The Wrong Snowball: Reintegration Signals and Demobilization in Colombia

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Abstract

Reintegration programs can influence demobilization behavior beyond enrolled participants by transmitting public signals about post-exit life. In Colombia, one of the most visible milestones in reintegration is the state-funded opening of a business: after laying down arms, ex-combatants who complete a transition that often lasts years become eligible for seed capital, so each opening signals whether the state is delivering on a central promise. Whether such signals encourage or discourage future defections is theoretically ambiguous: successful openings may generate a positive demonstration effect, but fragile post-opening trajectories and intimidation risk may reverse it. Using municipality-quarter administrative data and a stacked difference-in-differences design, we find that business openings by ex-combatants are followed by lower demobilization counts in the same area, implying about 734 fewer demobilizations over the next three years, or 5.1 percent of observed FARC demobilizations in the 2005–2016 study period. Quantitative heterogeneity and coded focus-group evidence point to two complementary mechanisms: an expectation gap between the program's formal promise and observed post-opening fragility, and persistent intimidation risk that raises the perceived security cost of exit. Effects are largest for commerce and service businesses—the most publicly visible sector types—and in municipalities with higher ex-combatant death rates, consistent with a mechanism that operates through the local observability of post-exit trajectories and perceived security risk. The results point to a broader lesson for ongoing and future DDR processes: reintegration generates a positive snowball only when implementation quality and post-exit security are sufficient to shift beliefs favorably; otherwise, the same visibility on which DDR policy relies can backfire.

Keywords: demobilization; reintegration; peer effects; political violence; stacked difference-in-differences; Colombia

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Introduction

Disarmament, Demobilization, and Reintegration (DDR) programs are among the few policy instruments that can simultaneously affect violence in the short run and state legitimacy in the long run (Armand et al., 2020; Schulhofer-Wohl & Sambanis, 2010). In practice, however, they are often evaluated as if they only changed outcomes for the individuals directly enrolled. That framing misses a central social mechanism. In high-risk conflicts, potential defectors update beliefs about life after exit by observing what happens to previous defectors (Kaplan & Nussio, 2018; Oppenheim et al., 2015). This mechanism is a special case of a broader pattern in which publicly visible program outcomes shape take-up decisions among non-participants (Karing, 2023; Bhuller et al., 2020). Reintegration outcomes therefore operate as public information. The key question is whether that information encourages or discourages future demobilization.

In Colombia, that informational channel is especially salient because demobilization does not end when a combatant lays down arms. Individuals who enter the reintegration pathway move through a years-long, milestone-based process that includes psychosocial support, education, and economic stabilization before they become eligible for state seed capital to open a productive project or business unit (Presidencia de la República de Colombia, 2011a; Agencia para la Reincorporación y la Normalización, 2021; Agencia para la Reincorporación y la Normalización, 2022). Business openings are therefore not incidental outcomes. They are the public culmination of a state-promised reintegration pathway and one of the first moments at which peers and local networks can observe whether that pathway appears to deliver.

Theory provides competing predictions about how observers interpret that milestone. A positive snowball is the default expectation in the DDR literature: when former combatants reach that visible stage and their ventures appear successful, local networks observe that post-exit economic life is viable. That demonstration effect lowers perceived uncertainty for potential defectors, signals that the state's reintegration offer is credible, and suggests that participants can operate without immediate retribution (Schulhofer-Wohl & Sambanis, 2010; Kaplan & Nussio, 2018). Colombia's own communication strategy leaned on this logic by framing reintegration as a concrete, observable route back into civilian life and by amplifying stories of successful economic reintegration (Fattal, 2018; Agencia para la Reincorporación y la Normalización, 2021). Under this view, each opening becomes a favorable signal that compounds over time.

A negative snowball is equally plausible. The same opening can reveal economic fragility. If observers see businesses launch but then struggle because of closures, inadequate capital, or poor market fit, they update toward a view of post-exit life as precarious rather than viable (Oppenheim & Söderström, 2018; Fundación Ideas para la Paz, 2022). The same opening can also reveal security exposure. When intimidation risk remains salient, opening a productive project makes a participant more identifiable in local networks and can raise the perceived risk of intimidation or retaliation (Defensoría del Pueblo de Colombia, 2021; Kaplan & Nussio, 2018; EX020–EX022). When implementation quality is low and post-exit security is unreliable, the snowball can switch sign. Since these mechanisms predict opposite directions, the question of which dominates is empirical.

This paper studies that question in Colombia. We use municipality-quarter administrative data and a stacked difference-in-differences design to estimate how local business openings by demobilized ex-combatants shape future demobilization in the same area. The core estimation window is 2005–2016, starting with the institutional rollout of the reintegration route under study and ending before the post-agreement shift to collective FARC demobilization in 2016 (Agencia para la Reincorporacion y la Normalizacion, 2020; Congreso de Colombia, 2005; United Nations Security Council, 2022). We find that opening episodes are followed by lower subsequent FARC demobilization counts, not higher ones. Over the next three years, the implied decline is about 734 demobilizations, or 5.1 percent of observed FARC demobilizations in the 2005–2016 study period.

This pattern is most consistent with an expectation gap reinforced by persistent intimidation risk. The reintegration pathway is long and milestone-based, and participants report that delivered economic support often falls short of what is needed for sustained operation (EX012; Q005, Q011). That gap between the state's formal promise and observed post-opening fragility is precisely the kind of information that travels through peer networks and discourages additional exits. When threat is perceived as credible, security concerns reinforce the same negative update (Q019–Q021, EX020–EX022).

The paper's core distinction is between first-order and second-order effects. First-order outcomes concern participants already inside the reintegration pathway: completion, business operation, social integration, and recidivism risk (Kaplan & Nussio, 2018; Oppenheim & Söderström, 2018). Second-order outcomes concern individuals still inside armed groups who are deciding whether to leave. A policy can perform reasonably on first-order indicators and still transmit an adverse second-order signal if observed post-exit trajectories appear economically fragile or unsafe. That is the margin this paper studies.

Empirically, the design relies on repeated local opening episodes and on administrative timing variation in when approved projects become operationally visible. Participants can influence project type and location, but not the exact quarter in which an opening becomes locally observable, because approvals, compliance checks, provider disbursements, and procurement introduce timing uncertainty (Presidencia de la República de Colombia, 2011a; Agencia para la Reincorporación y la Normalización, 2022; Agencia para la Reincorporación y la Normalización, 2021). A stacked event-study framework is therefore appropriate: it preserves an episode-centered comparison structure and avoids well-known weighting problems that can affect conventional two-way fixed-effects event studies in staggered settings (Cengiz et al., 2019; Callaway & Sant'Anna, 2021; Chaisemartin & D'Haultf, 2020; Goodman-Bacon, 2021; Sun & Abraham, 2021). The formal diagnostics later in the paper evaluate this institutional timing argument rather than substitute for it.

The administrative archive is also unusual for this literature. It links demobilization flows, business openings, and ex-combatant deaths in a unified framework with broad national coverage (Agencia para la Reincorporación y la Normalización, 2015; Agencia para la Reincorporación y la Normalización, 2025; Defensoría del Pueblo de Colombia, 2021). Two focus groups — one with ex-combatants and one with ARN professionals — complement those records by documenting implementation realities and by clarifying why the estimated effects are behaviorally plausible in this setting (Fattal, 2018; Kaplan & Nussio, 2018; Oppenheim & Söderström, 2018). The qualitative evidence is used to interpret the quantitative effects, not to replace them.

The paper contributes to the DDR and conflict literature by shifting attention from participant outcomes to what potential future defectors observe and believe. Existing work mainly studies first-order outcomes such as employment, recidivism, and social reintegration (Armand et al., 2020; Blattman & Annan, 2010; Gilligan et al., 2013; Kaplan & Nussio, 2018; Mvukiyehe & Samii, 2010; Oppenheim & Söderström, 2018). This paper instead estimates a second-order spillover: how observed reintegration trajectories shape the willingness of others to exit (McLauchlin, 2015; Nussio, 2013; Nussio, 2018; Richards, 2018). Related work on Colombia also shows that ex-combatants differ substantially in their ideological commitments and postwar orientations (Ugarriza & Craig, 2013).

It also contributes to empirical work on heterogeneous-timing difference-in-differences and to the quantitative literature on Colombian conflict dynamics. Methodologically, it provides a repeated-episode stacked event-study implementation with explicit support and pre-trend diagnostics. Substantively, it shows that a state administrative process can itself become part of the local information environment that shapes armed behavior (Acemoglu et al., 2013; Blair et al., 2022; Weintraub et al., 2015). Dube and Vargas (2013) show that exogenous commodity price shocks propagate through local economies to shape conflict intensity in Colombian municipalities; the present paper identifies a complementary channel in which policy-generated economic signals—rather than market income shocks—affect armed behavior through peer-network information updating. Fergusson et al. (2022) demonstrate that political competition conditions the local delivery of state programs in Colombia; the mechanism documented here operates downstream of that delivery: it is not only whether a program reaches communities but what peers observe about post-exit trajectories once it does that determines the sign of any behavioral spillover. Galindo-Silva (2021) shows that local political openness also conditions conflict dynamics in Colombian municipalities, and Rodríguez and Sánchez (2012) show that violence exposure reshapes civilian human-capital investment and child labor decisions. In policy terms, implementation credibility and post-exit security are not peripheral details; they are part of the signal that peers observe and respond to.

Background

Conflict history and strategic role of demobilization

Colombia's conflict is better understood as a long sequence of territorial competitions than as a single war episode. Since the 1960s, guerrilla organizations, state forces, paramilitary structures, and later successor armed groups have competed for local authority, economic rents, mobility corridors, and social control. This territorial logic matters empirically because armed behavior has repeatedly responded to local incentives rather than only to national political shocks (Acemoglu et al., 2013; Blair et al., 2022; Dube & Vargas, 2013; Fergusson et al., 2022; Weintraub et al., 2015). Arjona (2016) further shows that armed actors do not merely fight over territory; they also shape local wartime social order, and Ibáñez et al. (2024) show that variation in that wartime rule can leave durable economic legacies.

The political economy of this conflict also made demobilization policy strategically central. In high-friction settings, an exit program does more than change the lives of those who enroll. It also generates information for those who remain armed and are considering whether to defect. For that reason, demobilization has always had a dual function in Colombia: immediate reduction in active combatants and longer-run expectation management inside armed organizations (Kaplan & Nussio, 2018; Nussio, 2013).

This expectation channel became especially salient in periods when the state expanded legal and institutional pathways for exit while violence persisted unevenly across territory. A municipality can observe both a formal policy message and local evidence about whether post-exit life is economically viable and physically safe. Those local observations are exactly where second-order spillovers emerge: visible success can encourage additional exits, while visible fragility or intimidation can discourage them.

Against this background, the question in this paper is not whether demobilization programs matter in general, but what type of social signal they transmit once implementation unfolds on the ground. That framing is important in Colombia because the same national policy can produce very different local perceptions depending on administrative timing, market conditions, and security context.

Reintegration architecture, participant pathway, and timing uncertainty

Colombia's reintegration architecture was built incrementally and adapted across conflict stages. The legal base includes Law 418 (1997), Decree 128 (2003), Law 975 (2005), and Decree 423 (2007), later complemented by Decree 1391 (2011) for economic support design. Institutionally, implementation moved from earlier presidential structures to ACR (Decree Law 4138 of 2011) and then ARN (Decree Law 897 of 2017), with reincorporation instruments for former FARC-EP members under Decree Law 899 and policy anchors such as CONPES 3554 and CONPES 3931 (Congreso de Colombia, 1997, 2005; Departamento Nacional de Planeación, 2008, 2018; Presidencia de la República de Colombia, 2003, 2007, 2011a, 2011b, 2017a, 2017b). The sequence and its operational implications are summarized in Table 1.

Table 1: Timeline of major institutional changes and operational implications for reintegration timing.

Year	Institutional/legal change	Operational consequence for participant timing
1997	Law 418	Establishes a formal legal basis for demobilization and reintegration policy.
2003	Decree 128	Defines implementation pathways through executive regulation rather than one-step transfers.
2005-2007	Law 975 and Decree 423	Expands transitional-justice and reintegration procedures, adding compliance stages and documentary requirements.
2011	Decree 1391 and Decree Law 4138	Formalizes milestone-based economic support and institutionalizes ACR, increasing procedural sequencing before disbursement.
2017	Decree Law 897 and Decree Law 899	Transitions ACR to ARN and defines reincorporation instruments for former FARC-EP members.
2018	CONPES 3931	Updates policy architecture for reincorporation and implementation coordination.

Operationally, the participant pathway is long and conditional. Demobilization initiates entry into a monitored route that combines psychosocial, educational, and economic components, not immediate unrestricted cash transfers. On the economic side, participants can follow employability support or productive-project support. In the FARC reincorporation stage, the productive-project branch has become dominant: ARN reports 11,066 beneficiaries out of 12,129 active signatories with productive-project support as of January 2025 (about 91 percent) (Agencia para la Reincorporación y la Normalización, 2025).

The treatment analyzed in this paper is tied to that visible economic component. An opening episode starts in the first municipality-quarter with positive FARC-linked business-unit openings and remains active while openings continue in consecutive quarters. The outcome is municipal FARC demobilization count per quarter. Substantively, the estimand asks whether visible openings in a local setting are followed by more exits or fewer exits in subsequent quarters.

Timing uncertainty is the institutional cornerstone of the design. Under Decree 1391 and ARN procedures, productive support is milestone-based and provider-mediated. Participants can shape project type, sector, and location, but not the precise calendar quarter in which an approved project becomes operationally visible (by which we mean that the business unit is physically open and observable in the local community, distinct from earlier administrative milestones such as viability approval or disbursement) (Agencia para la Reincorporación y la Normalización, 2021, 2022; Presidencia de la República de Colombia, 2011a). Focus-group testimony from ARN staff and ex-combatants repeatedly describes this implementation uncertainty through document revisions, compliance loops, procurement delays, and individualized processing windows (ARN004, ARN008, EX007, EX009, EX010, EX011).

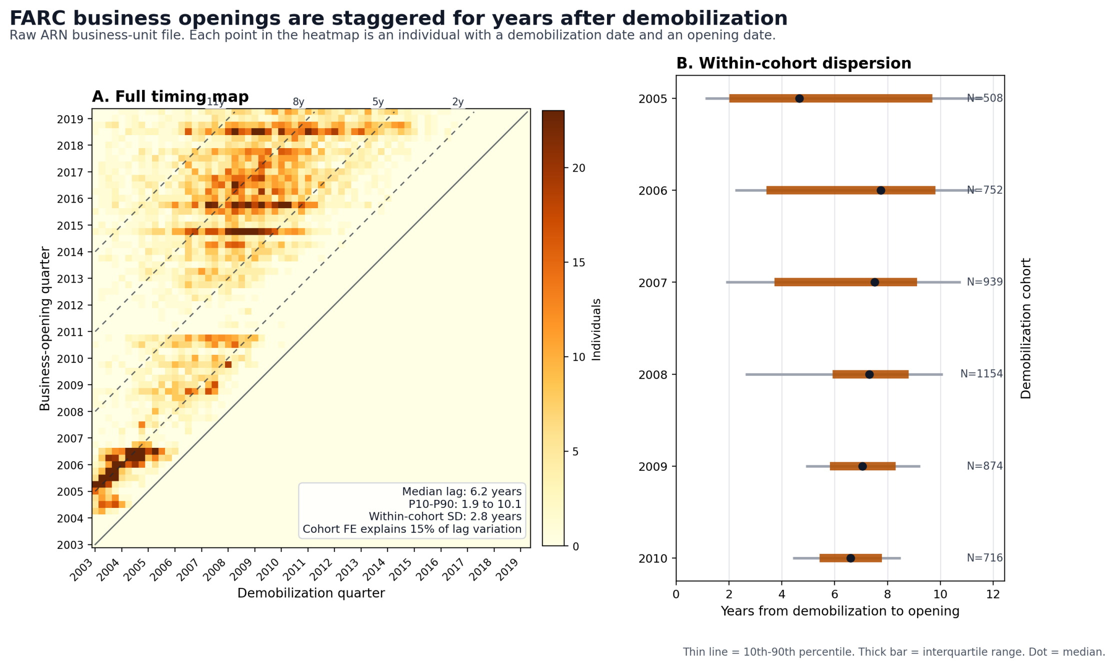

Figure 1: Timing from demobilization to business opening among FARC ex-combatants in the raw ARN business-unit file. Panel A plots demobilization quarter against opening quarter for individual observations. Panel B summarizes within-cohort dispersion in years from demobilization to opening; thin line = 10th–90th percentile, thick bar = interquartile range, dot = median.

Figure 1 makes that timing heterogeneity visible. Panel A shows that openings are spread over many years after demobilization, and Panel B shows substantial within-cohort dispersion in time to opening. The median lag is 6.2 years, the 10th-90th percentile range is 1.9 to 10.1 years, and demobilization-cohort fixed effects explain only 15 percent of lag variation.

The second background pillar is social visibility. In the municipalities studied here, openings are discussed in local networks and interpreted as public signals about what post-exit life actually looks like (ARN013, ARN014, EX013, EX014). Observers do not need access to full administrative records; they can see whether a business opens, whether it survives, and whether the participant appears safe.

That visibility intersects with two persistent realities documented in program follow-up and policy assessments: economic fragility and security exposure (Agencia para la Reincorporación y la Normalización, 2015; Defensoría del Pueblo de Colombia, 2021; Fundación Ideas para la Paz, 2022). The first source is the Instrumento de Seguimiento a Unidades de Negocio (ISUN), the ARN follow-up module for productive projects. In this archive, the four-follow-up business survival rate is 60.3 percent, meaning roughly four in ten business units are no longer operating by that horizon.

ISUN records also show high non-functionality rates in latest-follow-up cuts and closure patterns consistent with economic and security stress across municipality groups (see Table A4).

The second source is the ARN administrative death archive for former participants. In the 2005-2016 window used in the core quantitative analysis, this archive records 635 FARC ex-combatant deaths (about 53 per year) across 211 municipalities. Even when explicit security-coded business closure reasons are a smaller share than economic reasons, these levels of observed violence can still matter disproportionately for perceived risk in peer networks because intimidation signals are highly salient in conflict settings.

Why timing and visibility identify the effect

The identifying claim is deliberately narrow. Participation in reintegration is not assumed random, and location choice of projects is not assumed random. The claim is that, conditional on the stacked design and fixed-effects structure, the exact quarter in which an opening becomes operationally visible contains substantial administrative variation that is difficult to perfectly forecast by participants and local implementers (Agencia para la Reincorporación y la Normalización, 2021, 2022; Presidencia de la República de Colombia, 2011a). This distinction is crucial: the design does not require random participation, only plausibly as-if-random timing at the event-quarter margin after conditioning.

Institutionally, that timing variation comes from milestone sequencing and provider-mediated disbursement. Approval, compliance verification, supplier processes, and local administrative capacity all affect when a project transitions from approved to operationally visible (Agencia para la Reincorporación y la Normalización, 2022; Presidencia de la República de Colombia, 2011a). In our qualitative material, ARN staff and ex-combatants describe that uncertainty in directly comparable terms (ARN004, ARN008, EX007, EX009, EX010, EX011). Figure 1 is consistent with this institutional account: even within demobilization cohorts, time to opening varies widely rather than clustering around a single predictable lag.

This timing logic is paired with a setting in which outcomes are socially visible. Local networks observe whether businesses open, whether they persist, and whether participants appear safe; that is the information channel through which a snowball effect can arise in either direction (Kaplan & Nussio, 2018; Nussio, 2018; Oppenheim & Söderström, 2018). The combination of timing dispersion and local visibility is what makes the causal question empirically tractable in this case.

Given repeated opening episodes and potentially heterogeneous dynamic effects, a stacked event-study design is the main estimator because it preserves an episode-centered comparison structure and avoids well-known weighting and contamination issues that can affect conventional TWFE event studies in staggered settings (Chaisemartin & D'Haultf, 2020; Goodman-Bacon, 2021; Sun & Abraham, 2021).

The design identifies the average dynamic effects of opening episodes on municipal demobilization counts in the observed population and period. It does not claim channel-level structural identification. Mechanism interpretation is disciplined through triangulation between quantitative heterogeneity patterns and coded qualitative evidence, with competing explanations discussed explicitly in the mechanisms section. Causal interpretations are anchored primarily to the $e = 0$ to $e = +6$ window; estimates at longer horizons carry wider uncertainty as potential confounders accumulate.

Credibility is built as a joint argument, not a single test. Institutional evidence motivates the timing assumption; event-study leads and joint pre-trend tests evaluate whether pre-exposure dynamics are consistent with it; and robustness event-study designs in the appendix probe whether the sign is stable under alternative constructions (Sun & Abraham, 2021). Three alternative explanations merit explicit attention before formal diagnostics.

Mean-reversion and pre-existing trends. Municipalities with elevated pre-event demobilization counts may naturally revert to lower levels regardless of opening episodes. This is addressed directly by the pre-treatment event-study coefficients. In the core quarterly design, leads from $e = -8$ to $e = -2$ are close to zero and jointly non-rejecting ($p = 0.36$), which is inconsistent with a pre-existing declining trend that would produce mechanical post-event reversion. The adverse post-event path begins at or after $e = 0$, not before.

Endogeneity of opening timing to FARC internal dynamics. If periods of internal stress or organizational pressure both prompted accelerated program participation and coincided with temporarily elevated demobilization rates, an opening episode could appear to cause a subsequent decline simply because demobilization was already near a local peak. Against this, institutional evidence is inconsistent with participants timing their administrative milestones around internal FARC dynamics: the compliance, disbursement, and verification sequence operates on administrative calendars largely insensitive to external conflict shifts, and the qualitative record shows that participants experience the timing as outside their control (ARN004, ARN008, EX007, EX009, EX010, EX011). The dispersion of episode starts across 39 distinct quarters also makes systematic event-timing around conflict cycles difficult to sustain empirically.

Organizational punishment. FARC imposed costs on members who attempted to leave, through surveillance, coercion, or targeted violence against would-be defectors. If punishment intensity rose precisely in municipalities where program activity became visible—because the group perceived these as higher-defection-risk areas—it could suppress demobilization counts independently of any signal from openings. Two features of the design reduce this concern. First, to the extent punishment pressure is a stable organizational feature that does not systematically shift within the event window, it is absorbed by unit-by-stack fixed effects. Second, the ex-combatant deaths regression (Figure 4) yields an ATT of -0.024 ($p = 0.109$) over the post-event window, indistinguishable from zero. Systematic escalation of targeted violence after openings would produce rising post-event death counts; it does not. This rules out escalating active punishment as the primary driver of the demobilization decline, without dismissing the baseline deterrent role that organizational discipline plays in any armed group context.

Conceptual Framework

We formalize interpretation with a one-period expected-utility model in the spirit of Becker's crime-choice framework (Becker, 1968). The underlying information environment is grounded in the social-learning literature: potential defectors observe outcomes of early movers and update their priors about post-exit payoffs (Banerjee, 1992; Bikhchandani et al., 1992; Conley & Udry, 2010; Dupas, 2014; Ben-Yishay & Mobarak, 2019). In that literature, the sign of the cascade depends on whether early observed outcomes are positive or negative relative to prior beliefs (DeMarzo et al., 2003). This paper's contribution is to connect that logic to an administrative DDR setting where the signal is the visible trajectory of a productive project rather than an abstract action choice. Each potential defector i chooses once between demobilizing ($defect_i = 1$) and remaining armed ($defect_i = 0$). The choice is conditioned on local post-exit signals $s_i = (s_i^E, s_i^C)$: s_i^E captures observed economic viability of exit trajectories, and s_i^C captures observed security-risk exposure after exit.

Let $U(\cdot)$ be utility over payoffs, Y_i be baseline civilian payoff for individual i, $g_i(s_i^E)$ be the signal-driven economic gain from civilian insertion, $p_i(s_i^C) \in [0,1]$ be the probability of punishment/retaliation after exit, and $f_i > 0$ be the payoff loss if that risk materializes. Let U_i^A denote utility from remaining armed. Expected utility from demobilizing is:

$$\begin{aligned} EU_i^D(s_i) = {} & p_i(s_i^C)U\Big(Y_i + g_i(s_i^E) - f_i\Big) \\ & + \Big[1 - p_i(s_i^C)\Big]\, U\Big(Y_i + g_i(s_i^E)\Big) \end{aligned} \tag{1}$$

Define the within-period utility gap as $\Delta_i(s_i) = EU_i^D(s_i) - U_i^A$. A deterministic version of the model chooses demobilization iff $\Delta_i(s_i) > 0$. For empirical interpretation, we use the standard latent-index representation $defect_i = 1\{\Delta_i(s_i) + \varepsilon_i \geq 0\}$, where ε_i is an idiosyncratic unobserved term, so:

$$\Pr(defect_i = 1) = G(\Delta_i(s_i)) \tag{2}$$

where G is the cumulative distribution function of $-\varepsilon_i$. This probability object is therefore a one-period heterogeneity mapping from utility differences into observed choice frequencies, not an additional dynamic period. The first-order derivatives with respect to economic viability and security risk are:

$$\begin{aligned} \frac{\partial \Delta_i}{\partial s_i^E} = {} & g_i'(s_i^E)\Big[p_i(s_i^C)U'\Big(Y_i + g_i(s_i^E) - f_i\Big) \\ & + \Big(1 - p_i(s_i^C)\Big)\, U'\Big(Y_i + g_i(s_i^E)\Big)\Big] > 0 \end{aligned} \tag{3}$$

$$\frac{\partial \Delta_i}{\partial s_i^C} = p_i'(s_i^C)\Big[U\Big(Y_i + g_i(s_i^E) - f_i\Big) - U\Big(Y_i + g_i(s_i^E)\Big)\Big] < 0 \tag{4}$$

under $g_i'(s_i^E) > 0$, $p_i'(s_i^C) > 0$, $f_i > 0$, and $U'(\cdot) > 0$. Equation 3 formalizes that better economic viability signals raise the utility gap and thus demobilization propensity. Equation 4 formalizes that higher security-risk signals reduce the utility gap and thus demobilization propensity. A negative composite signal therefore lowers predicted demobilization.

This framework yields three ex-ante predictions mapped to our tests. Prediction 1: negative post-exit signals reduce demobilization. Prediction 2: the adverse effect is larger where economic fragility signals are stronger or more accumulated. Prediction 3: the adverse effect is larger where expected security costs are higher.

Data and Empirical Strategy

The quantitative analysis integrates anonymized ARN administrative records on demobilizations, business-unit openings, and ex-combatant deaths. The core panel is municipality-quarter and focuses on 2005-2016 for the main FARC specification. This window matches the policy process under study: the reintegration route analyzed here begins with the 2005 institutional configuration, and 2016 is the natural cutoff because the peace agreement moved FARC demobilization into a collective stage (Agencia para la Reincorporacion y la Normalizacion, 2020; Congreso de Colombia, 2005; United Nations Security Council, 2022). The archive covers all registered program participants and openings in the relevant administrative system, which means treatment episodes, outcomes, and support diagnostics are drawn from a consistent source. Because the archive is anonymized, deterministic micro-level linkage to external government datasets is not possible.

The core outcome is the number of FARC demobilizations recorded in a municipality in a given quarter. Treatment is an opening episode that starts in the first municipality-quarter with positive FARC-linked business-unit openings and continues while openings remain consecutive. Event time spans $e \in [-8, 12]$ with $e = -1$ as omitted reference. The core build contains 53,856 municipality-quarter observations, 1,122 municipalities (versus the official DANE count of 1,103, as the ARN archive retains historical DIVIPOLA codes for entities subsequently reclassified or merged), and 178 opening episodes. Treated support is stable over event time at 106 treated municipalities per event quarter in the core.

Three clarifications on the treatment definition are worth making explicit. First, the treated sample consists of municipalities where at least one FARC-linked business-unit opening occurred; municipalities where participants enrolled in the program but produced no opening remain in the comparison pool. The design therefore estimates the effect of a visible opening episode, not the effect of program enrollment. Second, the timing variation used for identification is the calendar quarter of the first opening in a municipality, which is shaped by administrative milestone sequencing rather than by post-opening business performance. Whether a business ultimately succeeds or closes does not determine when the episode starts; it determines what signal is subsequently observable to peers. Third, both the treatment variable and the outcome are constructed exclusively from FARC-linked records in the ARN archive. AUC and other paramilitary ex-combatant openings — while present in the same administrative system — are excluded from the treatment variable, and the outcome tracks FARC demobilizations specifically. The design is therefore internally consistent within the FARC reintegration pathway; cross-group contamination from paramilitary openings affecting FARC demobilization decisions cannot be ruled out entirely but is unlikely to be the primary driver of the estimated effects given the organizational and territorial separation between these groups in the 2005–2016 period.

This data structure is uncommon in the demobilization literature because it combines full-route administrative depth with repeated episode tracking in a long panel. Treatment episodes, outcomes, risk context, and support diagnostics are all derived from the same institutional archive, which avoids compositional mismatch across data systems. Anonymization blocks deterministic linkage to outside microdata, which limits covariate expansion.

Focus groups and qualitative protocol

To complement the administrative panel, we conducted two semi-structured focus groups on October 15, 2022 at ARN offices in Medellin: one with demobilized ex-combatants and one with

Table 2: Summary statistics for variables used in the core and heterogeneity analysis.

Variable description	Unit of analysis	Mean	SD	Min	Max	N
Number of FARC demobilizations	Municipality-quarter	0.269	2.342	0	128	53,856
Number of business units opened	Municipality-quarter	0.034	0.471	0	25	53,856
Number of ex-combatant deaths	Municipality-quarter	0.017	0.172	0	7	53,856
Neighboring municipality had opening (indicator)	Municipality-quarter	0.090	0.287	0	1	53,856
High post-exit risk municipality (indicator)	Municipality-quarter	0.227	0.419	0	1	53,856
Municipal population (thousands)	Municipality-year merged to quarter	40.827	251.688	0.225	7,980.001	53,856
Business openings in first episode quarter	Opening episode	2.157	2.678	1	22	178
Episode length (consecutive quarters)	Opening episode	1.461	1.328	1	10	178
Mean demobilizations in previous 8 quarters	Opening episode	6.053	12.854	0	98	178
Local spillover before episode (indicator)	Opening episode	0.247	0.433	0	1	178
Episode start year	Opening episode	2010.787	1.934	2007	2013	178

ARN professionals involved in reintegration implementation. Both sessions were moderated by the research team and started with an informed-consent script. The script stated that participation was voluntary, audio would be recorded, responses would be confidential, participants could skip questions, and withdrawal was possible without penalty.

The ex-combatant session was organized around participants with direct experience in the productive-project route (new ventures and strengthening cases). All ex-combatant participants had demobilized individually and were enrolled in the individual Reintegration route (the pre-2016 ACR/ARN pathway), not the post-2016 collective Reincorporation process for FARC-EP peace accord signatories; this distinction matters because the two programmes differ in legal framework, benefit structure, and timing. Individual demobilization years are not recorded in the session transcripts, but enrollment in the pre-2016 pathway implies participants demobilized before 2016, placing them within the 2005–2016 study window. Although both sessions were conducted at ARN offices in Medellín, each demobilized participant is assigned to the ARN office closest to their reintegration municipality, so the Medellín office serves a catchment that extends across multiple municipalities in Antioquia. Participants' reintegration trajectories therefore reflect a range of municipal contexts, not only the Medellín metropolitan area, which reduces the geographic scope concern. The ARN session included staff with operational roles in eligibility screening, route follow-up, and productive-project support. At the start of each session, participants introduced themselves and their role or business activity, which was then used only to guide discussion flow, not for identification in the manuscript.

The discussion guide covered four common modules across both groups. The first module documented the operational sequence from reintegration entry to project implementation, including eligibility filters and route milestones. The second module focused on timing and disbursement mechanics, including whether participants can predict when support becomes operational. The third module covered post-opening performance and support constraints, including reasons why some units remain active and others close. The fourth module addressed information and security channels: how reintegration outcomes become visible in local networks, whether those signals shape demobilization expectations, and how intimidation risk affects behavior.

Audio files were transcribed, editorially cleaned, and converted to line-indexed text. When duplicate recordings existed for the same session, transcripts were unified into a single quality-controlled version before coding. We then applied a predeclared hybrid codebook and linked coded excerpts to an evidence bank and mechanism matrix in the qualitative workspace. In this manuscript, qualitative claims are tied to traceable evidence IDs and used to interpret mechanisms consistent with the quantitative effects rather than to claim separate channel-level causal identification (Kaplan & Nussio, 2018; Nussio, 2018; Oppenheim & Söderström, 2018).

Estimation framework

The core event-study equation is:

$$Y_{i,s,e} = \sum_{k\in[-8,12],k\neq-1} \beta_k \left(\mathbb{1}\{e=k\} \times Treat_{i,s}\right) + \alpha_{i,s} + \lambda_{e,s} + \varepsilon_{i,s,e},$$

where $Y_{i,s,e}$ is demobilization count in municipality i, stack s, and event time e; $\alpha_{i,s}$ are unit-by-stack fixed effects; $\lambda_{e,s}$ are time-by-stack fixed effects; and standard errors are clustered at municipality level.

Stacked DID is our primary estimator because repeated treatment timing and heterogeneous dynamic effects can bias or complicate interpretation in standard TWFE decompositions (Borusyak et al., 2024; Chaisemartin & D'Haultf, 2020; Goodman-Bacon, 2021; Roth et al., 2023; Sun & Abraham, 2021).

Estimator choice is therefore substantive. With repeated episodes, TWFE event studies can mix comparisons against already-treated units and assign implicit weights that are hard to interpret when treatment effects vary over cohorts and horizons. The stacked design keeps an episode-centered comparison set and a direct event-time interpretation aligned with this paper's question, following the approach developed in Cengiz et al. (2019), formalized for heterogeneous treatment effects in Callaway & Sant'Anna (2021), and extended as a general estimator in Wing et al. (2024).

Parallel trends are defended in three complementary ways. Institutional timing uncertainty, documented in the background, motivates as-if-random event timing conditional on controls. Graphical evidence shows pre-coefficients back to $e = -8$. Formal tests report a non-rejection in the core quarterly design ($p = 0.36$). Joint pre-trend tests can reject gross violations; failure to reject is not proof of validity. The design therefore combines tests with diagnostics and institutional evidence. We also keep inference conservative in subgroups where pre-trend tests are weaker, especially the high-spillover split that rejects at conventional levels. Those subgroup estimates are treated as suggestive context, not as decisive mechanism evidence.

Support diagnostics are reported directly because dynamic DID estimates are only meaningful where support is adequate. In the quarterly core and listed heterogeneity splits, observed support equals expected support at 20 of 20 event-time points. This is one reason the quarterly design is preferred in the main text, while monthly versions are left to appendix robustness.

Sensitive testimony is handled conservatively in the main text: one direct anonymized quote is used for security exposure, with additional corroborating testimony paraphrased to reduce disclosure risk. Coding follows a consistency workflow intended to minimize selective quotation. Evidence items are first coded by mechanism category, then cross-checked against quantitative findings in the triangulation matrix, and only then elevated to manuscript evidence IDs. This sequencing reduces the risk that excerpts are chosen post hoc to fit a preferred story and keeps the boundary between descriptive testimony and mechanism-consistent interpretation explicit.

One identification concern merits explicit attention. The stacked design uses variation in *when* an opening episode begins, conditional on the municipality eventually receiving one. The institutional milestone sequencing described in Section provides the as-if-random timing argument: administrative delays in eligibility screening, capital disbursement, and business registration mean that the exact quarter in which the first opening materializes is not predictable by potential demobilizers. This timing variation is the core of the identification strategy. What it does not resolve is the question of *which* municipalities ever receive an opening. Treated municipalities—those with at least one FARC-linked business-unit opening between 2005 and 2016—may differ from never-treated control municipalities in ways that correlate with the demobilization outcome, independent of episode timing.

Appendix Table A1 compares the 286 ever-treated and 836 never-treated municipalities on pre-period observables (2005–Q1 to 2006–Q4). The groups differ significantly on all five measures. Treated municipalities are on average 6.6 times larger by population (104k vs. 16k, $p = 0.001$), have quarterly demobilization rates 22 times higher (0.87 vs. 0.04, $p = 0.002$), and are far more likely to have had any pre-period demobilization activity (69% vs. 17%, $p < 0.001$). These differences are programmatically expected: the ARN program directed initial openings to municipalities with active FARC presence and larger ex-combatant caseloads. They do not, however, permit dismissal of the concern. If municipalities with higher prior FARC activity would have experienced declining demobilization regardless of program openings—due to depletion, local conflict dynamics, or other factors—the stacked DID could capture that trend rather than a program effect.

Appendix Table A2 addresses this directly through propensity-score trimming. A probit model predicts ever-treatment from log population, pre-period mean quarterly demobilizations, and pre-period ex-combatant death rates. Population and prior demobilizations drive selection into treatment ($p < 0.001$ for both); death rates add little additional predictive power once those two factors are controlled. Appendix Figure A6 shows the propensity-score distributions for both groups. Following the overlap criterion in Crump et al. (2009), the control pool is restricted to municipalities with propensity scores in the common support window $[0.109, 0.426]$, dropping 285 of 836 control municipalities (34.1%) while retaining all 178 treated episodes. The windowed ATT estimates under this restriction are $\text{ATT}[0, 4] = -0.123$ and $\text{ATT}[0, 6] = -0.246$, compared with -0.123 and -0.247 in the baseline. Standard errors are unchanged and the pre-trend joint test remains non-rejecting ($p = 0.37$ vs. 0.36). The near-exact numerical stability confirms that the main finding does not rest on extrapolation to structurally dissimilar control units.

Empirical Findings

The findings begin with the core pattern and then build outward. The first question is whether business openings are followed by more or fewer FARC demobilizations in the same municipality. The next is whether that pattern survives the basic checks that matter for a dynamic design: flat pre-periods, stable support, plausible magnitudes, and robustness to alternative samples and windows. Only after establishing the sign do we use heterogeneity to preview the mechanisms developed in the next section.

This sequence follows the conceptual framework without asking the mechanism evidence to do too much too soon. Figure 2 tests Prediction 1 directly: if openings transmit an adverse post-exit signal, post-opening coefficients should move below the pre-period baseline. The mechanism-specific evidence is developed only after this main sign is clear.

Figure 2 is the empirical center of the paper. It plots stacked DID event-time coefficients for the 2005–2016 FARC quarterly core, with $e = -1$ omitted and 95% confidence intervals shown.

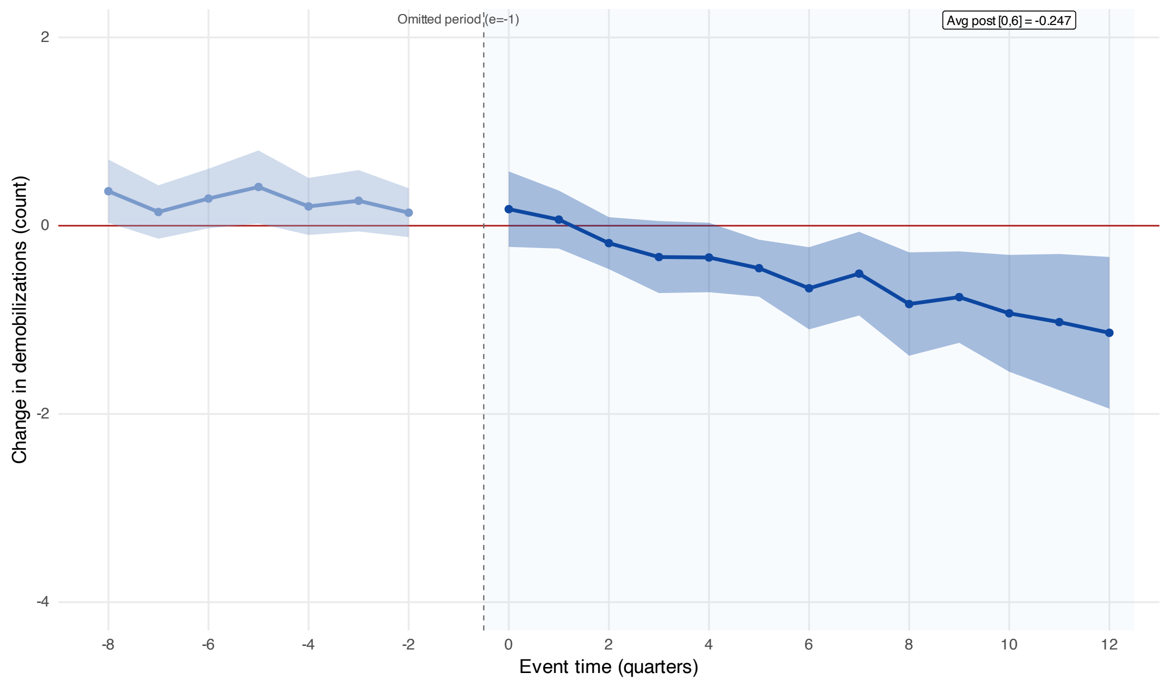

Figure 2: Quarterly stacked DID dynamic effects of opening episodes on municipal FARC demobilization counts, 2005–2016 core window. Pre-trend joint test for leads (e=-8) to (e=-2): (p=0.36). Shaded bands are 95% confidence intervals.

The pre-period is flat. Leads from $e = -8$ to $e = -2$ are close to zero and jointly non-rejecting ($p = 0.36$). After the opening, the coefficients move below zero and remain negative through later horizons. The pattern is therefore not a positive snowball. It is more consistent with an adverse signal that persists rather than dissipating immediately.

We use quarterly bins in the main text because they fit the policy process and reduce noise in a sparse-count outcome. From the participant perspective, the sequence from approval to a visible business opening is rarely monthly-precise. Quarters better match the administrative rhythm documented in ARN materials and interviews. The monthly versions in the appendix are noisier, as expected, but they do not reverse the direction of the result (Figure A5).

The quarterly build also has stable support. Treated municipality and episode counts remain constant across event time, so the late-horizon path is not being driven by support collapsing mechanically at the end of the window. The appendix then asks whether the sign is fragile to reasonable choices about horizon aggregation and municipality size (Figures A1–A3). These checks are not meant to be dispositive one by one. Their purpose is to show whether the same directional pattern survives plausible alternatives while making clear where uncertainty remains wide.

The size of the effect is easiest to read across three post-opening windows. Average effects are (-0.123) for quarters 0–4, (-0.247) for 0–6, and (-0.533) for 0–12. With average treated support of

106 municipalities, these imply about 65, 184, and 734 fewer demobilizations across the respective windows. Normalized per episode, using the 178 opening episodes in the core sample, the $e = 0$–6 estimate implies roughly one fewer demobilization per opening episode over the seven post-opening quarters. That is small in absolute terms, but it is meaningful in context: the typical treated municipality averages only 6 FARC demobilizations per quarter in the pre-period. Relative to total observed FARC demobilizations in 2005–2016 (14,505), the implied shares are approximately 0.45 percent, 1.27 percent, and 5.06 percent. Relative to a treated-sample baseline-flow counterfactual from pre-periods $e = -8$ to $e = -2$, the corresponding shares are about 2.0 percent, 4.0 percent, and 8.7 percent (Table A5).

Table 3 places the core estimate beside a depletion-control check. Column (1) is the baseline stacked DID. Column (2) adds the lagged cumulative FARC demobilization rate in the municipality, scaled by population. The control addresses a simple mechanical concern: each additional demobilization leaves fewer FARC members available to demobilize later. If municipalities with heavier earlier outflows were mechanically headed toward lower subsequent demobilization counts, the baseline estimate could partly reflect depletion rather than the opening itself. The estimate changes little after adding this control, and the pre-trend p-value remains well above conventional thresholds.

Table 3: Main results: stacked difference-in-differences estimates. Effect of FARC business-unit openings on subsequent demobilization counts, municipality-quarter panel 2005–2016.

	(1) Baseline	(2) Depletion controls
Avg. ATT $[e = 0, \ldots, 4]$	-0.123	-0.102
	(0.139)	(0.139)
Avg. ATT $[e = 0, \ldots, 6]$	-0.247*	-0.222
	(0.142)	(0.142)
Avg. ATT $[e = 0, \ldots, 12]$	-0.533***	-0.495**
	(0.205)	(0.205)
Pre-trend p-value	0.365	0.453
Observations	3,600,681	3,600,681
Episodes (treated units)	178	178
Municipality × episode FE	Yes	Yes
Quarter × episode FE	Yes	Yes
Cluster	Muni	Muni

Notes: Stacked DiD estimates for the 2005–2016 municipality-quarter panel. Entries are unweighted averages of post-treatment event-time coefficients over the stated window; standard errors are in parentheses. Pre-trend p-value reports the joint test of leads $e = -8, \ldots, -2$. Column (2) adds the lagged cumulative FARC demobilization rate per 100,000 residents, which captures depletion pressure: each additional demobilization leaves fewer FARC members available to demobilize in later periods. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The broader evidence map is cautious but consistent. The adverse sign appears in both early and later cohorts, which weakens a pure cohort-regime explanation. The business-survival split points in the same direction: shorter-lived businesses are followed by larger declines than longer-lived businesses. High-prehistory and persistent-episode splits add supporting evidence, while one spillover-context split is treated cautiously because its high-spillover pre-trend test rejects.

Spillovers are handled with the same restraint. In the spillover-context split (Figure 3), high-spillover municipalities show a more negative path than low-spillover municipalities. Because the high-spillover pre-trend test rejects, this pattern is suggestive rather than definitive. Quantitatively, the indirect-neighbor 0–6 average is (-0.044), while the own-treatment estimate with contemporaneous neighbor exposure controls is (-0.248), nearly identical to the core own effect of (-0.247). Together these imply a larger additive deterrence envelope ((-0.291), Table A6). Estimations that account for local spillovers report a 0–12 average of (-0.147), implying about 960 fewer demobilizations (6.62% of 14,505), as shown in Figure A2.

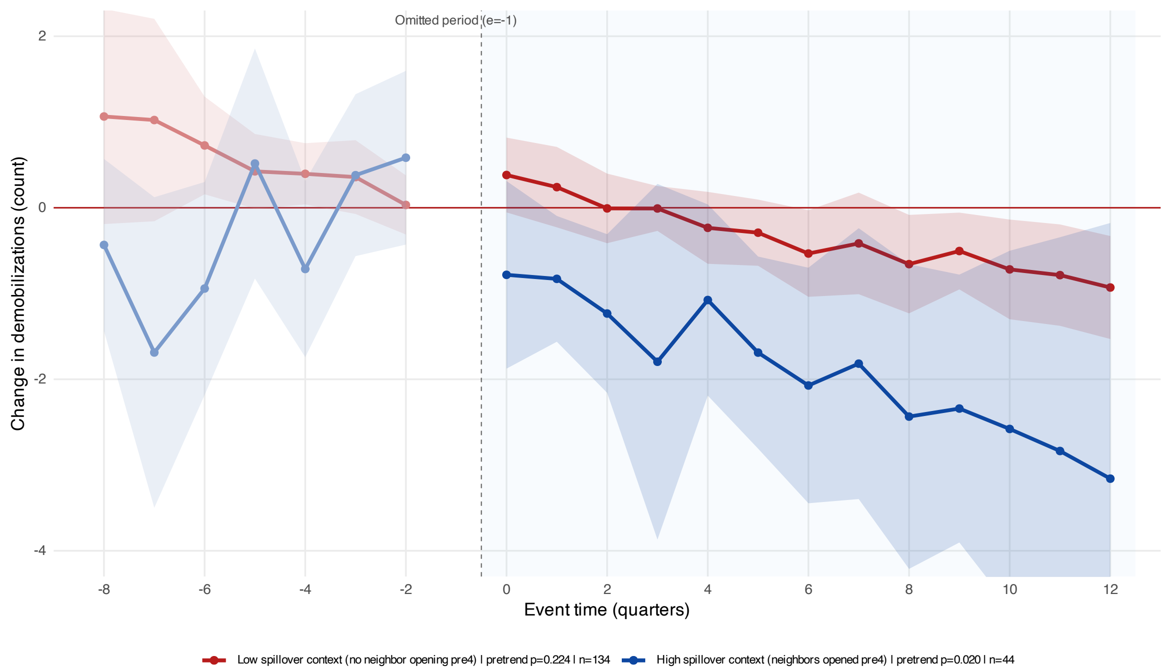

Figure 3: Quarterly stacked DID by spillover context. Pre-trend joint-test p-values: low spillover (p=0.2243), high spillover (p=0.0199). Shaded bands are 95% confidence intervals.

A final check addresses the possibility that the decline reflects direct retaliation rather than a broader post-exit signal. Figure 4 repeats the stacked DID design with ex-combatant deaths per municipality-quarter as the outcome. The average ATT over $e = 0$–6 is -0.024 (95% CI: $[-0.082, 0.035]$), indistinguishable from zero, and the pre-trends are clean ($p = 0.109$). Deaths do not rise systematically after opening episodes.

This result narrows the interpretation. It rules out retaliation-driven mortality as the primary reason demobilizations fall after openings. It does not rule out perceived security risk. Armed organizations can rely on surveillance, warnings, and low-level coercion without producing a concurrent increase in killings (Shapiro, 2013). The deaths result therefore fits an information-based mechanism: openings can transmit signals about post-exit trajectories and risk without requiring a visible increase in lethal violence.

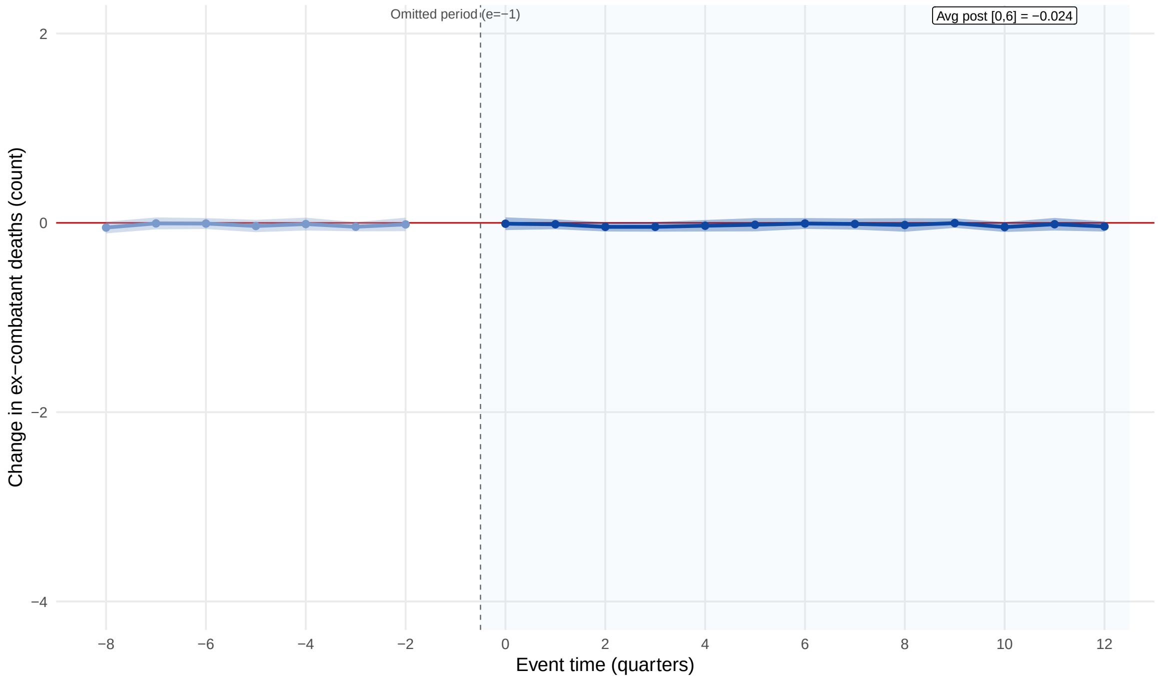

Figure 4: Stacked DiD dynamic effects of opening episodes on ex-combatant death counts per municipality-quarter, 2005–2016. Average ATT over $e = 0$–6: -0.024 (95% CI: $[-0.082, 0.035]$). Pre-trend joint test $p = 0.109$. Shaded bands are 95% confidence intervals.

The remaining diagnostics reinforce the same reading rather than replacing it. The appendix places the core estimate alongside multiple heterogeneity and specification checks. Confidence intervals widen in some subgroups, but the central tendency is not suggestive of a strong positive snowball in any main split that satisfies pre-trend diagnostics. Monthly estimates are noisier because demobilization counts are sparse and the opening process is not monthly-precise, but the directional finding remains stable across these constructions (Figure A5).

Rambachan & Roth (2023) honest-inference bounds complement the parallel-trends checks. The baseline design already combines institutional timing uncertainty, flat event-time leads, a non-rejecting core pre-trend joint test ($p = 0.36$), and stable support across event time. The sensitivity exercise asks how much post-treatment trend deviation would be needed to overturn the sign. Under no trend violation (`Mbar=0`), both medium-term (0–6) and long-term (0–12) effects are clearly negative: robust 95% intervals are ([-0.510,-0.073]) and ([-0.737,-0.316]). Even with a non-trivial deviation (`Mbar=0.10`), the long-term 0–12 interval remains strictly negative ([-1.159,-0.105]), while the 0–6 central estimate remains negative but its conservative interval reaches zero. Full sensitivity results are reported in Table A7 and Table A8.

Taken together, the empirical findings support a narrow but strong claim. The paper does not estimate a structural model of demobilization behavior, and it does not claim that every municipality experiences the same adverse effect. It shows that, in the core population and period, business openings are not followed by a positive average snowball in FARC demobilizations. The post-opening path is instead more consistent with deterrence than encouragement.

That conclusion motivates the mechanisms section. Participants and ARN staff repeatedly describe peer observation and word-of-mouth around program trajectories (ARN013, ARN014, EX013, EX014), which is why the next section turns to survival and risk.

Mechanisms

The mechanism evidence is organized in two complementary blocks that map directly to the conceptual predictions. The first concerns Prediction 2: larger adverse effects where economic fragility signals are stronger or more accumulated. The second concerns Prediction 3: larger adverse effects where expected security costs are higher. The two channels are not treated as mutually exclusive; they can reinforce each other. Critically, the focus-group evidence does not merely support the behavioral plausibility of these channels—it directly documents them: ex-combatants and ARN staff describe, in their own words, the expectation gaps, capital shortfalls, and intimidation pressures that the quantitative heterogeneity patterns identify. That convergence across independent data sources—administrative records, event-study heterogeneity, and coded qualitative testimony—is the principal basis for the mechanism interpretation. Related Colombian work also shows that attitudes toward former combatants' reintegration respond to conflict experience and conciliatory public signals: García-Sánchez and Plata-Caviedes (2020) study support for FARC political involvement, while Agneman et al. (2025) show that public-facing apologies can improve reintegration attitudes. These findings are not direct tests of demobilization behavior, but they reinforce the interpretation of openings as socially meaningful signals rather than private program milestones.

Two information transmission channels can carry opening signals to potential defectors. The direct social network channel operates through personal ties between active fighters and former members, but its reach is constrained by the geographic fragmentation typical of the Colombian conflict: a fighter active in Dabeiba may demobilize and settle in Medellín, with warzone peers dispersed across multiple regions. Cárdenas et al. (2024) show such ties can transmit behavioral signals among ex-combatants, but their reach to active fighters in different municipalities depends on individual network configurations the design cannot measure. The public visibility channel is less sensitive to geographic dispersion: a business opening creates a physical presence observable by anyone in the local community without requiring direct personal contact. We treat public visibility as the primary mechanism, which is why the sector heterogeneity below — strongest for commerce and services, the most physically visible types — is the most direct evidence on the channel.[1]

Fragility and expectation gaps

The first mechanism starts from a simple distinction. Opening a business is not the same as making that business last. If potential defectors see former combatants receive support but then watch the business struggle, the opening can become a discouraging signal even though the state did formally deliver the benefit.

The clearest way to test that idea is to look at business survival directly. The ARN follow-up records allow us to do this because they record whether supported business units were still operating when staff returned to observe them. The limitation is timing. The follow-up system arrives late in our 2005–2016 window: in the FARC-linked records used here, inferred opening years begin in

[1]Both channels can carry information about fragility or risk. The qualitative material documents both as active, with visibility-based signals reaching a wider local audience and network-based signals carrying more contextual detail where ties exist (ARN013, ARN014, EX013, EX014). The opening stage is also the natural threshold: proposal and acceptance occur within internal administrative processes not observable to peers, whereas a business opening is the first moment a participant's post-exit trajectory becomes physically present in the local environment.

2011 and are concentrated after 2014. That leaves too little support to draw the same event-study plot we use elsewhere in the paper. The observed archive contains 18,666 business units overall, but only 222 match to FARC opening cells in our design. Once we keep the event window with usable support, the direct comparison is small: 6 short-lived opening episodes and 26 longer-lived opening episodes.

Table 4 reports the simple ATT comparison from those observed follow-up records. We read businesses with 0–1 operating follow-ups as unsuccessful, and businesses with 3–4 operating follow-ups as successful. The estimates are necessarily imprecise, but the ordering is informative. By the sixth and eighth post-opening quarters, the unsuccessful group is followed by a larger decline in FARC demobilizations than the successful group.

Table 4: Observed business survival and subsequent FARC demobilization.

Observed survival group	Episodes	ATT $[0, 4]$	ATT $[0, 6]$	ATT $[0, 8]$	Pretrend p
Unsuccessful (0–1 operating follow-ups)	6	-0.032	-0.424	-0.511	0.115
Successful (3–4 operating follow-ups)	26	-0.202	-0.247	-0.284	0.263

Note: The table uses the observed ARN follow-up records. The post horizon is eight quarters because it is the longest observed window with usable support in both survival groups.

The small observed comparison gives the mechanism its most direct anchor, but it cannot carry the full section on its own. To see what the same relationship would look like over the full study period, we impute business survival from observables in the follow-up data. The imputation uses observed survival patterns for similar openings, beginning with municipality-sector cells when available and then moving to broader sector-by-risk, sector, and overall cells. Three things this imputation can and cannot establish are worth stating explicitly. First, the imputation assigns predicted survival status based on observable cell characteristics, not on demobilization outcomes, so it does not mechanically produce the pattern we find. Second, the imputed classification inherits measurement error from the underlying ISUN follow-up records, which have incomplete coverage particularly for units that closed early or in high-insecurity contexts; if anything, this biases the survival gradient toward zero. Third, the imputation cannot recover the counterfactual trajectory of any individual business unit, so the survival split should be read as a signal-strength exercise rather than a structural decomposition. Within those limits, it extends the observed pattern to a sample large enough to estimate event-study dynamics, and the direction it produces is consistent with the smaller observed comparison in Table 4.

Figure 5 shows the resulting event study. The same pattern becomes clearer in the larger imputed sample. Openings imputed to the unsuccessful group generate a more negative post-opening path than openings imputed to the successful group; over quarters 0–12, the average effect is -0.817 for the unsuccessful group and -0.187 for the successful group. This is the expectation-gap mechanism in its most direct form: visible failure appears more discouraging than visible continuity.

Because the central survival evidence requires this imputation step, the remaining fragility evidence is best read as supporting triangulation. Figure 6 asks whether openings that remain visible across consecutive quarters carry a stronger signal than one-quarter episodes. Persistent episodes are not identical to successful businesses, but they expose the local environment to the opening for longer. The estimated path is consistent with stronger deterrent updating in those more sustained contexts, although uncertainty widens in some cuts.

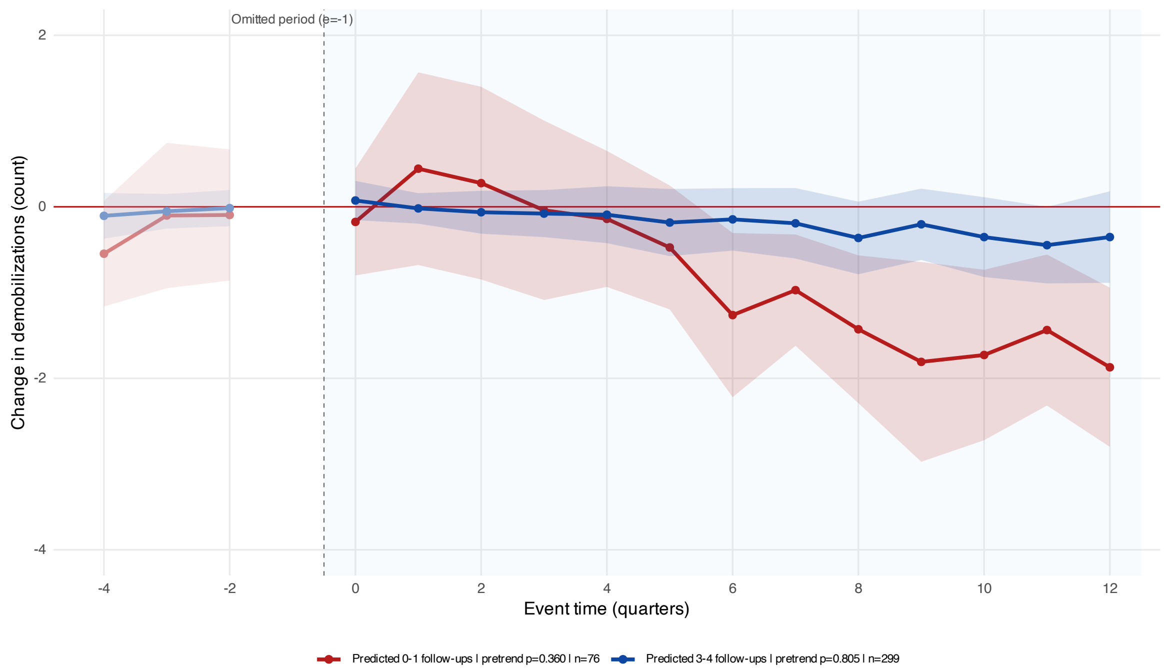

Figure 5: Imputed business-survival split in the stacked DID. Short-lived openings are those imputed to 0–1 operating follow-ups; longer-lived openings are those imputed to 3–4 operating follow-ups. Shaded bands are 95% confidence intervals.

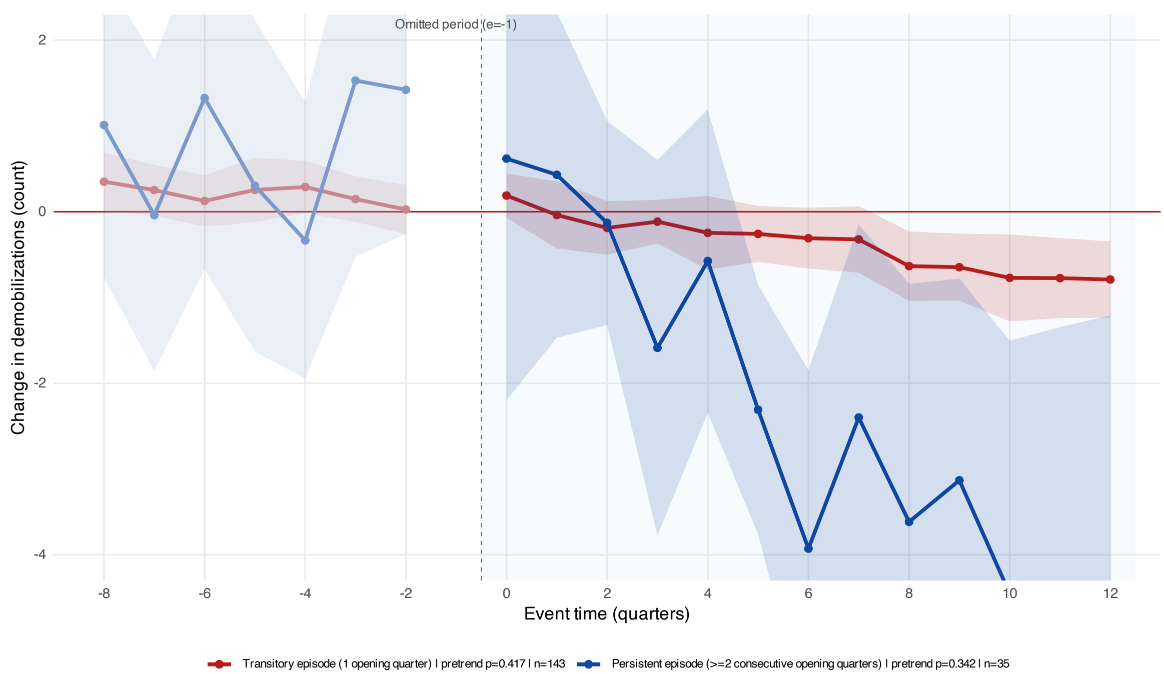

Figure 6: Quarterly stacked DID heterogeneity by episode persistence. pre-trend joint-test p-values: transitory (p=0.4174), persistent (p=0.3422). Shaded bands are 95% confidence intervals.

The prehistory split pushes the same logic one step further. Municipalities with deeper prior demobilization exposure have had more opportunities to observe how post-exit trajectories unfold. In Figure 7, high-prehistory settings show larger adverse averages than low-prehistory settings, consistent with the idea that accumulated local memory makes fragile openings more informative.

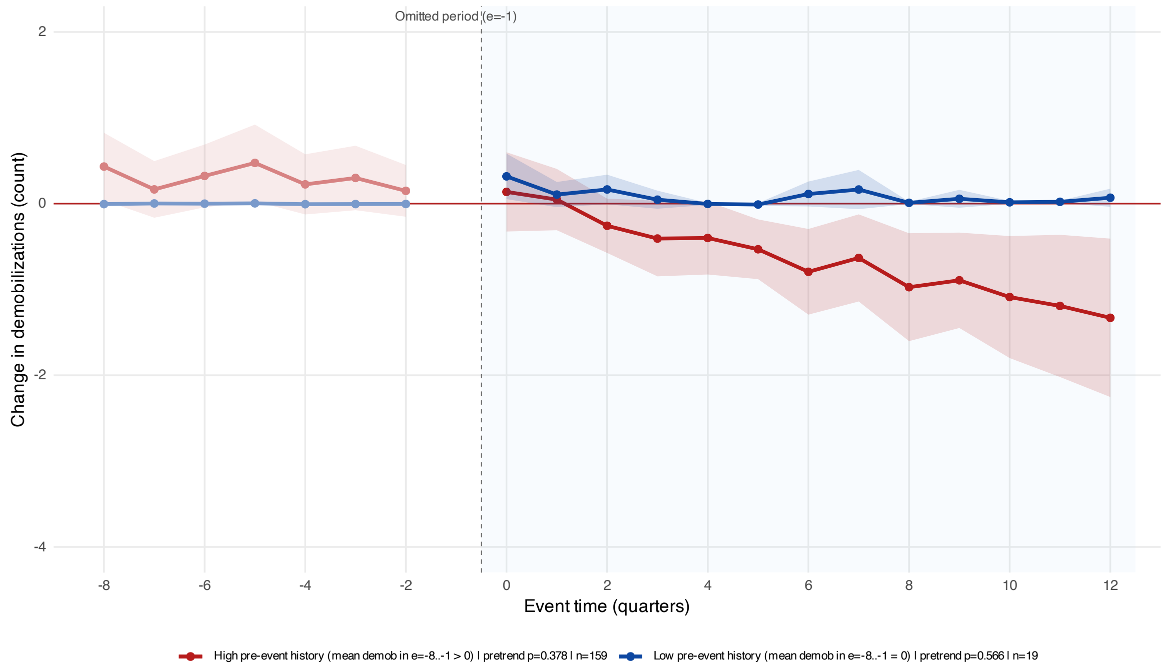

Figure 7: Quarterly stacked DID heterogeneity by pre-event demobilization history. pre-trend joint-test p-values: high prehistory (p=0.3783), low prehistory (p=0.5655). Shaded bands are 95% confidence intervals.

The qualitative record helps explain why the survival gradient is plausible. Staff and participants describe long, individualized pathways and uncertain disbursement timing (ARN004, ARN008, EX007, EX009, EX010). Participants also report that the eight-million-COP amount can be insufficient for some starts and working-capital needs (EX012), while staff emphasize that not all profiles are well suited to entrepreneurship and that employability pathways may fit part of the population better (ARN016, ARN017). The picture is not that the promise is absent. It is that the promise can arrive in a form too fragile to change expectations favorably.

ISUN context points in the same direction. In the last-follow-up business cut, 43.9 percent of units are non-functional, and non-functioning is higher in high-risk municipalities than in low-risk municipalities (45.1 vs. 39.6 percent) (QD001, QD002).[2] These figures are not a causal estimate in our design, but they describe the same world that the direct survival exercise is trying to measure.

Process testimony reinforces this interpretation. Participants repeatedly describe uncertainty between formal approval and operational materialization, and they describe mismatches between expected and realized economic traction once units start (Q001, Q005, Q007, Q011). That mismatch is exactly the type of information that can travel through peer networks and discourage additional

[2]The 43.9 percent figure is computed over business units with at least one ISUN follow-up record. Units with no follow-up entry are excluded from the denominator. If ISUN coverage is systematically lower for units that closed early or in high-insecurity contexts, the true non-functionality rate would be higher than reported here.

exits.

This interpretation is also consistent with prior findings that implementation quality can shape participant perceptions independently of headline program goals (Oppenheim & Söderström, 2018). In our setting, the relevant audience is broader than participants themselves. It includes active members of armed groups observing those trajectories indirectly, often through trusted interpersonal channels rather than formal program communication.

Violence and intimidation risk

The second mechanism asks whether post-exit threat exposure makes the same opening look more dangerous. If opening a business makes a demobilized person easier to locate in a high-risk municipality, potential defectors may update toward higher security costs even without observing a new killing after the opening. In this sense, the relevant signal is not only realized violence. It is the local salience of post-exit risk.

Figure 8 brings the direct evidence to the front. The left panel splits municipalities above and below the median FARC ex-combatant death rate in the ARN archive. The right panel uses a stricter check, comparing the bottom 20 percent of municipalities in that distribution with the remaining 80 percent. These are two ways of asking the same question: is the adverse demobilization response stronger where post-exit violence risk is more visible? The median split is the cleaner comparison, and the lower-tail split is more descriptive because the very-low-risk group is small and noisier. Read together, however, they are reassuring. The result does not hinge on a convenient threshold: places with more salient ex-combatant death histories are the places where openings are followed by the larger decline in demobilizations.

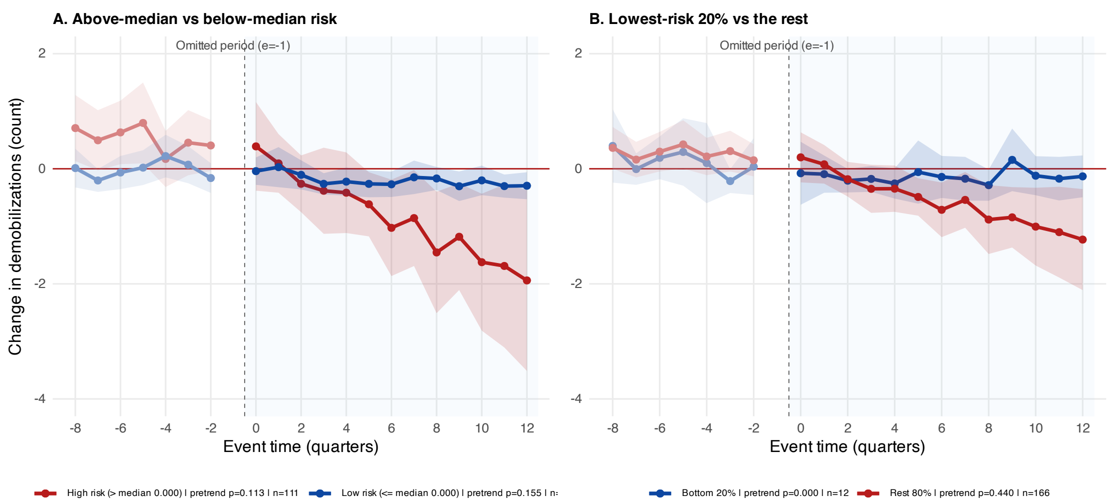

Figure 8: Violence-risk splits in the stacked DID. The left panel splits municipalities above and below the median FARC ex-combatant death rate; the right panel compares the bottom 20 percent of municipalities with the remaining 80 percent. Shaded bands are 95% confidence intervals.

This pattern fits the intimidation mechanism. Armed groups can counter reintegration signals by surveilling or threatening ex-members who become publicly visible through productive activities (Shapiro, 2013). Leaders who see business openings creating positive peer signals have incentives to make the cost of exit more salient, shifting the information available to potential defectors toward risk even when some individual trajectories succeed (Themnér, 2017). Recent Colombian evidence is consistent with this logic: Wyer (2026) documents selective targeting of demobilized FARC rebels by splinter groups in areas affected by fragmentation.

Once the risk gradient is clear, the sector split helps explain why visibility matters. Figure 9 disaggregates the core finding by business sector.[3] Commerce and services openings are the most physically visible sector types: they more often involve storefronts or service locations observable to anyone passing through the local area. They are also the sectors with the most negative estimates, ATT$[0, 6] = -0.754$ for commerce and -0.677 for services. Livestock openings are also negative (-0.451), while agriculture and manufacturing are positive but fail pre-trend diagnostics and are included only for completeness. The point is not that sector is the central test. It is that the risk mechanism is most plausible when openings are locally visible.

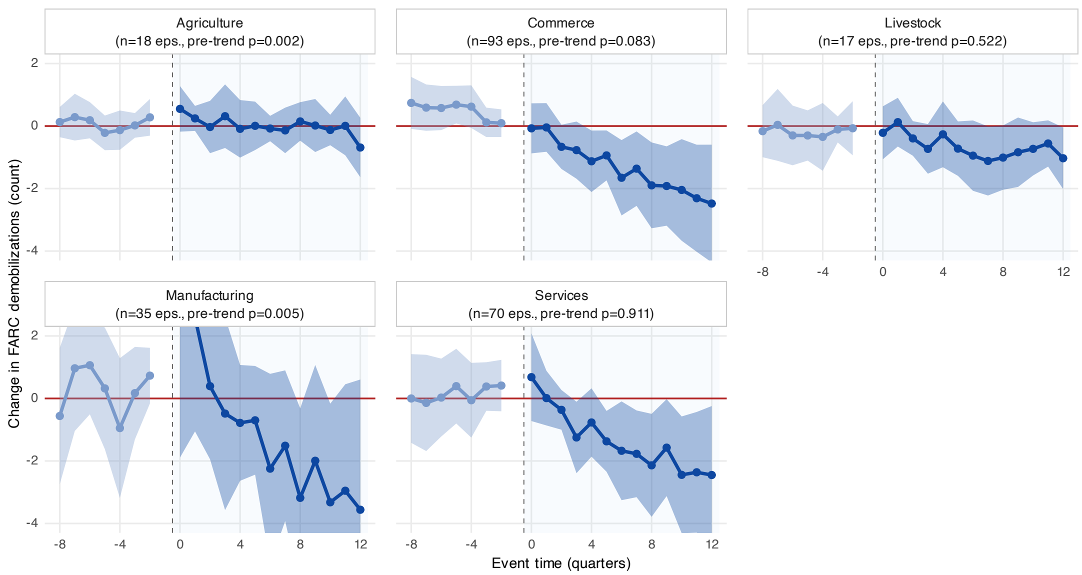

Figure 9: Stacked DID dynamic effects by business sector, outcome = FARC demobilization count per municipality-quarter, 2005–2016. Each panel shows event-time coefficients for a sector-specific treatment variable. Commerce: avg. ATT$[0, 6] = -0.754$, pre-trend $p = 0.083$. Services: -0.677, $p = 0.911$. Livestock: -0.451, $p = 0.522$. Agriculture and manufacturing show positive point estimates but fail pre-trend diagnostics and are included for completeness only. Shaded bands are 95% confidence intervals.

Cohort-stability estimates provide a final check on the mechanism story. The split is informative because the study period contains very different conflict phases. The 2005–2011 cohort spans active conflict and major military operations against the FARC; the 2012–2016 cohort spans formal peace negotiations and a different collective strategic environment. If the adverse effect were driven mainly by these aggregate conflict regimes, it should concentrate in one period. Instead, Figure 10 shows adverse estimates in both cohorts, consistent with an individual-level information channel that travels across broader political-military contexts.

[3] Sector specifications follow the business-type taxonomy in the ARN administrative archive: commerce, services, agriculture, manufacturing, and livestock. All five were estimated as a set.

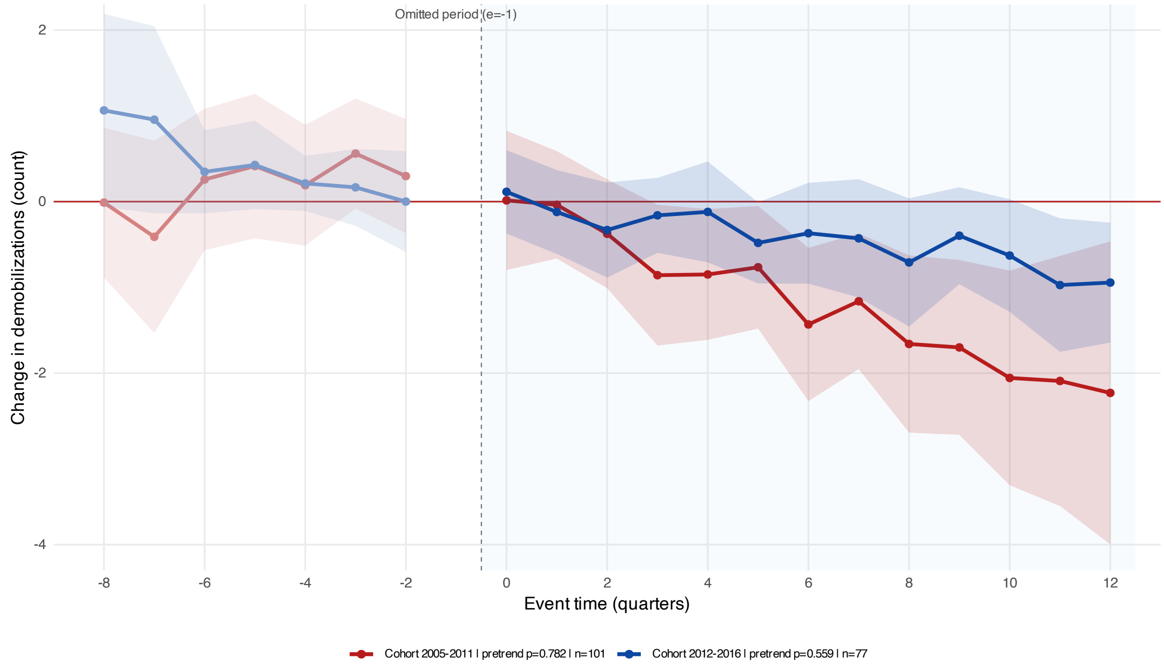

Figure 10: Quarterly stacked DID heterogeneity by cohort. pre-trend joint-test p-values: cohort 2005-2011 (p=0.7819), cohort 2012-2016 (p=0.5588). Shaded bands are 95% confidence intervals.

The security testimony is consistent with this pattern. One anonymized direct quote is included here because it captures post-demobilization intimidation in the most concrete way:

> "They found me at my stall. I told them: if you are going to do something to me, do it here; I am not leaving Medellin." (Q019)

Related evidence from Q020 and Q021 indicates that intimidation can mute peer encouragement: some ex-combatants avoid explicitly encouraging contacts still inside armed groups when threat is perceived as credible. Three additional coded entries in the evidence bank (EX020–EX022) provide corroborating accounts from the security module of the focus-group protocol, documenting ex-combatants' assessments of visibility risk near their business locations and the effect of that perceived risk on their willingness to communicate with former contacts.

The two mechanism blocks fit together. Fragility and expectation gaps lower perceived economic returns to exit. Threat exposure raises perceived security costs. A potential defector observing both may rationally delay or avoid demobilization even when formal program access exists.

This complementarity is important for interpretation. Economic fragility can make threatened participants less resilient to security shocks, while security pressure can undermine business continuity and amplify visible failure. The mechanisms are therefore presented as mutually reinforcing pathways rather than as a forced either-or choice.

A central rival explanation is composition/depletion: municipalities with earlier exits may mechanically have fewer potential future defectors. We address this directly by adding lagged cumulative demobilization pressure (per-100k) as a control and by splitting estimates by depletion pressure. The depletion-control specification shifts the 0–6 average from -0.247 to -0.222, and the 0–12 average from -0.533 to -0.495, with wide confidence intervals, while the split shows a more adverse path in low-depletion contexts and near-zero averages in high-depletion contexts (Figure A4; Table A9). We interpret this as evidence that composition/depletion can contribute to the dynamics, but does not by itself overturn the adverse pattern or replace the information and security channels emphasized above.

Channel-level causality is not separately identified: the quantitative design estimates the average treatment effect of openings on demobilization counts; heterogeneity splits and qualitative evidence help rank plausible mechanisms but cannot resolve them definitively. Several rival pathways remain plausible — unobserved local shocks correlated with opening timing, selective recall among interview participants, survivorship in who remains available for interview, and underreporting of security pressure where fear persists. Within those limits, the mechanism evidence is triangulated across source classes: the fragility channel is supported by observed and imputed survival evidence, event-study heterogeneity, ISUN failure-rate context, and interview accounts on capital adequacy and market-fit problems; the security channel by risk-group stratification, the deaths regression, and intimidation testimony from ex-combatants.

Conclusion

The paper asked whether reintegration business openings generate a positive snowball in demobilizations. In the quarterly stacked DID core for municipalities with FARC-linked opening episodes in 2005-2016, the answer is no. The average dynamic effect is negative and persistent after opening episodes. The identification case combines institutional timing logic, full support diagnostics in the event window, visible lead coefficients through two pre-treatment years, and a non-rejecting core pre-trend joint test. Robustness designs in the appendix, including alternative windows and spillover/depletion checks, do not overturn the directional conclusion.

Mechanism evidence is bounded to what the data can support. Quantitative heterogeneity plus coded qualitative evidence is consistent with two complementary channels. The first is a credibility channel driven by fragile post-opening trajectories and expectation gaps after long administrative paths. The second is a security channel in which intimidation risk weakens the attractiveness of visible reintegration.

The evidence points to three design margins that appear especially relevant. One is reducing uncertainty and opacity in disbursement and delivery timing, because timing itself can affect perceived credibility. A second is matching program modality more closely to participant profiles, so entrepreneurship is not treated as a default where employability routes may fit better. A third is strengthening protection around visible post-exit livelihoods, especially in municipalities with higher realized risk.

Appendix Table A3 quantifies these margins where the data permit. On the viability margin: 43.9% of ARN business units were no longer functional at last ISUN follow-up, approximately four years after opening. Raising the functional share to 66.1% — a +10.1 percentage-point improvement in survival — would be sufficient to drive the estimated ATT to zero, under a linear extrapolation from the low-risk and high-risk failure rates and ATTs (see Appendix Table A4) and under the assumption that the security context is simultaneously addressed. As an intermediate benchmark, matching the failure rate observed in low-risk municipalities (39.6%, vs. 45.1% in high-risk) would require a +4.3 pp improvement and is consistent with better market-fit screening and post-opening support. Sector heterogeneity reinforces the modality margin: commerce and services generate the largest adverse effects ($\text{ATT}[0, 6] = -0.754$ and -0.677, respectively), consistent with those sectors being the most visible to peer networks and thus most likely to transmit a negative signal when businesses struggle. On the security margin: the excess deterrent effect in high-risk relative to low-risk municipalities is -0.155 per municipality-quarter, while security-related closures account for only 1.5% of all business units and the deaths regression yields an ATT of -0.024 ($p = 0.109$). The near-zero realized-violence effect indicates that the security channel operates primarily through perceived exit risk rather than actual post-exit violence, which means protective measures need to reduce threat exposure, not only provide incident response after closures occur. Neither channel alone is sufficient to reverse the effect; Appendix Table A3 shows both must be addressed jointly to reach a net-neutral programme equilibrium.

These implications are operational. The evidence does not say productive support should be removed. It says its design must account for the information externality created by visible post-exit trajectories. If those trajectories are fragile or unsafe, the program can inadvertently deter future demobilization.

External validity should still be treated with care. Colombia's institutional depth, administrative scale, and long reintegration route make the signaling mechanism especially legible here: there are many observable milestones, wide social networks of ex-combatants, and a prolonged program timeline that creates repeated visibility episodes. DDR settings with shorter, less administratively structured programs —such as Sierra Leone (Humphreys Weinstein, 2007) or Burundi (Gilligan et al., 2013)— may generate weaker information signals, as fewer observable milestones compress the window for peer updating. Even so, the core policy lesson is transportable wherever exit programs have visible implementation milestones observed by peer networks: reintegration design must be evaluated as an equilibrium communication process, not only as a participant-level service package.

For practitioners, this means monitoring systems should track not only individual completion and business activation, but also trajectory credibility indicators that are likely to be observed by peers: continuity at one and two years, exposure to intimidation near business sites, and clarity of implementation timelines. Those indicators are closer to the actual information set of potential defectors and therefore more directly linked to the sign of any snowball effect. Embedding those indicators in routine evaluation would align program management with the behavioral channel this paper identifies and make adverse spillovers easier to detect early.

For future research, two extensions are especially relevant. One is to connect administrative trajectories with richer territorial market measures to better separate economic from security components of the signal. The second is to measure information transmission directly, for example, through networked survey modules around ex-combatant communities. Both would sharpen mechanism quantification.

This conclusion also has a methodological implication for program evaluation in conflict settings. When interventions are publicly visible, evaluation frameworks should include social spillovers as first-order outcomes rather than postscript discussion points. The argument parallels recent work showing that policy interventions generate information externalities that shape the behavior of non-participants: in health contexts through visible take-up (Karing, 2023), and in counterinsurgency contexts through the information environment that civilian policy creates for armed actors (Dell & Querubin, 2018). The Colombian reintegration case adds DDR programs to that list. For Colombia and other DDR settings, the broader lesson is simple: demobilization policy is not finished when someone leaves an armed group. It is judged by what others observe afterward.

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Appendix

Figures

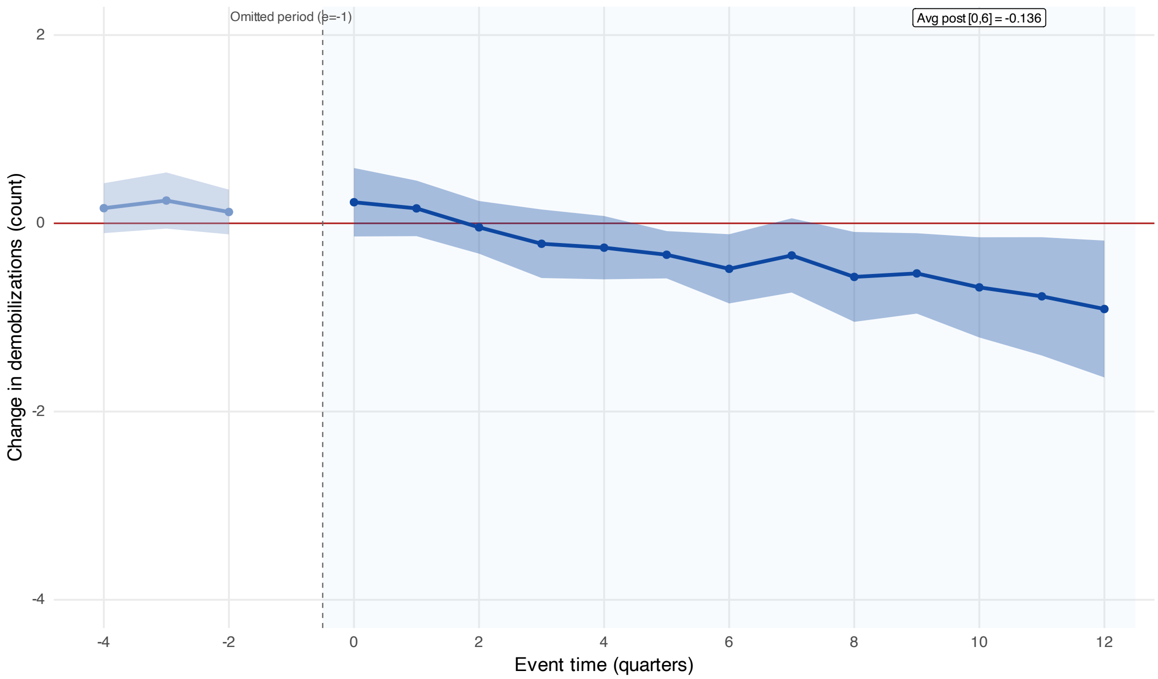

Figure A1: Quarterly event-study robustness using a shorter lead window ((e=-4) to (e=12)). pre-trend joint-test p-value (core quarterly benchmark): (p=0.36). Shaded bands are 95% confidence intervals.

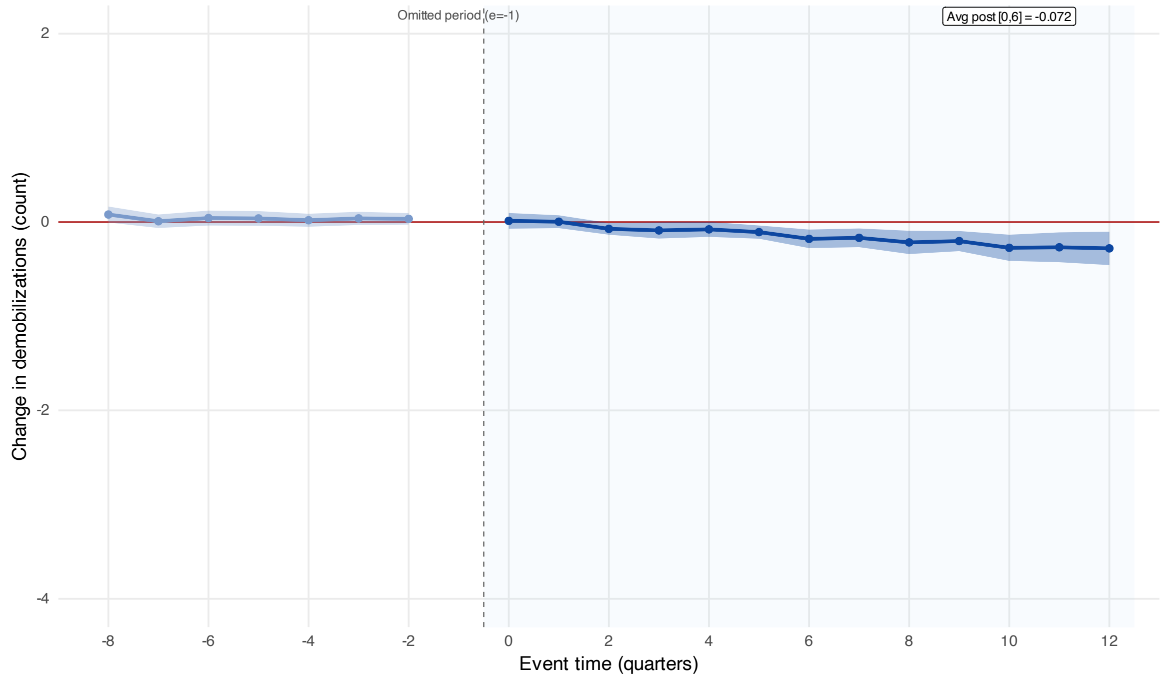

Figure A2: Quarterly stacked DID total local exposure (own OR neighbor opening). pre-trend joint-test p-value: (p=0.6413). Shaded bands are 95% confidence intervals.

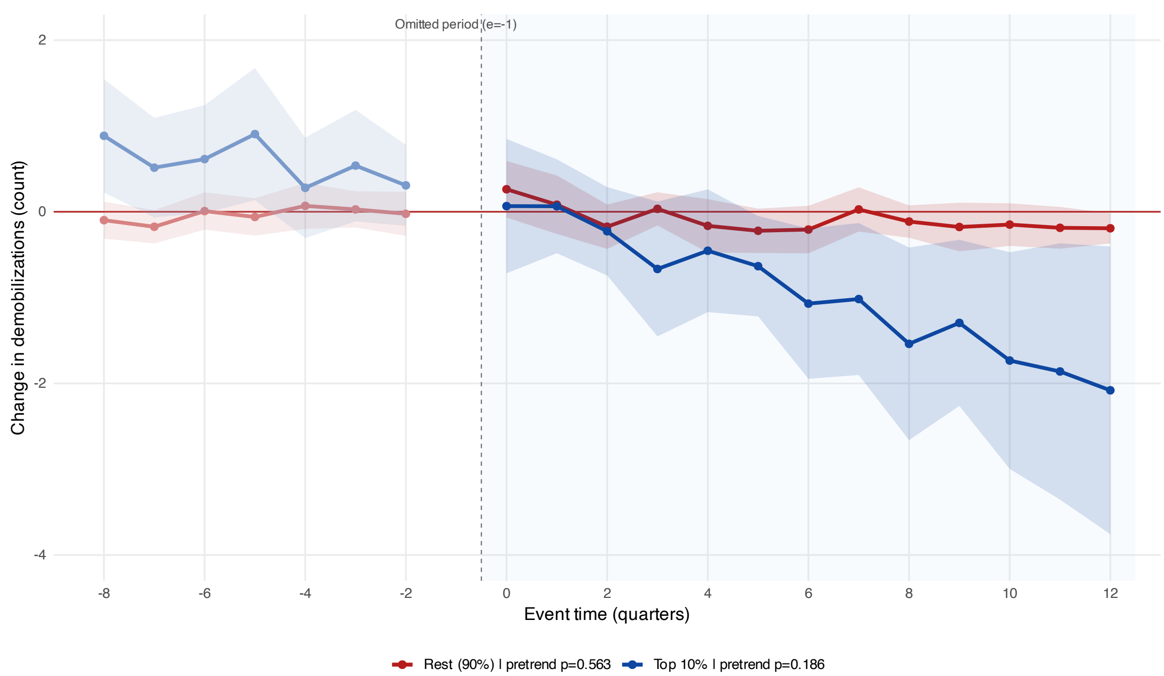

Figure A3: Quarterly stacked DID by municipality size. pre-trend joint-test p-values: top 10% (p=0.1864), rest 90% (p=0.5626). Shaded bands are 95% confidence intervals.

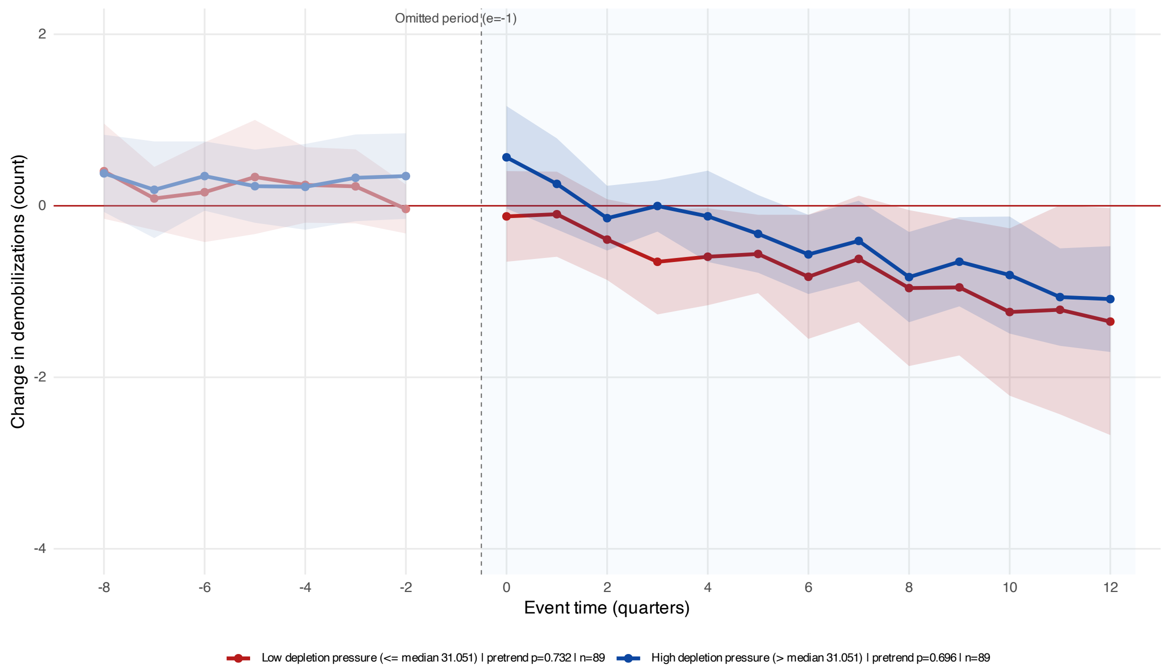

Figure A4: Quarterly stacked DID by depletion pressure (lagged cumulative demobilization split).

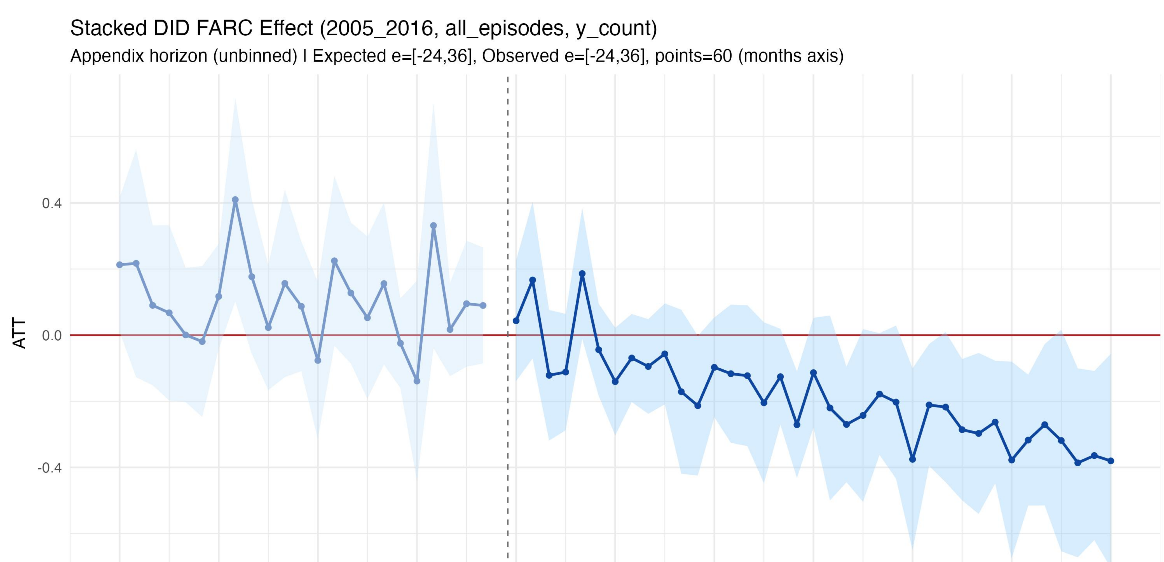

Figure A5: Monthly event-study robustness. pre-trend joint-test p-value (core quarterly benchmark): (p=0.36). Shaded bands are 95% confidence intervals.

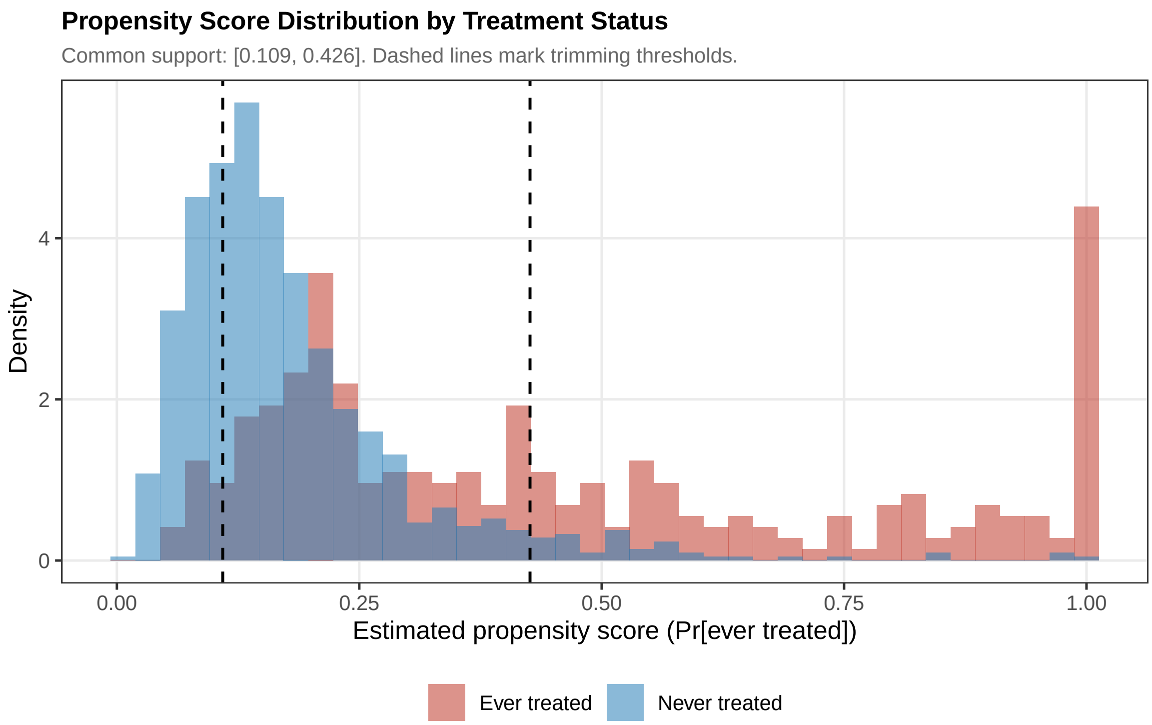

Figure A6: Propensity-score overlap between ever-treated and never-treated municipalities. Propensity scores estimated via probit with log population (2005), mean quarterly demobilizations (2005–Q1 to 2006–Q4), and mean quarterly ex-combatant deaths (2005–Q1 to 2006–Q4). Vertical dashed lines mark the common support window $[0.109, 0.426]$ (5th–95th percentile overlap, Crump et al. 2009). Control municipalities outside the window (shaded region) are excluded in the PS-trimmed specification reported in Table A2.

Tables

Table A1: Pre-period balance: never-treated vs. ever-treated municipalities.

	(1) Never treated (N = 836)	(2) Ever treated (N = 286)	(3) Difference
Population 2005 (thousands)	15.686	104.343	88.657***
	(20.518)	(454.132)	[0.001]
Mean quarterly demobilizations (2005–06)	0.039	0.872	0.833***
	(0.124)	(4.423)	[0.002]
Total demobilizations (2005–06)	0.310	6.972	6.662***
	(0.991)	(35.384)	[0.002]
Mean quarterly ex-combatant deaths (2005–06)	0.002	0.020	0.018**
	(0.022)	(0.121)	[0.014]
Any pre-period demobilization (indicator)	0.172	0.689	0.517***
	(0.378)	(0.464)	[0.000]

Notes: Pre-period defined as 2005-Q1 to 2006-Q4 (eight quarters before the earliest possible treatment event). Column (1): municipalities with no FARC-linked business-unit opening in 2005–2016. Column (2): municipalities with at least one opening. Column (3): difference (Ever minus Never treated) with Welch t-test p-value in brackets. Standard deviations in parentheses. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Table A4: ISUN fragility by risk and size.

Group type	Group	Units	Share non-functional	Share security reason
Risk	High risk	12,984	0.4512	0.0344
Risk	Low risk	5,510	0.3964	0.0321
Size	Rest 90%	6,976	0.3764	0.0351
Size	Top 10%	11,518	0.4703	0.0331

Table A5: Magnitude benchmarks with internal denominators, 2005-2016.

Post window	Avg effect	Implied fewer demobilizations	Share of total observed 2005-2016 demobilizations (N=14,505)	Share vs baseline-flow counterfactual
0-4	-0.123	65.2	0.45%	2.01%
0-6	-0.247	183.6	1.27%	4.05%
0-12	-0.533	734.1	5.06%	8.72%

Table A2: Robustness to propensity-score trimming. Stacked DiD estimates on the full sample (column 1) and after restricting the control pool to the common support (column 2).

	(1) Baseline	(2) PS-trimmed
Avg. ATT [$e = 0, \ldots, 4$]	-0.123	-0.123
	(0.139)	(0.139)
Avg. ATT [$e = 0, \ldots, 6$]	-0.247	-0.246
	(0.142)	(0.142)
Pre-trend p-value	0.365	0.374
Observations	3,600,681	2,535,351
Episodes (treated units)	178	178
Control municipalities trimmed	—	285
Municipality × episode FE	Yes	Yes
Quarter × episode FE	Yes	Yes
Cluster	Municipality	Municipality

Notes: Column (1) is the baseline stacked DiD (full control pool). Column (2) restricts the control pool to municipalities whose estimated propensity score falls within the common support of treated municipalities (Crump et al. 2009 overlap trimming; thresholds at the 5th–95th percentiles of both distributions). Propensity scores estimated via probit with covariates: log population (2005), mean quarterly demobilizations (2005–06), and mean quarterly ex-combatant deaths (2005–06). All treated municipalities are retained; 285 control municipalities are dropped as outside the common support. Average ATT computed as unweighted mean of event-time coefficients over the stated window; standard errors from the full VCV matrix (in parentheses). Pre-trend p-value: joint Wald test, leads $e = -8, \ldots, -2$. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Table A3: Policy design margins. Observable benchmarks, estimated gaps, and targets for the three actionable margins identified in the empirical analysis.

Design margin	Current benchmark	Target / gap	Channel
Business viability	56.1% of openings functional after ∼4 years	66.1% functional (break-even*); gap: +10.1 pp	Credibility
Match low-risk performance	60.4% functional (low-risk) vs. 54.9% (high-risk)	Gap of 4.3 pp in survival rate	Credibility
Security exposure	$\text{ATT}[0,6] = -0.317$ in high-risk; -0.162 in low-risk	Excess deterrence: 0.155 per muni-quarter; security closures = 1.5% of total	Security
Realized violence	Deaths $\text{ATT}[0,6] = -0.024$ ($p = 0.109$)	Already nil; channel is perceived, not realized risk	Security
Programme scale	$\text{ATT}[0,6] = -0.247$; ≈ 184 fewer demob. over 7 quarters	ATT≈0 requires both margins	Both

Notes: All thresholds derived from pre-estimated results; no new regressions. Break-even survival rate: linear extrapolation from the (failure rate, $\text{ATT}[0,6]$) data points for low-risk (39.6% failure, ATT= -0.162) and high-risk (45.1% failure, ATT= -0.317) municipalities. Because this slope captures both fragility and security channels jointly, the break-even is a conservative bound (overestimates required survival improvement if security is simultaneously addressed). Security-related closures: 3.4% of all closures (N= 278 businesses). Magnitude: implied fewer demobilizations over quarters $e = 0$ to $e = 6$ across 106 treated municipalities.

Table A6: Spillover summary with own, indirect-neighbor, and additive-envelope effects.

Component	Average effect (0-6)
Own effect (core)	-0.247
Indirect neighbor exposure	-0.044
Own effect + neighbor control	-0.248
Additive envelope (own + indirect)	-0.291

Table A7: HonestDiD core sensitivity bounds for quarterly stacked DID (relative-magnitude method with Mbar grid).

Window	Mbar	Robust CI lower	Robust CI upper
0-4	0.00	-0.355	0.071
0-4	0.10	-0.498	0.071
0-4	0.20	-0.640	0.213
0-4	0.30	-0.782	0.355
0-4	0.40	-0.924	0.498
0-4	0.50	-1.208	0.640
0-4	1.00	-2.061	1.635
0-6	0.00	-0.510	-0.073
0-6	0.10	-0.656	0.073
0-6	0.20	-0.801	0.073
0-6	0.30	-1.093	0.364
0-6	0.40	-1.384	0.656
0-6	0.50	-1.530	0.801
0-6	1.00	-2.842	2.113
0-12	0.00	-0.737	-0.316
0-12	0.10	-1.159	-0.105
0-12	0.20	-1.580	0.105
0-12	0.30	-2.002	0.527
0-12	0.40	-2.423	0.948
0-12	0.50	-2.844	1.369
0-12	1.00	-4.108	3.687

Table A8: HonestDiD subgroup checks at Mbar=0, 0-12 window only.

Subgroup model	Window	Robust CI lower	Robust CI upper
High opening intensity	0-12	-1.479	-0.296
Top 10% municipality size	0-12	-1.517	-0.217
High exposure	0-12	-1.370	-0.587

Table A9: Depletion/composition checks.

Specification	Average effect (0-6)	Average effect (0-12)
Core stacked DID	-0.247	-0.533
Core + lagged cumulative depletion-rate control (per 100k)	-0.222	-0.495
Indirect neighbor exposure	-0.044	-0.062

Table A10: Glossary of key terms used in the empirical design.

Term	Working definition in this paper
ACR	Agencia Colombiana para la Reintegración, predecessor institution of ARN.
ARN	Agencia para la Reincorporación y la Normalización, current implementing agency for reintegration/reincorporation policy.
Reintegración	Program pathway historically used for demobilized ex-combatants from multiple armed organizations before the FARC peace accord cycle.
Reincorporación	Post-accord institutional pathway tailored to former FARC-EP signatories.
BIE	Beneficio de Inserción Económica framework governing productive-project support disbursement under milestone conditions.
Opening episode	Sequence that starts in the first municipality-quarter with positive FARC-linked business-unit openings and continues while openings remain consecutive.
Treated episode	An opening episode that enters the stacked DID event window and support restrictions used in the core 2005-2016 design.

Qualitative materials

Qualitative files used for coding and mechanism mapping are archived in the Git repository:

- `01_codebook_hibrido.tsv`
- `02_evidence_bank.tsv`
- `03_matriz_mecanismo_hallazgo.tsv`
- `05_quote_bank_bilingual.tsv`
- `06_limitaciones_y_alternativas.txt`