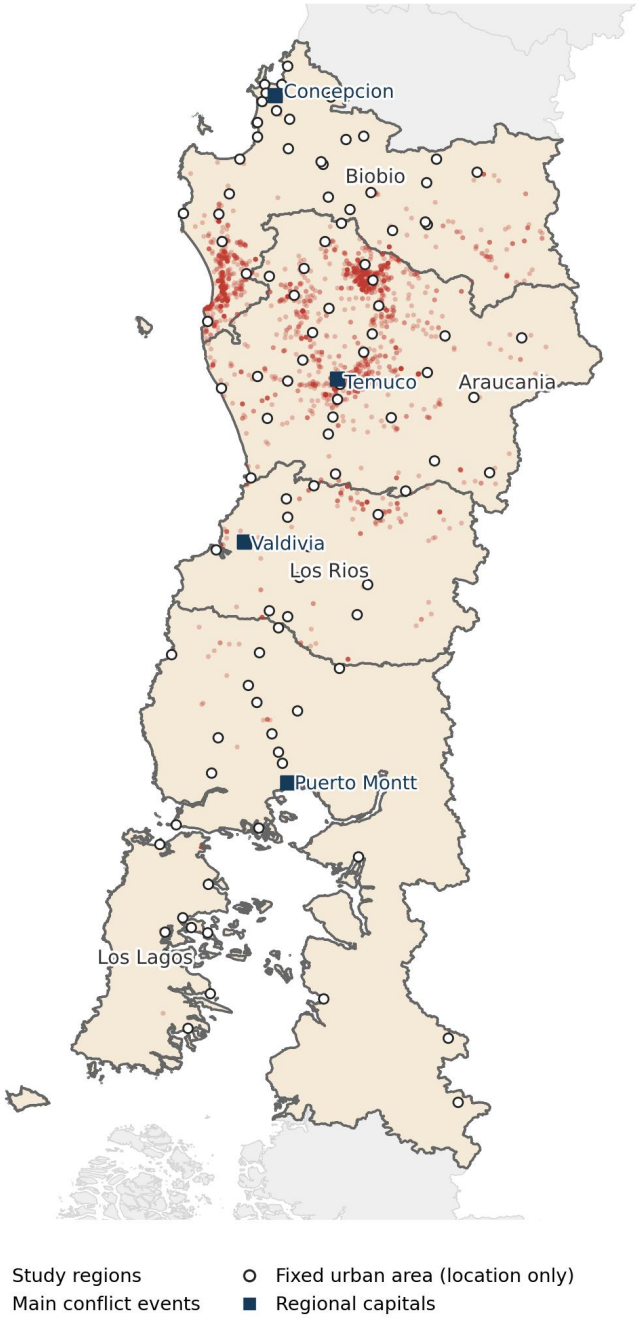


Figure 1: Main conflict events and fixed urban areas in southern Chile. The map shows attacks, coercive actions, and land invasions in Biobío, Araucanía, Los Ríos, and Los Lagos during 1990–2021. Point markers locate the 104 receiving fixed urban areas, with squares for the four regional capitals; marker size does not represent footprint area.

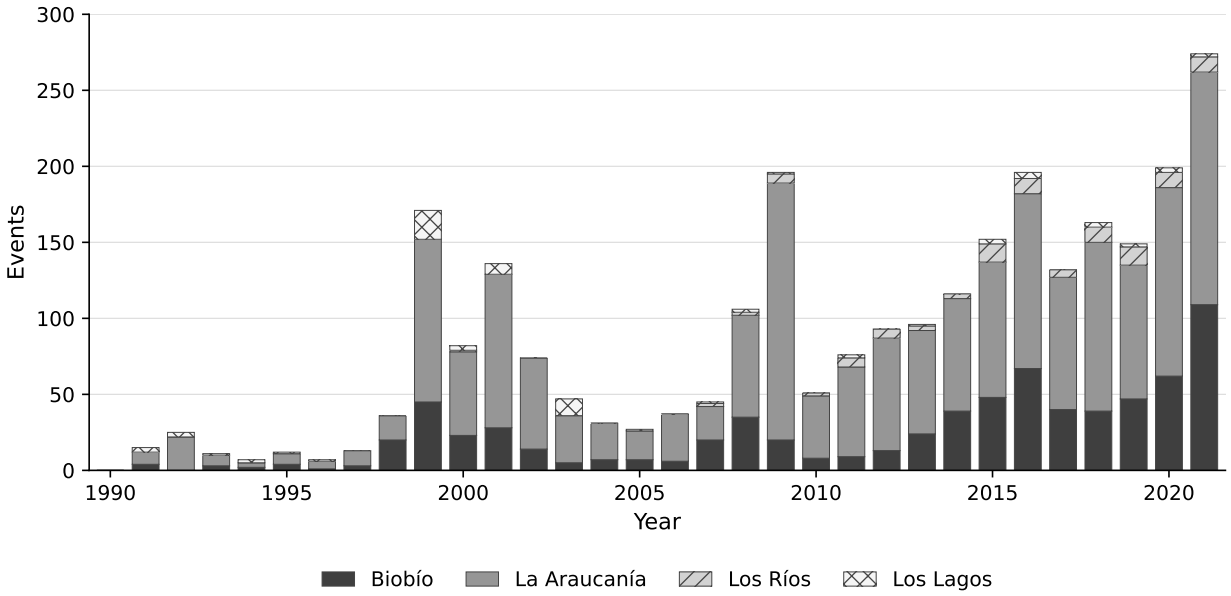


Figure 2: Annual main Mapuche conflict events by region, 1990–2021. Stacked bars count attacks, coercive actions, and land invasions in Biobío, Araucanía, Los Ríos, and Los Lagos.

SII administratively assigns to the municipality of their tax or headquarters address. Both series are available from 2005 to 2021. These outcomes measure formal business activity registered in the municipalities associated with the receiving urban areas, rather than activity directly observed within their fixed urban footprints.[3]

Nighttime luminosity provides a spatial measure of activity within the fixed urban footprints from 1992 to 2021, using harmonized DMSP–VIIRS observations. Lights do not measure formal firms or sales directly, but provide a complementary indicator of broader urban economic activity.

The property cadastre provides total fiscal appraisal and mean appraisal per property within each fixed urban area from 2010 to 2021. Fiscal appraisal serves as a tax-based proxy for property values, combining assessed land and construction values, but is administratively determined rather than based on observed market transactions. Changes in assessed values may therefore reflect administrative adjustments as well as changes in underlying property values.[4]

WSF Evolution records changes in built surface from 1990 to 2015. It measures a physical flow and complements the economic outcomes by tracking built-area expansion. Results for newly built area appear in Table 1 and Figures 3 and 4.

[3] Appendix D examines this spatial interpretation.

[4] SII updates fiscal appraisals through mass revaluations, individual inspections, taxpayer requests, and semiannual CPI indexation. The 2010–2021 panel includes nationally scheduled non-agricultural revaluations in 2013, 2014, and 2018. The 2006 round preceded the panel, while the 2013 and 2014 rounds covered different property classes. Region-by-year fixed effects absorb revaluation-year shocks shared within regions, but not heterogeneous parcel-level or cadastral changes (see Servicio de Impuestos Internos, 2026a,b,c).

3.2 Conflict data and external exposure

Conflict exposure comes from annual geocoded events in the Mapuche Conflict Event and Data Archive (MACEDA) (Cayul et al., 2022). The main event family includes attacks, coercive actions, and land invasions; protests are excluded. Let A_{mt} denote the number of eligible events in year t within 25 kilometers of urban area m and outside its footprint. External exposure is

$$D_{mt} = \log(1 + A_{mt}) \,. \tag{1}$$

Conflict counts are zero-heavy and right-skewed. The $\log(1 + x)$ transformation retains zero-exposure area-years and dampens the influence of high counts; coefficients therefore describe changes in transformed exposure and are not elasticities with respect to the raw event count. Table B.3 applies the inverse hyperbolic sine to contemporary external conflict, within-area conflict, and cumulative pre-2005 conflict, holding the sample and other controls fixed.

Distance is the exact minimum straight-line distance from each event point to the fixed urban-area footprint, calculated in metric projection EPSG:32718. Events outside the receiving area qualify when this distance is at most 25 km. Events inside the receiving area enter separately as $I_{mt} = \log(1 + B_{mt})$, where B_{mt} is the annual count of main events inside that area.

The conflict literature does not provide a standardized distance cutoff for nearby exposure. Closely related studies use local windows in the 20–30 km range (Brancati et al., 2026; Fadare et al., 2024), while other work defines exposure along transport corridors (Alfano and Cornelissen, 2026; Liu et al., 2026). I use 25 km as a baseline measure of geographic proximity, rather than observed economic connectivity. This choice is not based on outcome estimates, and I assess alternative radii from 10 to 60 km. An outcome-free comparison finds that annual area-level exposure counts under the 20–30 km definitions are strongly correlated with counts under a 30-minute contemporary road-network definition (Appendix A.4).

Because exposure is defined separately for each receiving area, an event may contribute to multiple areas when their windows overlap. Of 2,775 eligible events in 1990–2021, the preferred mapping contains 8,480 event–area pairs. Each event enters about 3.1 areas on average (median 3; maximum 9), while 109 events (3.9 percent) enter no area's external exposure. Events outside all study urban areas make up 81.7 percent of receiver–event pairs; events inside another study urban area make up the remainder. Each area-year remains one panel observation. Both location categories contribute to the preferred exposure measure.

4 Empirical Strategy

4.1 Baseline specification and interpretation

For outcome Y_{mt}, the preferred area-level specification is

$$Y_{mt} = \beta D_{mt} + \rho I_{mt} + \mu_m + \lambda_{r(m),t} + \tau_t (X_m^0)'\theta + \varepsilon_{mt}, \tag{2}$$

where D_{mt} measures external conflict exposure and I_{mt} captures contemporaneous conflict inside the receiving urban area. The terms μ_m and $\lambda_{r(m),t}$ are area and region-by-year fixed effects, respectively. The vector X_m^0 contains predetermined characteristics interacted with linear calendar time τ_t, allowing areas with different initial conditions to follow different trends.

Specifically, X_m^0 includes standardized 2002 municipal log population, 2002 municipal rural share, and cumulative external conflict exposure from 1990 through 2004. Population and rural share are measured at the municipal level using the 2002 Census, rather than within the later fixed urban footprint. Population enters as $\log(1 + P_m^{2002})$, while historical conflict uses the same external exact-25-km definition as contemporary exposure, with cumulative events transformed as $\log(1 + H_m)$.

Area fixed effects account for persistent differences across locations, while region-by-year effects absorb annual shocks shared by areas within Biobío, La Araucanía, Los Ríos, and Los Lagos. The predetermined trends allow economic trajectories to differ with initial population, rural composition, and historical conflict exposure. The model also conditions on contemporaneous conflict inside the receiving area, so the coefficient on external exposure describes its association with economic activity conditional on direct local conflict.

These predetermined characteristics precede the 2005–2021 formal business and 2010–2021 fiscal appraisal panels. However, they do not predate the full nighttime-lights and built-area series, which begin in 1992 and 1990, respectively. For these longer-window outcomes, I retain area and region-by-year fixed effects and within-area conflict, but omit the predetermined trends.

Standard errors are clustered by urban area. Because nearby areas can share conflict events and economic shocks, Table B.4 also reports spatial-HAC inference allowing for spatial and temporal residual dependence.

The coefficient β is estimated from changes in external conflict exposure within urban areas over time, after accounting for the included controls. Nevertheless, unobserved local shocks may simultaneously affect conflict and economic activity, and conditioning on within-area conflict does not eliminate this concern. I therefore interpret the estimates as conditional associations rather than causal effects.

4.2 Timing patterns

To examine how these relationships evolve over time, I extend the baseline specification by including future, contemporaneous, and lagged values of annual external conflict exposure:

$$Y_{mt} = \sum_{h=1}^{3} \pi_h D_{m,t+h} + \sum_{\ell=0}^{L_Y} \beta_\ell D_{m,t-\ell} + \rho I_{mt} + \mu_m + \lambda_{r(m),t} + \tau_t (X_m^0)'\theta + \varepsilon_{mt}. \tag{3}$$

The specification includes three future-exposure terms and lags through eight years for enter-

prises, sales, fiscal appraisal, and newly built area. The nighttime-lights profile extends through ten years, taking advantage of its longer temporal coverage.

For presentation, coefficients on future exposure are plotted at negative horizons. These terms help assess whether economic outcomes were already changing before the corresponding exposure was realized. Contemporaneous and lagged coefficients describe how the association evolves during and after exposure. Because conflict is continuous and recurrent, these estimates should not be interpreted as a conventional event study or as causal dynamic treatment effects.

Appendix C examines historical frequency spikes and documented destructive events. For each definition, the Callaway and Sant'Anna (2021) approach treats exposure as absorbing after the first episode. The repeated-episode comparison instead aligns outcomes around each supported escalation year and uses comparison municipalities without an escalation in the same window.

5 Results

5.1 Baseline estimates

Nearby external conflict is associated with lower formal business activity in municipal tax records linked to the receiving urban areas. In the preferred specification, moving from one to ten external events per year, corresponding to the interquartile range among area-years with positive exposure, is associated with approximately 2.2% fewer registered enterprises and 5.3% lower tax-reported sales. Table 1 reports the baseline estimates.

The enterprise and sales associations remain negative as the specification adds predetermined trends, region-by-year effects, and within-area conflict. The trends allow outcome paths to differ with initial population, rural composition, and prior conflict. The contemporaneous estimates for nighttime lights, total fiscal appraisal, and newly built area show no common significant contraction. The results therefore suggest differences between formal business activity and the measures of physical urban development, although they do not establish why these outcomes respond differently.

The enterprise and sales estimates remain negative across exposure radii from 10 to 60 km (Table B.5).

The negative enterprise association is not confined to firms classified in forestry or wood and paper manufacturing. Excluding those activities changes the coefficient from -0.0132 to -0.0127, with wider uncertainty (Table B.6).

5.2 Timing patterns

The continuous timing profile shows negative enterprise associations across several years following exposure, with the strongest pattern around the medium-term lags (Figure 3). Future exposure does not show a comparable systematic enterprise pattern.

Sales estimates are generally negative, but their uncertainty leaves no clear dynamic profile. Fiscal appraisal does not fall contemporaneously; its negative pattern emerges at medium lags. Because appraisal records an administratively updated property stock, that delay may reflect when

Table 1: External conflict exposure and urban-area outcomes

Specification	(1) Registered enterprises	(2) Tax-reported sales	(3) Nighttime lights	(4) Fiscal appraisal total	(5) WSF new built area
Area + year FE	-0.020**	-0.050**	-0.005	0.001	-0.041
	(0.009)	(0.022)	(0.011)	(0.021)	(0.034)
+ predetermined trends	-0.012**	-0.034**	—	0.002	—
	(0.006)	(0.017)		(0.016)	
+ region-by-year FE	-0.013**	-0.032*	-0.004	0.019	0.018
	(0.007)	(0.019)	(0.012)	(0.013)	(0.031)
+ within-area conflict (preferred)	-0.013**	-0.032*	-0.004	0.019	0.021
	(0.006)	(0.019)	(0.012)	(0.013)	(0.030)
Observations	1,768	1,768	3,060	1,248	2,704
Areas	104	104	104	104	104
Sample period	2005–2021	2005–2021	1992–2021	2010–2021	1990–2015

Notes: External exposure counts attacks, coercive actions, and land invasions within 25 km of the fixed urban footprint, excluding events inside the receiving area. All specifications include area fixed effects. The four rows add year effects, the three predetermined trends, region-by-year effects, and contemporaneous within-area conflict. The trends interact calendar time with 2002 municipal population, 2002 rural share, and cumulative external conflict in 1990–2004. Predetermined trends are not estimated for nighttime lights and WSF because their panels begin before the baseline characteristics. WSF is measured in asinh-transformed hectares. Standard errors are clustered by fixed urban area. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

values are reassessed. The three future-exposure coefficients for newly built area are jointly significant ($p = 0.031$).

Appendix C compares outcomes around first and repeated episodes of two kinds of escalation, historical frequency spikes and documented destructive events.

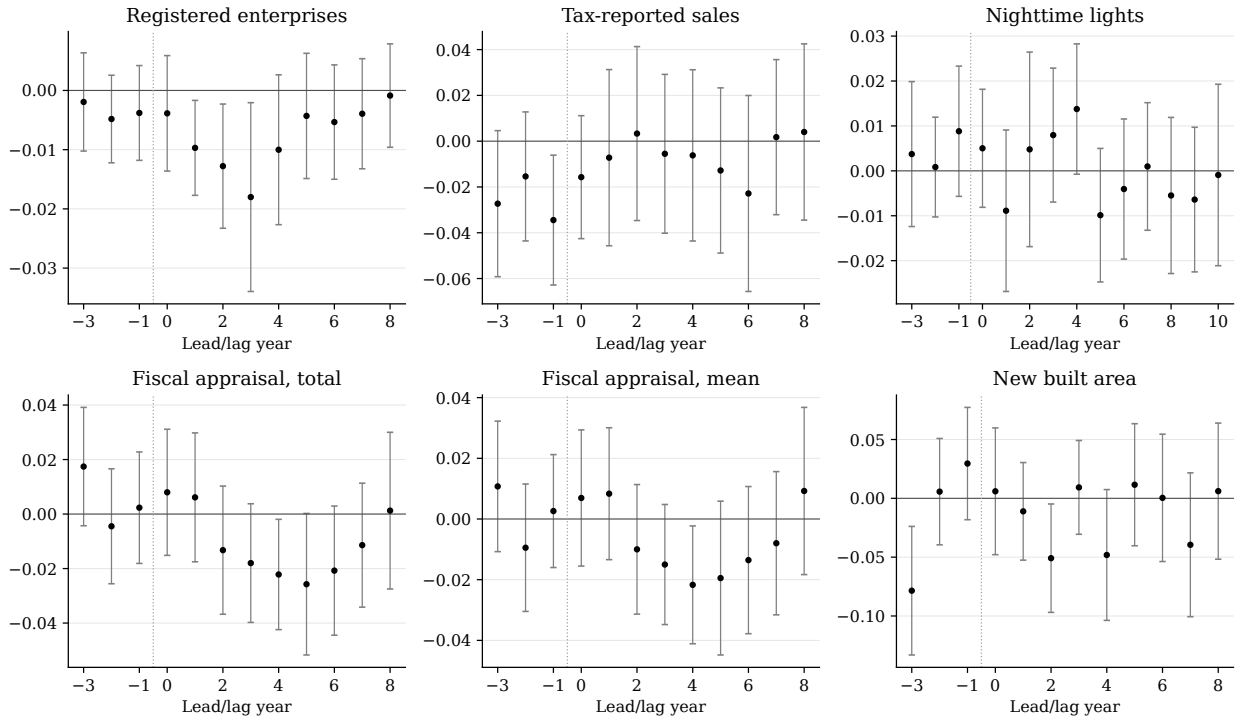


Figure 3: Continuous lead–lag profiles for urban-area outcomes. Negative horizons denote future exposure, zero denotes current exposure, and positive horizons denote lagged exposure. Bars are 95 percent confidence intervals based on area-clustered standard errors. All models use 25-km external exposure, contemporaneous within-area conflict, and area and region-by-year effects. Enterprises, sales and appraisal include the three predetermined trends. Lights and newly built area omit them.

5.3 Spatial interpretation and supporting evidence

SII outcomes are assigned through municipal tax and headquarters addresses, so they do not necessarily locate all reported activity inside urban areas. However, the negative association is also present among the subset of legal entities whose historical headquarters can be matched to urban cadastral parcels, while no significant association arises for entities with rural headquarters (Appendix D.1).

I compare fixed 1-km grid cells within the same urban area and year, with grid-cell and urban-area-by-year fixed effects. This comparison uses attacks, coercive actions, and land invasions outside all study urban areas. The aggregate baseline can also count events in another study urban area. For a radius R, I weight each event by $\max\{0, 1 - d/R\}$, where d is distance from the cell centroid, and use $\log(1 + S_R)$ for the weighted sum S_R. Each specification controls for contemporaneous conflict within the cell, $\log(1 + L)$.

The estimates for selected legal-entity headquarters are negative at 5 and 10 km and positive at larger radii (Figure 4). Commerce-use share is negatively associated with exposure at several radii. Nighttime lights, fiscal appraisal, and newly built area show no consistent negative pattern.

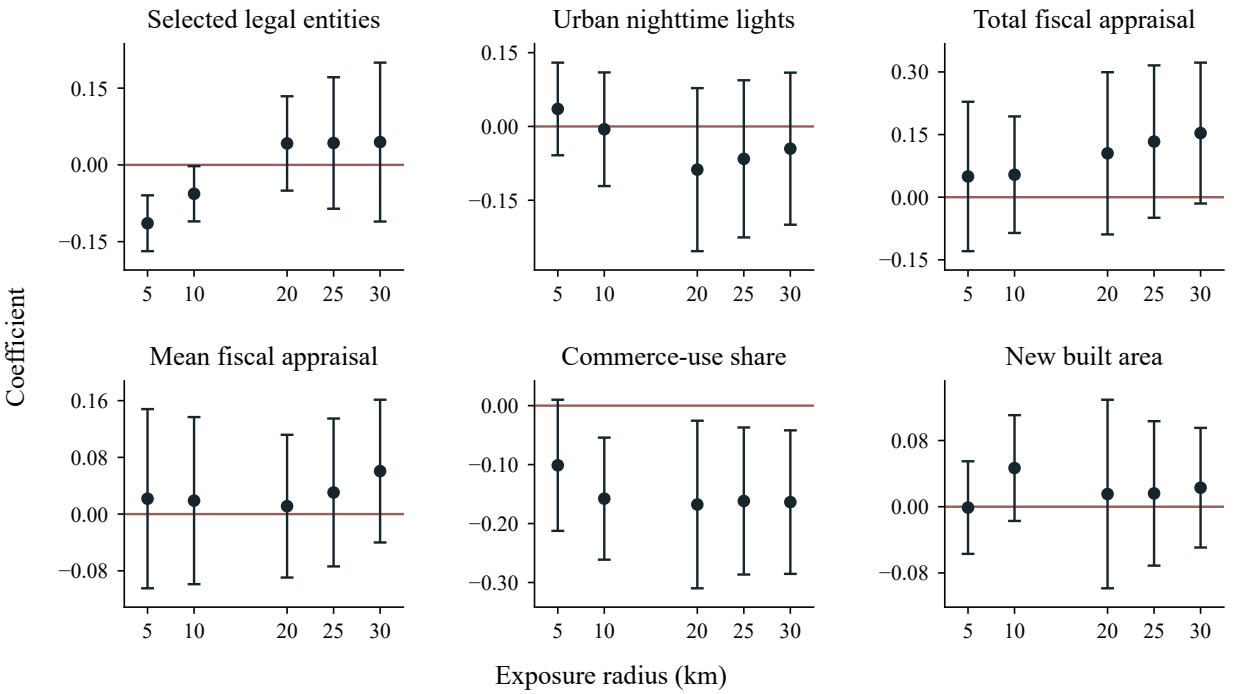


Figure 4: Within-area spatial incidence across exposure radii. The sample covers 1,623 fixed 1-km cells in 104 urban areas. Weights are $\max\{0, 1 - d/R\}$. All models include cell and urban-area-by-year fixed effects and direct conflict in the cell. Whiskers are 95 percent confidence intervals clustered by urban area. Sample years are 2005–2021 for legal entities and lights, 2010–2021 for both appraisal outcomes, 2012–2021 for commerce-use share, and 2005–2015 for newly built area.

I also examine whether the municipal business associations are stronger in sectors more closely connected to the surrounding rural economy. Using historical input-output matrices, I measure each sector's linkages with agricultural and forestry production through its inputs and customers. The estimates show no systematic differences by the strength of these linkages (Appendix D.2).

6 Additional Evidence

6.1 Robustness

Several checks examine whether the formal-activity associations depend on particular areas, exposure definitions, or estimation choices. Omitting each province in turn leaves the enterprise and sales estimates negative, although their precision varies (Figure B.1). Applying the inverse hyperbolic sine transformation consistently to conflict measures also produces similar magnitudes for the same change in raw exposure (Table B.3). Alternative wild-cluster and spatial-HAC inference preserves the negative estimates, with some differences in precision, particularly for sales (Table B.4).

I also examine whether the results depend on where conflict occurs. Separating events outside all study urban areas from those occurring inside another city, the enterprise association remains negative for the former. The evidence for sales is less clear, and the coefficients for the two locations cannot be statistically distinguished (Table B.1). Controlling for protests both outside and inside

the receiving area leaves the main enterprise and sales estimates largely unchanged (Table B.2). In Panel B, I distinguish attacks and land invasions from coercive actions, which MACEDA codes mostly to State actors. The enterprise coefficient is more negative for attacks and land invasions, and the sales coefficient is more negative for coercive actions. Neither difference is statistically significant.

Finally, excluding forestry and related manufacturing activities leaves the enterprise association negative and close to the baseline estimate (Table B.6).

6.2 Business dynamics and transport-related conflict

I examine whether the association with formal business activity differs according to where conflict occurs. Events recorded at roads, bridges, and highways show a more negative association with registered enterprises than events at other locations (Table B.8). A similar pattern appears in the SII series published as first registrations, including when the comparison is restricted to attacks.

However, the registration series does not provide a reliable measure of new firm creation. Its estimates vary across observation periods, and SII publishes identical records under different administrative labels. Recorded terminations show no clear association before the administrative changes of 2016–2017. These data therefore do not allow us to determine whether the lower enterprise stock reflects fewer entries or more exits (Table B.7).

7 Discussion and Conclusion

This paper examines whether persistent territorial conflict is associated with weaker economic activity in nearby urban areas, even when conflict occurs outside their boundaries. The Mapuche self-determination conflict in southern Chile provides an interesting setting, where recurrent territorial disputes coexist with formal markets and active State institutions. Across 104 urban areas, greater external conflict exposure is associated with fewer registered enterprises and lower tax-reported sales in the surrounding municipalities. Moving from one to ten events per year corresponds to approximately 2.2% fewer enterprises and 5.3% lower sales, although the latter estimate is less precise.

An important finding is that these associations do not extend uniformly across different dimensions of economic activity. Nighttime lights and newly built area show no comparable contemporaneous decline, while fiscal appraisal becomes negatively associated with conflict at medium lags. One possible interpretation is that firms adjust their operations, investment decisions, or expectations before these changes become visible in the physical urban environment. The spatial analysis also reveals differences within cities, particularly in the location of selected legal entities and commercial land use. These patterns suggest that understanding the economic consequences of territorial conflict requires looking beyond changes in the physical urban landscape.

Several mechanisms could explain how conflict reaches nearby urban economies. Towns and cities provide services, markets, and economic opportunities for their surrounding rural territories,

and persistent conflict may disrupt these relationships. The stronger negative association between registered enterprises and conflict at transport locations is consistent with this possibility, although the sectoral analysis does not reveal systematic differences according to agricultural or forestry linkages. Uncertainty about territorial claims and perceptions of insecurity may also influence firms' decisions. However, the results do not show systematically stronger associations in municipalities with greater historical Títulos de Merced coverage or larger Mapuche population shares (Figure E.1).

The institutional context adds another dimension to these findings. Unlike conflicts accompanied by widespread institutional breakdown, territorial disputes in southern Chile persist alongside established property rights, tax administration, Indigenous land protections, and public restitution policies. Greater external conflict exposure is followed by more recorded CONADI land acquisitions two years later (Figure F.1), suggesting that public intervention may respond to persistent disputes. Whether these interventions reduce uncertainty, prevent further conflict, or improve local economic conditions remains an open question.

More broadly, the findings highlight a potentially overlooked dimension of the relationship between territorial conflict and regional development. In regions such as La Araucanía, where economic disadvantages have persisted, recurrent conflict may represent an additional constraint on economic activity beyond the territories directly affected. Although the analysis does not establish that conflict causes regional underdevelopment, it shows why nearby urban markets deserve attention when studying its economic consequences. Evaluating territorial disputes and the policies intended to address them therefore requires considering not only the contested rural areas, but also the towns and cities connected to them.

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A Data, measurement, and sample construction

A.1 Fixed urban-area geography and study sample

I group 2017 INE urban census blocks (*manzanas* coded `AREA=1`) by municipality to construct 104 urban areas. Boundaries remain fixed throughout the study period and can contain disconnected components. The four-region corridor covers areas where historical Mapuche occupation, recognized claims, restitution, and recent conflict overlap. This geographic selection limits national representativeness.

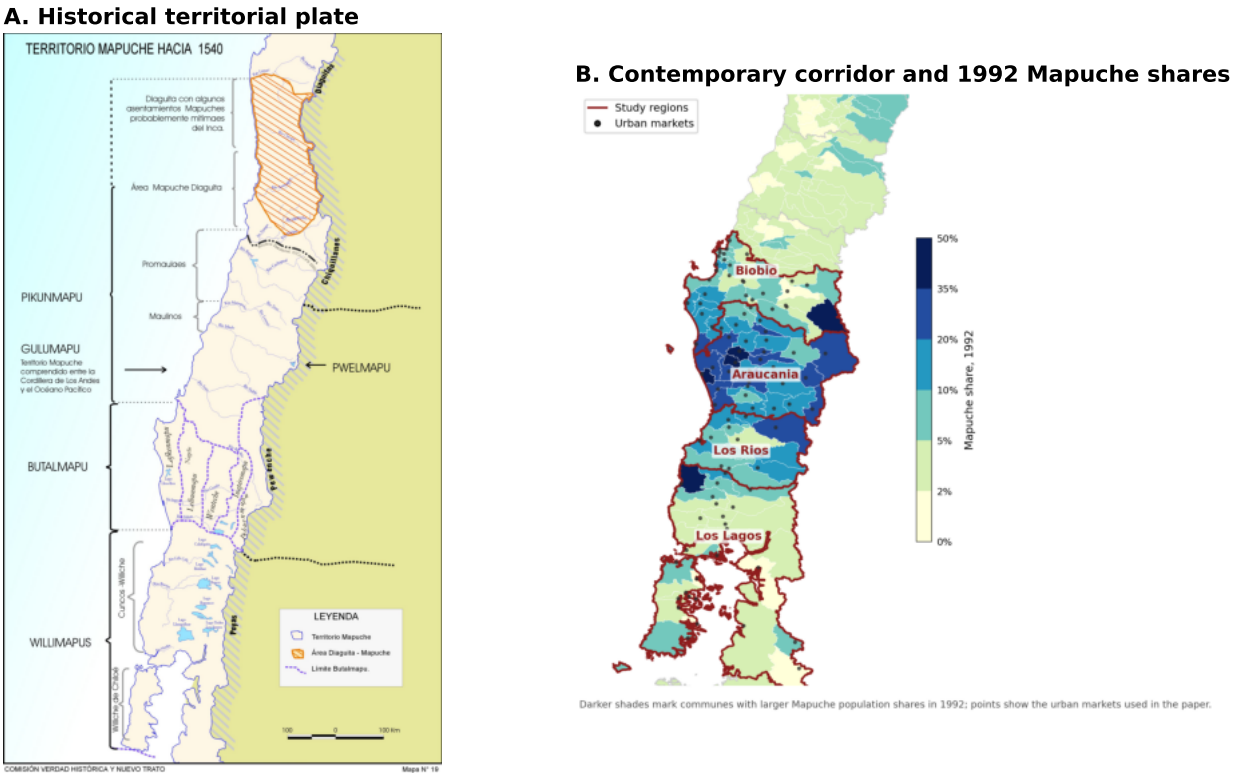


Figure A.1: Historical Mapuche territory and the contemporary study corridor. Panel A reproduces a map of Mapuche territorial organization around 1540 from the Comisión Verdad Histórica y Nuevo Trato. Panel B shows the study corridor and 1992 Mapuche population shares.

A.2 Conflict data and event composition

Table A.1 summarizes MACEDA events in the four-region study corridor over 1990–2021.

A.3 External exposure construction

Figure A.2 illustrates the external exposure defined in Equation (1). An event inside one urban area can contribute to external exposure for another area.

Table A.1: MACEDA event composition and regional concentration

Panel A. Event composition				
Event family	Events	Rural events	Rural share	
All MACEDA events (study corridor)	3,730	2,587	69.4%	
Main event family	2,775	2,278	82.1%	
Protests	955	309	32.4%	
Panel B. Main events by urban location				
Classification	Events	Share		
Outside all fixed urban areas	2,288	82.5%		
Inside a fixed urban area	487	17.5%		
Panel C. Regional concentration of main events				
Region	Main events	Event share	2017 population	Population share
Biobío	752	27.1%	1,556,805	41.8%
La Araucanía	1,847	66.6%	957,224	25.7%
Los Lagos	76	2.7%	828,708	22.2%
Los Ríos	100	3.6%	384,837	10.3%

Notes: Exposure uses the main event family, which includes attacks, coercive actions, and land invasions. Panel A uses MACEDA's rural classification. Panel B uses the fixed 2017 urban boundaries. Panel C reports shares of the corridor's main events and 2017 population.

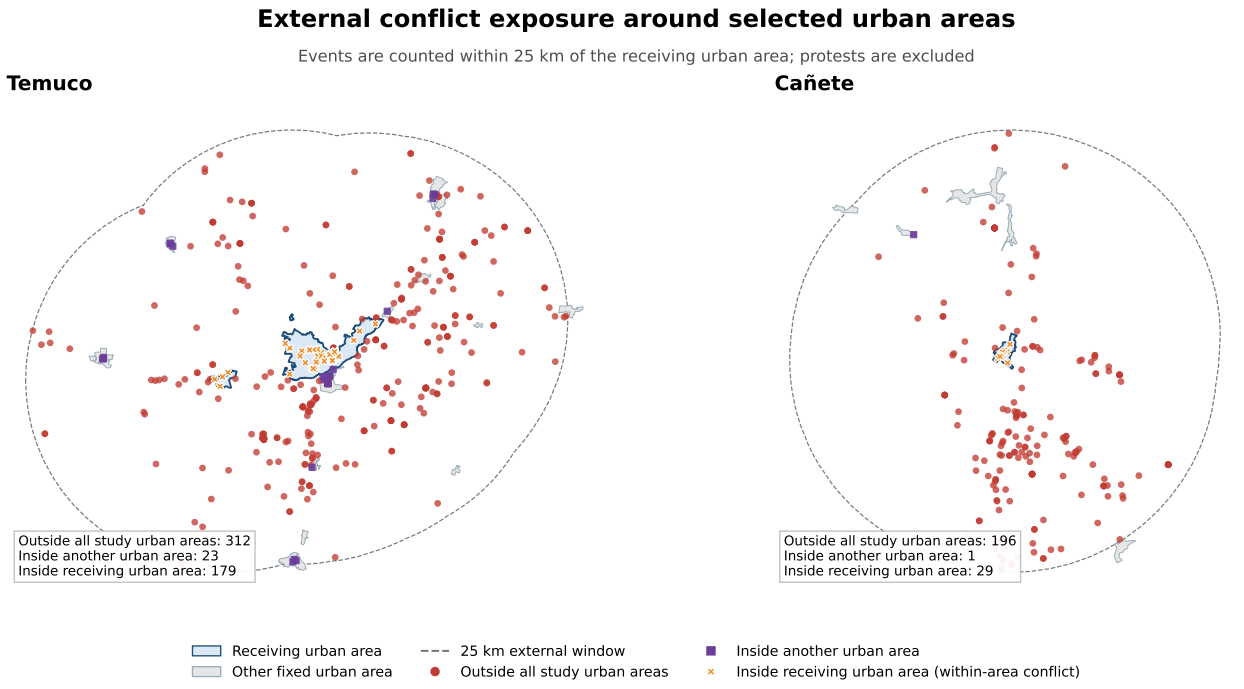


Figure A.2: External conflict exposure around Temuco and Cañete. The dashed boundary marks the 25-km window. Events within this window count as external if they occur outside the receiving area. Events inside the receiving area enter the separate within-area measure.

A.4 Geographic distance and road accessibility

I compare straight-line distance to each urban footprint with a 30-minute window based on fastest routes on the September 2026 OpenStreetMap network. Routes can start from any component of a footprint. Posted speeds cover about 15 percent of road length. I use road-class speeds elsewhere and exclude ferries. The road comparison omits 22 events outside the routing area or more than 2 km from a road segment.

Euclidean distance and 30-minute road exposure

Definition	Event–area pairs	Jaccard	Count corr.	Mismatch (%)
Euclidean 20 km	6,614	0.58	0.91	38 / 11
Euclidean 25 km	8,480	0.63	0.93	27 / 18
Euclidean 30 km	11,455	0.64	0.92	14 / 29
Road 30 min	9,513	—	—	—

Notes: Jaccard overlap is the share of pairs in either definition that both include. Count correlations use area-year counts for 1990–2021. The two percentages report the share of road pairs missed by the distance rule, then the share of distance pairs missed by the road rule.

Annual counts correlate most closely at 25 km, while pair overlap is highest at 30 km. These comparisons describe agreement between exposure definitions. The contemporary network and imputed speeds cannot reconstruct travel times during the study period.

A.5 Outcome construction and interpretation

Table A.2: Outcome construction and interpretation

Outcome	Source and spatial unit	Years	Transformation	Interpretation
Registered enterprises	SII municipality tax-address measure mapped to area	2005–2021	log(enterprises)	Formal enterprise stock associated with the area
Tax-reported sales	SII municipality tax-address measure mapped to area	2005–2021	log(sales)	Scale of formal activity associated with the area
Nighttime lights	Harmonized DMSP–VIIRS, fixed urban footprint	1992–2021	log(total DN)	Urban luminosity
Fiscal appraisal total	SII cadastre, fixed urban footprint	2010–2021	log(appraised value)	Assessed urban property stock
Fiscal appraisal mean	SII cadastre, fixed urban footprint	2010–2021	log(mean appraisal)	Mean assessed property value
WSF new built area	WSF Evolution, fixed urban footprint	1990–2015	asinh(new hectares)	New built-area flow

Notes: SII enterprises and sales use municipal tax addresses. The other outcomes are measured within fixed urban footprints.

B Robustness and margins of adjustment

B.1 Where external conflict occurs

Events outside all study urban areas account for 81.7 percent of external event–area pairs. The rest occur inside another study city within 25 km of the receiving area. Entering both exposures jointly leaves a negative enterprise estimate for events outside all study cities. The sales estimate is more precise for events in another city. The equality tests distinguish neither pair of coefficients (Table B.1).

Table B.1: Where external conflict occurs

Panel A. Exposure composition, 1990–2021	Outside all study urban areas	Inside another study urban area	All external events
Event–area pairs	6,931	1,549	8,480
Share of pairs	81.7%	18.3%	100.0%
Area-years with positive exposure	28.2%	14.6%	33.6%

Panel B. Joint exposure estimates, 2005–2021	Outside all study urban areas	Inside another study urban area	Equality p-value
Registered enterprises	-0.0139*	-0.0029	0.242
	(0.0074)	(0.0062)	
Tax-reported sales	-0.0162	-0.0498**	0.275
	(0.0218)	(0.0225)	

Notes: External events are attacks, coercive actions, and land invasions outside the receiving area and within 25 km. Panel A covers 1990–2021. The component exposures have a correlation of 0.449. Panel B enters both exposures jointly for 2005–2021 with area and region-by-year effects, the three predetermined trends, and contemporaneous within-area conflict. Standard errors are clustered by area. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.2 Conflict composition and protest exposure

Nearby conflict and protest exposure have a correlation of 0.718 across area-years in 2005–2021. Controlling for external and within-area protests leaves the enterprise and sales estimates close to baseline, including with a pre-2005 protest-history trend (Table B.2).

MACEDA codes most contentious actions (attacks and land invasions) to Mapuche actors and most coercive actions to the State. The coding uses `ACTOR1` when the source does not identify an initiator. In the joint model, the coercive-action estimate is negative for both outcomes and more precise for sales. The equality tests do not distinguish the contentious and coercive coefficients. State responses may be concentrated where underlying conflict is more severe.

Table B.2: Conflict composition, protest exposure, and formal activity

Exposure / specification	Registered enterprises	Tax-reported sales
Panel A. Main conflict and protest exposure		
Baseline: main-family exposure	-0.0132** (0.0065)	-0.0320* (0.0189)
Protest-only exposure	-0.0073 (0.0061)	-0.0012 (0.0199)
Joint: main-family exposure	-0.0126* (0.0065)	-0.0362* (0.0195)
Joint: protest exposure	-0.0026 (0.0059)	0.0124 (0.0202)
Joint + protest-history trend: main family	-0.0143** (0.0065)	-0.0374* (0.0193)
Joint + protest-history trend: protests	-0.0094 (0.0061)	0.0073 (0.0201)
Panel B. Contentious and coercive actions		
Contentious actions	-0.0140* (0.0077)	-0.0103 (0.0246)
Coercive actions	-0.0033 (0.0065)	-0.0424** (0.0208)
Equality of exposure coefficients (p)	0.296	0.393
Area-years	1,768	1,768
Urban areas	104	104

Notes: The sample covers 104 areas in 2005–2021 (1,768 area-years). Outcomes are logged and counts use $\log(1+x)$. External exposure uses exact point-to-footprint distance of at most 25 km and excludes events inside the receiving area. All models include area and region-by-year effects and the three predetermined trends, with standard errors clustered by area. In Panel A the baseline controls for contemporaneous within-area main-family conflict. The protest-only and joint models also control for contemporaneous within-area protests. The protest-history trend uses pre-2005 external protests. Panel B enters both external exposures jointly with their matching contemporaneous within-area exposures. Attacks and land invasions are summed before transformation. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.3 Transformation and inference sensitivity

An increase from one to ten events gives similar enterprise and sales estimates under $\log(1+x)$ and asinh (Table B.3).

Table B.3: Comparable raw-count contrasts under consistent conflict transformations

Outcome	Transformation	1 to 10 events (%)	95% CI	p
Registered enterprises	$\text{Log}(1+x)$	-2.22	[-4.34, -0.06]	0.045
Registered enterprises	Consistent asinh	-2.13	[-4.12, -0.10]	0.040
Tax-reported sales	$\text{Log}(1+x)$	-5.30	[-11.17, 0.95]	0.094
Tax-reported sales	Consistent asinh	-5.53	[-11.19, 0.50]	0.071

Notes: For logged outcomes the percent change is $100\{\exp[\hat{\beta}(g(10)-g(1))]-1\}$. Each transformation g applies to current external and within-area counts and to the pre-2005 count before its trend interaction. Both models use the same 1,768 area-years and other controls, with area-clustered confidence intervals.

The wild-cluster bootstrap gives larger p-values for sales than for enterprises. Spatial-HAC gives smaller p-values for both outcomes (Table B.4).

Table B.4: Alternative inference for headline formal outcomes

Outcome	Coef.	Clustered SE	Wild p	Spatial-HAC SE	Spatial-HAC p
Registered enterprises	-0.0132	(0.0065)	0.041	(0.0053)	0.013
Tax-reported sales	-0.0320	(0.0189)	0.079	(0.0136)	0.019

Notes: Headline area-clustered intervals use a t distribution with 103 degrees of freedom. The wild-cluster bootstrap uses 999 repetitions clustered by area. Spatial-HAC uses a normal reference distribution, a 60 km cutoff, and three annual lags. Results are similar with 30 and 90 km cutoffs.

B.4 Province influence

The enterprise and sales estimates remain negative after omitting each province in turn (Figure B.1).

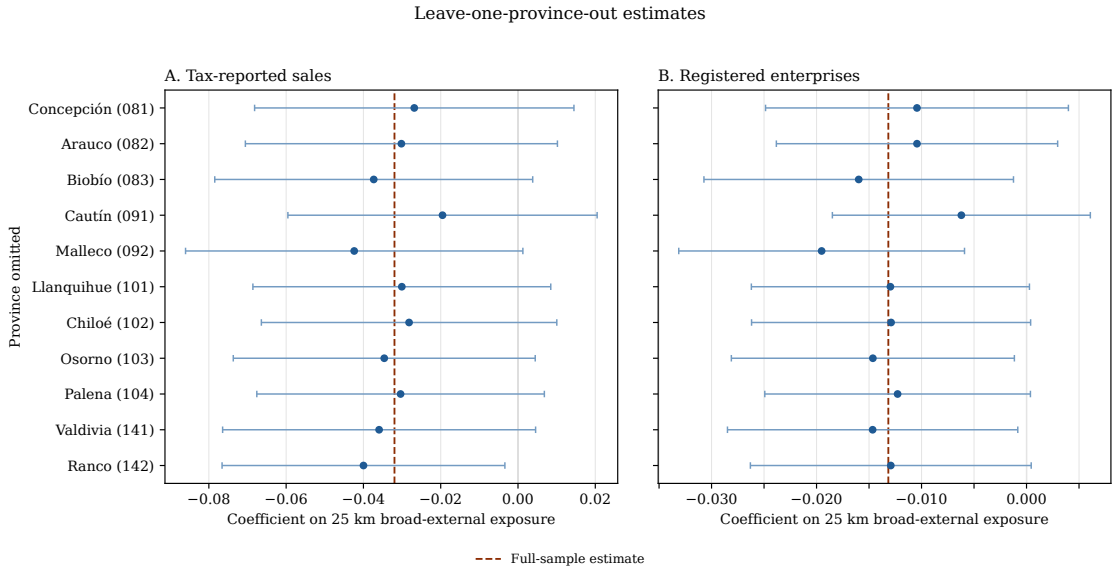


Figure B.1: Leave-one-province-out estimates. Vertical lines mark the full-sample estimates and bars show 95 percent confidence intervals.

B.5 Distance sensitivity

The enterprise and sales estimates remain negative across contemporary exposure radii from 10 to 60 km (Table B.5).

Table B.5: Cumulative external-conflict windows and formal activity

External window	Registered enterprises	Tax-reported sales
Within 10 km	-0.0133* (0.0078)	-0.0209 (0.0245)
Within 20 km	-0.0138** (0.0055)	-0.0292 (0.0195)
Within 25 km (preferred)	-0.0132** (0.0065)	-0.0320* (0.0189)
Within 30 km	-0.0181*** (0.0064)	-0.0478*** (0.0163)
Within 40 km	-0.0172*** (0.0062)	-0.0456** (0.0225)
Within 50 km	-0.0170** (0.0070)	-0.0414* (0.0238)
Within 60 km	-0.0166** (0.0079)	-0.0451* (0.0246)

Notes: Only the radius for contemporary log external exposure varies. Historical exposure uses external events within 25 km in 1990–2004 throughout. All other controls and area clustering follow the preferred specification. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.6 Forestry exclusions

The enterprise estimates remain close to baseline after either forestry exclusion, with 95 percent confidence intervals that include zero (Table B.6).

Table B.6: Registered enterprises excluding forestry and related manufacturing

Outcome definition	Coefficient	SE	p-value	Area-years
All activity	-0.0132	0.0065	0.045	1,768
Excluding narrow forestry	-0.0122	0.0066	0.067	1,768
Excluding broad forestry	-0.0127	0.0067	0.062	1,768

Notes: Narrow forestry excludes SII groups 021–024 (silviculture and nurseries, logging, nonwood forest products, and forestry services). Broad forestry also excludes groups 161, 162, and 170 (sawmilling, wood and related products, and pulp and paper). Firms are counted by assigned activity. Models use logged enterprise counts on positive support and the preferred specification for 104 areas in 2005–2021, with standard errors clustered by area.

B.7 Margins of adjustment

Administrative registrations are negatively associated with exposure in 2005–2021. In 2005–2015 the registration estimate is 0.0066 (SE 0.0087), while the enterprise-stock estimate is −0.0180 (SE 0.0083). SII publishes identical records in files labeled *primera inscripción* and *actividades inscritas*, despite defining the series separately. I use the former as a measure of administrative registration activity.

SII administrative actions increased recorded terminations nationally in 2016–2017. The reference window ends in 2015, when the termination estimate is imprecise (Table B.7). Omitting 2016–2017 from the full panel still pools observations on both sides of the change. Terminations cover the taxpayer universe and omit taxpayers who restarted before extraction. Together with the registration-source limitation, this coverage prevents interpreting the two series as firm births and exits or using them to decompose enterprise stock.

Table B.7: Enterprise stock, registrations, and terminations

Outcome	Window	Coefficient	SE	p	N
Enterprise stock	2005–2021	-0.0132**	0.0065	0.045	1,768
Registration activity	2005–2021	-0.0144*	0.0073	0.053	1,768
Current terminations	2005–2015	-0.0116	0.0218	0.595	1,144
Terminations, full window	2005–2021	-0.0319**	0.0160	0.0497	1,768
Terminations, omit 2016–2017	2005–2021	-0.0309*	0.0164	0.063	1,560

Notes: All rows use the preferred specification and 104 areas, with standard errors clustered by area. Enterprise stock is logged. Registrations and terminations use $\log(1 + x)$, and termination estimates include zero-count area-years. The last two rows vary the termination window around the 2005–2015 reference sample. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.8 Transport-related conflict and adjustment margins

The enterprise-stock estimate is more negative for events at transport locations than for other nearby events. Registrations show a similar pattern, including when the comparison is restricted to attacks. The pre-2016 termination contrast is imprecise (Table B.8). The comparison uses recorded event locations. MACEDA does not observe resulting disruptions between markets.

Table B.8: Transport and nontransport conflict by outcome

Outcome / specification	Transport	Nontransport	Transport − nontransport	Equality p-value
Enterprise stock	-0.0266**	-0.0081	-0.0185*	0.077
	(0.0105)	(0.0057)	(0.0104)	
SII first-registration series	-0.0400***	-0.0047	-0.0353**	0.028
	(0.0120)	(0.0076)	(0.0158)	
First-registration series: attacks only	-0.0394***	-0.0039	-0.0355*	0.078
	(0.0134)	(0.0105)	(0.0199)	
Terminations, 2005–2015	-0.0803	0.0038	-0.0841	0.160
	(0.0527)	(0.0218)	(0.0595)	

Notes: Transport events are attacks, coercive actions, and land invasions coded `ROAD`, `BRIDGE`, or `HIGHWAY`. Other events in those families form the comparison group. Both exposures use $\log(1 + x)$ counts of events outside the receiving area and within 25 km. They enter the preferred specification jointly, including in the attacks-only comparison, which retains the contemporaneous within-area main-family control. Standard errors are clustered by area. The sample covers 104 areas and 1,768 area-years in 2005–2021 (1,144 for 2005–2015 terminations). Enterprise stock is logged. Registrations and terminations use $\log(1+x)$ and follow the definitions in Appendix B.7. The difference is a linear contrast. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C Conflict escalations

I compare outcomes around two types of escalation. A historical extreme-frequency spike occurs when the annual external event count A_{mt} meets

$$S_{mt}^{\mathrm{HF}} = \mathbf{1}\{A_{mt} \geq 3,\ A_{mt} > \mu_{mt}^{10} + 4s_{mt}^{10}\}, \tag{4}$$

where μ_{mt}^{10} and s_{mt}^{10} are the mean and sample standard deviation over the ten preceding years. All ten years must be observed, so classification begins in 2000. The count follows the 25-km external definition in Equation (1): attacks, coercive actions and land invasions outside the receiving area, including events in another city. Threats and records without a specified day remain in the annual count. The second definition uses records documenting the burning or destruction of an identified productive or freight asset within the same external window.

For each definition, G_m is the first escalation year in the available history. The Callaway and Sant'Anna (2021) comparison treats exposure as absorbing from that year, $F_{mt} = \mathbf{1}\{G_m \leq t\}$. It compares changes from year $G_m - 1$ with changes in same-region municipalities never exposed or first exposed after both comparison years, excluding the focal cohort. Region-by-cohort cells are weighted by their shares of treated municipalities. The sample excludes municipalities first exposed at or before the outcome baseline.

The repeated comparison uses all supported years g with an escalation, including first episodes, continuations and re-entries. It compares changes from $g - 1$ with the mean change among same-region municipalities without an escalation from $g - 3$ to $g + 5$. Municipalities receive equal total weight, divided equally across their supported episodes. The donor sensitivity requires no escalation only through g, keeping the same episodes and weights while allowing later donor exposure.

Both comparisons omit the main specification's predetermined trends and within-area conflict control. Inference groups contributions by original municipality, including reused donors and overlapping windows. Individual conflict incidents can expose several municipalities, creating dependence across them.

Enterprise estimates are more negative after first frequency spikes than around repeated spikes (Figure C.1). Sales and total appraisal estimates are imprecise. The two nonreference leads are jointly significant for lights in the repeated comparison ($p = 0.0063$). The corresponding test for first-episode enterprises gives $p = 0.0907$. Enterprises and sales cover 2005–2021. Lights cover the same years with seven missing observations, leaving 100 complete municipalities for C&S. Appraisal covers 2010–2021 and has three supported cohorts.

For destructive events, the repeated estimates are more negative at year +5 for enterprises and sales (Figure C.2). The first-episode estimates at that horizon are imprecise, and the two first-episode sales leads are jointly significant ($p = 0.0397$). Allowing donors to experience later escalations moves the repeated +5 contrast from -0.0584 to -0.0280 for enterprises and from -0.1762 to -0.0860 for sales, on the same 174 focal municipality-years.

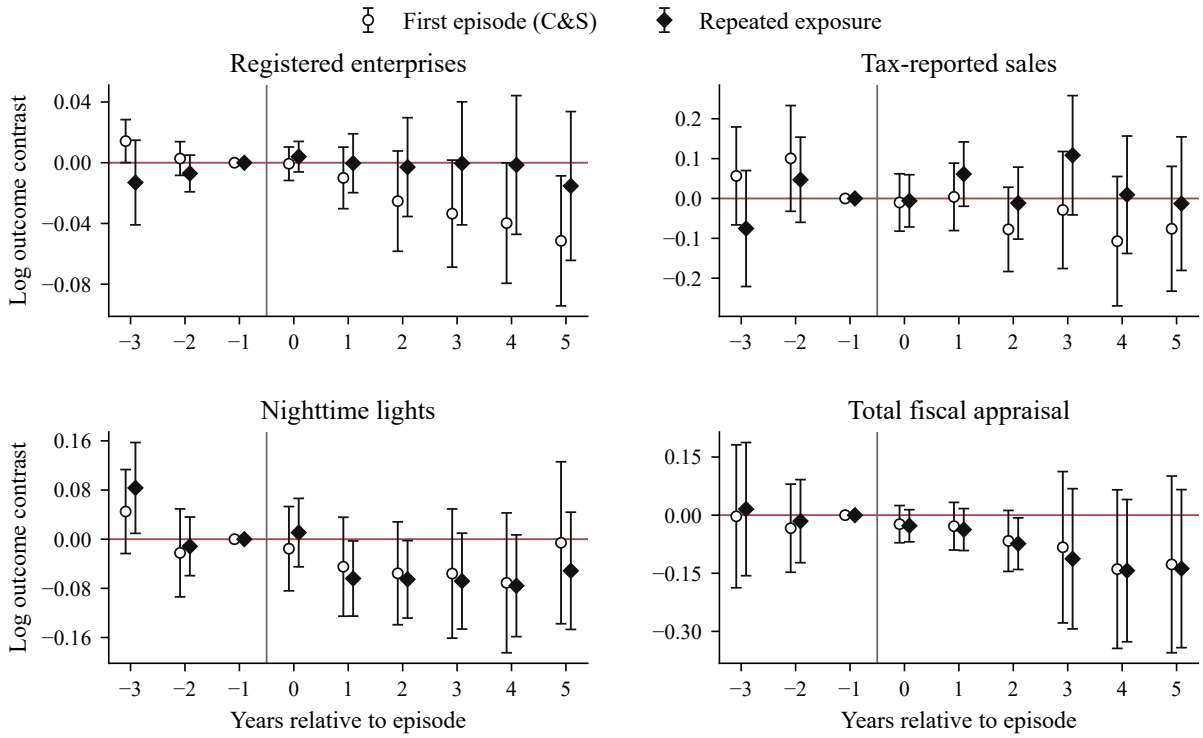


Figure C.1: First and repeated exposure around historical frequency spikes. Profiles use a fixed sample for each outcome and design, from $k = -3$ to $+5$, with the episode calendar year at zero and $k = -1$ as reference. Whiskers show pointwise 95 percent intervals based on the municipality-level covariance. For enterprises, sales and lights, the first-episode sample has 19 municipalities (19 episodes) and the repeated sample has 29 (30 episodes). Appraisal uses 10 (10) and 11 (11), respectively.

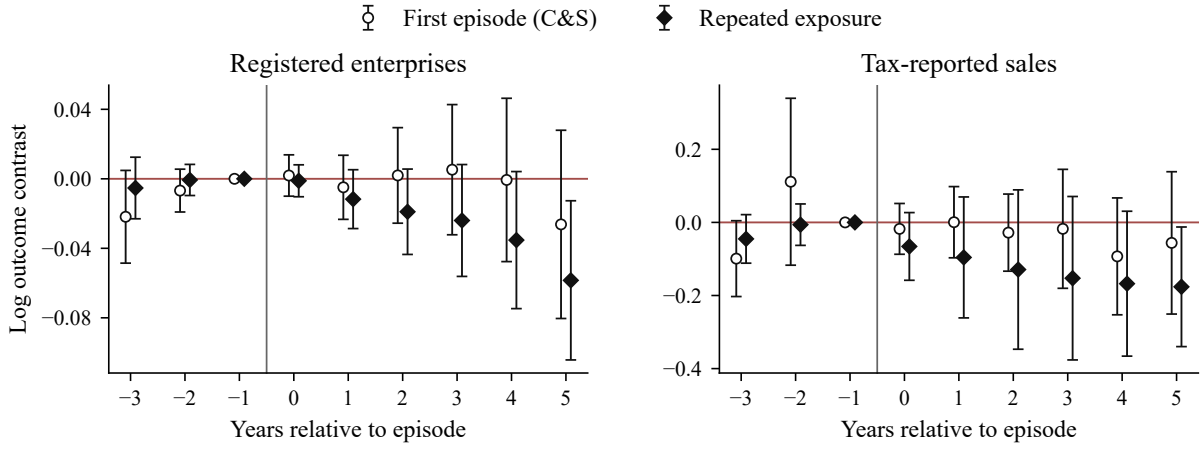


Figure C.2: First and repeated exposure around documented destructive events. The first-episode sample has 30 municipalities (30 episodes) and the repeated sample has 47 (174 focal municipality-years) for both outcomes. Event years, reference and pointwise 95 percent intervals follow Figure C.1.

D Interpretation of municipal business outcomes

D.1 Registered legal entities

SII assigns registered enterprises to municipalities by tax address. Table D.1 compares that series with four counts from the legal-entity (*persona jurídica*) panel. The total precedes temporal assignment and cadastral matching. The matched sample uses high-confidence links between historical headquarters addresses and cadastral parcels, which place headquarters inside or outside the fixed urban footprints. The linked subsets are selected, and headquarters addresses need not locate production or sales.

The urban-headquarters estimate is −0.0466 (SE 0.0221). The rural-headquarters estimate is 0.0073 (SE 0.0269) and is imprecise.

Table D.1: External conflict and registered legal entities

Outcome	Transformation	Coefficient	Clustered SE	Zero area-years
Registered enterprises	$\log(x)$	-0.0132**	0.0065	0
Total registered legal entities	$\log(x)$	-0.0073	0.0107	0
Cadastre-matched legal entities	$\log(1+x)$	-0.0335	0.0230	109
Cadastre urban legal entities	$\log(1+x)$	-0.0466**	0.0221	152
Cadastre rural legal entities	$\log(1+x)$	0.0073	0.0269	913

Notes: The matched count includes the urban and rural headquarters subsets. Zero-count area-years are retained for the $\log(1+x)$ outcomes. Standard errors are clustered by area. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D.2 Rural production linkages

I test whether nearby conflict has a stronger association with sales and registered enterprises in sectors more closely linked to rural production. The Banco Central de Chile input-output matrices give the shares of each sector's domestic intermediate purchases from rural sectors (input linkages) and domestic intermediate sales to them (customer linkages). The rural group covers agriculture, fruit growing, livestock, and forestry, excluding fishing and aquaculture. The preferred 2003 matrix is the closest benchmark before 2005–2021, and the 1996 matrix provides an earlier comparison. I standardize both measures across the ten comparable non-rural sectors.

Let L_s denote the standardized linkage for sector s. I estimate

$$Y_{mst} = \beta\,(D_{mt} \times L_s) + \rho\,(I_{mt} \times L_s) + \alpha_{ms} + \gamma_{st} + \delta_{mt} + \varepsilon_{mst}, \tag{5}$$

where D_{mt} and I_{mt} are external and within-area conflict. The fixed effects compare sectors within the same area and year, accounting for persistent area-sector differences and sector-wide annual shocks.

Table D.2: External conflict and predetermined rural sectoral linkages

Benchmark	Linkage	Sales interaction	Enterprise interaction
2003	Customer	-0.0168 (0.0279)	0.0046 (0.0050)
2003	Input	-0.0008 (0.0382)	-0.0034 (0.0034)
1996	Customer	-0.0167 (0.0378)	0.0021 (0.0054)
1996	Input	0.0033 (0.0369)	-0.0028 (0.0036)

Notes: Each entry reports β, the interaction of log external conflict exposure within 25 km with the indicated standardized linkage. Models include the corresponding within-area conflict interaction and area-by-sector, sector-by-year, and area-by-year fixed effects. Standard errors clustered by fixed urban area are in parentheses.

For either benchmark, the estimates show no systematic tendency for the enterprise or sales association to become more negative as rural input or customer linkages strengthen (Table D.2). Rural–urban economic connections remain a possible transmission channel.

E Historical heterogeneity

I measure historical State-recognized claims by the share of terrestrial land in each area's external 0–25 km catchment covered by *Títulos de Merced* (TDM). These boundaries do not measure current Indigenous ownership. Of the 104 areas, 43 have no TDM coverage in the catchment, and the positive variation is concentrated in Araucanía. I reconstruct the urban Mapuche population share from the 1992 Census on the fixed urban geography. Curaco de Vélez and Palena lack corresponding historical records, leaving 102 areas in the Mapuche-share and joint models.

Figure E.1 shows how the association with external conflict changes for a one-standard-deviation increase in each moderator. The enterprise and sales associations are not systematically more negative where historical TDM coverage or Mapuche population shares are higher. This leaves open a role for property-rights uncertainty or perceptions of insecurity.

Wild-cluster tests of the original joint-model interactions give $p \geq 0.174$. The estimates show no consistent pattern when the TDM catchment varies from 10 to 50 km with conflict exposure fixed at 25 km. Using 30 km for both boundaries and allowing region-specific conflict slopes yields the same qualitative result.

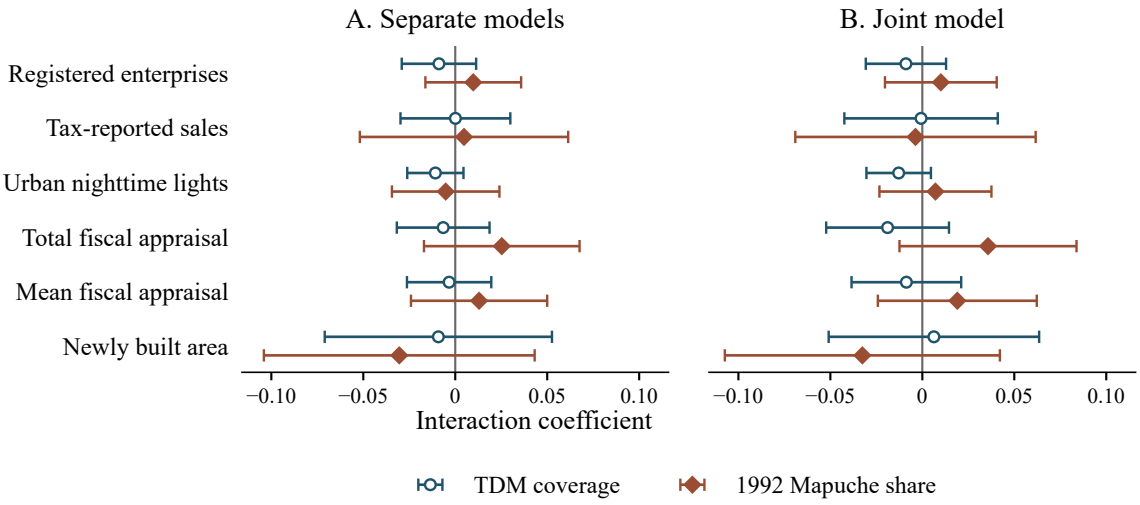


Figure E.1: External conflict interactions with historical land coverage and population. Panel A uses 104 areas for TDM and 102 for Mapuche share. Panel B uses 102 areas. Bars are pointwise 95 percent confidence intervals based on area-clustered standard errors. Models include area and region-by-year effects, contemporaneous within-area conflict, moderator trends and moderator interactions with within-area conflict. Formal outcomes and lights use 2005–2021 and appraisal uses 2010–2021, with the three predetermined trends. Built area omits those trends and covers 1993–2015 in separate models and 1992–2015 in the joint model.

F Institutional response: CONADI

I combine published CONADI records for Article 20(a) land-acquisition subsidies and Article 20(b) purchases related to historical territorial claims. I assign records by parcel municipality, count polygon parts with the same acquisition key as one group, and sum their reported hectares. The recorded year does not distinguish approval, registration, and delivery.

Greater external conflict exposure is associated with more recorded acquisition groups and reported hectares two years later (Figure F.1). The one-year estimates are imprecise. The sum of the current and two lagged coefficients is positive for both outcomes.

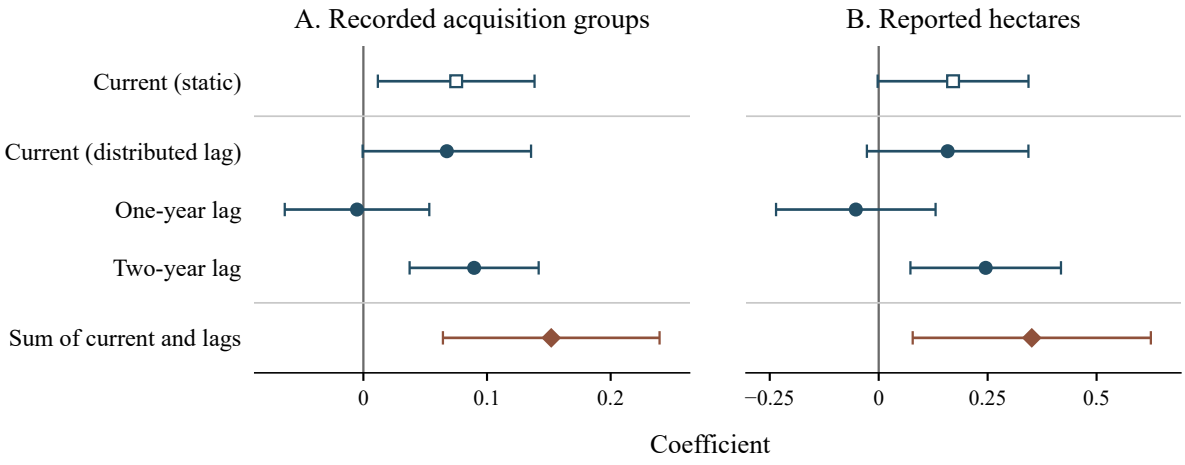


Figure F.1: External conflict exposure and recorded CONADI land acquisition. Records pool Articles 20(a) and 20(b). Squares show the static estimate. Circles show the three distributed-lag terms. Diamonds show their sum, with intervals based on the full covariance matrix. Both outcomes use $\log(1 + x)$. Whiskers show 95 percent confidence intervals based on area-clustered standard errors. The sample covers 104 areas in 2005–2021. Joint tests of the three distributed-lag terms give $p = 0.003$ for acquisition groups and $p = 0.022$ for hectares.