UP School of Economics
Discussion Papers

Discussion Paper No. 2026-04 May 2026

Obeying in advance: Duterte's election and the onset of the Philippine drug war

by

Jan Carlo B. Punongbayan

University of the Philippines School of Economics

Obeying in advance: Duterte's election and the onset of the Philippine drug war

Jan Carlo B. Punongbayan*

University of the Philippines School of Economics

May 11, 2026

UPSE Discussion Paper; comments are most welcome

Abstract

I use event-level data from the Armed Conflict Location & Event Data Project (ACLED) to estimate the impact of Rodrigo Duterte's "war on drugs" on political violence in the Philippines. Constructing a balanced panel of 80 provinces observed weekly from January 2016 through June 2017, I estimate event-study and difference-in-differences models that compare drug-war-tagged fatalities against a within-country placebo—non-drug-war political violence (battles, explosions, and non-drug violence against civilians). The headline finding is that the placebo series exhibits no discontinuity around either the election or the inauguration, while drug-war fatalities show a sharp, immediate spike. The violence increase is therefore specific to the drug war, rather than reflecting general conflict trends, ACLED source-coverage changes, or other simultaneous shocks. As a precondition for this comparison, I show that the relevant event date is the May 9 presidential election, not the June 30 inauguration. I discuss identification challenges including anticipation effects, the endogeneity of ACLED's tagging, and the absence of cross-regional treatment variation.

Keywords: Philippines, war on drugs, event study, political violence, Duterte
JEL classification: K42, D74, O17

*Email: jbpunongbayan@up.edu.ph. I thank Alicor Panao, Uriel Galace, and David Timberman for thoughtful comments and suggestions at the Philippine Political Science Association International Conference in Dumaguete City on 29 April 2026. Thanks as well to Aleli Kraft, Karl Jandoc, and other participants of the UPSE–PCED Friday Seminar Series on 8 May 2026. The author used generative AI to assist in this paper and assumes full responsibility for the final output.

1 Introduction

On June 30, 2016, Rodrigo Duterte assumed the presidency of the Philippines and immediately launched one of the most controversial law enforcement campaigns in recent history. The "war on drugs," operationalized through the Philippine National Police's *Oplan Double Barrel* and later *Oplan Tokhang*, resulted in thousands of killings of suspected drug users and dealers by state security forces and vigilante groups. Official government tallies place the death toll at approximately 6,252 suspects killed in anti-drug operations through 2022, while human rights organizations and academics estimate figures between 12,000 and 30,000 civilian deaths.

Despite the scale of the violence, quantitative analysis of the drug war's temporal and geographic dynamics has been limited by data availability. This paper leverages the Armed Conflict Location & Event Data Project (ACLED), which codes individual violent events in the Philippines and applies a "war-on-drugs-related event" tag to incidents linked to the drug campaign. We use these data to estimate event study models around the onset of the drug war, treating it as a sharp policy shock.

Our analysis makes three contributions. The headline contribution is the within-country placebo: we compare drug-war-tagged fatalities against non-drug-war political violence (battles, explosions, and non-drug violence against civilians) drawn from the same ACLED extract, and show that the comparison series exhibits no discontinuity around either the election or the inauguration. This rules out the most natural alternative explanations for the observed spike—ACLED source-coverage changes, general political instability, or seasonal patterns in violence—and locates the break specifically in the drug-war-tagged subseries. The corresponding difference-in-differences event study isolates the drug-war-specific component of the violence increase. The second and third contributions are methodological and descriptive: we document the precise timing of the violence onset at daily, weekly, and monthly resolution, and we show that the relevant treatment date for event-study estimation is the May 9 presidential election rather than the June 30 inauguration, since drug-war-tagged fatalities begin rising approximately two weeks after the election—consistent with anticipatory behavior by local police and vigilante groups acting on the announced policy program. The election-versus-inauguration comparison is a precondition for the placebo to deliver clean estimates; it is not the substantive finding.

The remainder of the paper is organized as follows. Section 2 reviews the related literature. Section 3 provides institutional background on the drug war. Section 4 describes the ACLED data and coding methodology. Section 5 presents descriptive patterns. Section 6 describes our empirical strategy. Section 7 presents results. Section 8 discusses identification challenges and limitations. Section 9 concludes.

2 Related literature

This paper contributes to a small but growing body of quantitative research on the Philippine drug war and, more broadly, to literatures on state-sponsored violence, political communication, and event study methods applied to political shocks.

2.1 Quantitative studies of the drug war

The most closely related work is Iglesias (2023), who uses the same ACLED data source to model the weekly trajectory of drug-war fatalities from July 2016 through June 2021 using Poisson regression. Iglesias conceptualizes the drug war as a campaign of state terror and examines whether accountability mechanisms—horizontal (legislative and judicial constraints), vertical (elections and approval ratings), and diagonal (protests and civil society)—explain the violence's decline after its initial peak. She finds that the violence peaked in the first few months of Duterte's term and then declined to low levels for the remainder of his presidency. The killings of Korean businessman Jee Ick-Joo and teenager Kian delos Santos, which triggered public outrage and Senate investigations, are associated with the largest immediate de-escalations. Horizontal accountability mechanisms (the ICC investigation, the UNHRC resolution) had significant long-run effects in reducing weekly fatalities. In contrast, protests had no significant effect, and the 2019 midterm elections showed no vertical accountability effect on violence levels.

Our analysis complements Iglesias's work in two respects. First, we focus on the onset rather than the full trajectory, using an event study design that is better suited to identifying the precise timing and magnitude of the initial shock. Second, our difference-in-differences approach, which uses non-drug-war political violence as a comparison group, provides a within-country placebo that Iglesias's single-series Poisson model does not exploit. Conversely, Iglesias's longer time frame and richer set of time-varying covariates—including approval ratings, protest counts, and specific policy events—allow her to address questions about de-escalation dynamics that our shorter window cannot capture.

2.2 Political communication and agenda setting

Jetter and Molina (2022) study whether Duterte's inauguration speech on June 30, 2016 systematically shifted Filipinos' policy priorities toward illegal drugs. Using daily Google Trends data and Pulse Asia opinion polls, they identify a structural break in drug-related online search activity coinciding with the inauguration. To establish causality, they exploit quasi-exogenous variation in exposure to the speech generated

by the historical timing of local festivals: provinces where a festival fell on inauguration day experienced approximately 20 percent smaller increases in drug-related search activity, consistent with reduced exposure to the speech. Survey data corroborate the pattern—the share of respondents listing crime as the most urgent national concern more than doubled between January and July 2016, but this increase was less than half as large in festival provinces. Importantly, no similar break is found for other policy topics, neighboring countries, or alternative event dates (such as the election or candidacy declaration).

The Jetter and Molina (2022) findings intersect with our analysis in a substantive way. Their identification of the inauguration speech as a discrete information shock that shifted public priorities is consistent with our finding that the election—not the inauguration—is the relevant event for actual violence onset. In other words, the political violence began escalating well before the speech that Jetter and Molina study. The two findings are not contradictory: the election may have signaled to security forces and vigilantes that the incoming administration would tolerate extrajudicial killings, triggering anticipatory violence, while the inauguration speech subsequently shifted *public opinion* about the salience of drugs as a policy issue. The former is about perpetrator behavior; the latter is about mass attitudes.

2.3 Economic consequences of the Duterte presidency

Balderas and Bernardo (2020) examine whether Duterte's negative business-related pronouncements had measurable effects on the Philippine Stock Exchange Index (PSEi). Using an interrupted time series model on daily PSEi data from June 30, 2016 to December 31, 2019, they classify presidential pronouncements into three categories: initial business pronouncements, anti-oligarch statements, and personal attacks against business leaders. They find a statistically significant negative relationship between negative business-related pronouncements and PSEi closing prices, with the largest effects concentrated on the first occasion a particular issue or personality was targeted. Aggregating across five key pronouncements during 2018–2019, the estimated stock market losses accumulate to approximately PHP 441 million within ten days, equivalent to 0.0027 percent of GDP.

While the Balderas and Bernardo (2020) study examines a different outcome (financial market reactions rather than violence), its event study approach and focus on the temporal precision of presidential actions resonates with our analysis. Both papers treat Duterte's policy signals as discrete, identifiable shocks whose effects can be isolated in time-series data.

2.4 Broader literatures

This paper also connects to several broader literatures. First, the event study methodology we employ—with province fixed effects and event-time dummies—follows the framework developed for settings with a single treatment date and multiple cross-sectional units, as discussed in Schmidheiny and Siegloch (2023) and Sun and Abraham (2021). The choice between binned and unbinned endpoint specifications follows Schmidheiny and Siegloch's recommendations.

Second, our use of non-drug-war political violence as a within-country comparison group draws on the logic of difference-in-differences designs applied to conflict settings. The key identifying assumption—that drug-war and non-drug-war violence would have followed parallel trends absent the policy—is analogous to assumptions made in studies of policy interventions affecting specific categories of crime or conflict.

Third, Iglesias's conceptual framework drawing on Laebens and Lührmann (2021) situates the Philippine drug war within a comparative literature on democratic backsliding and state violence in weak democracies, where accountability mechanisms may impede but not prevent authoritarian erosion. The drug war's arc—a sharp escalation followed by gradual decline under institutional pressure—is consistent with this framework and with comparative cases from Indonesia, Thailand, and Brazil that Iglesias discusses.

Fourth, our title and interpretive frame draw on **?**, whose first lesson in *On Tyranny* is the observation that authoritarian regimes are partly produced by subjects who comply with anticipated demands before any formal order is given. The pre-inauguration ramp in drug-war fatalities—killings that began weeks before Duterte assumed formal authority over the Philippine National Police—is consistent with this pattern of anticipatory compliance: local police and vigilante groups acting on a publicly announced policy program rather than waiting for explicit instructions. We emphasize that our empirical evidence does not directly identify the behavioral mechanism. The timing pattern we document is consistent with anticipatory compliance, with opportunistic violence by criminal actors expecting reduced enforcement, or with other agency-level responses to the election outcome; we cannot adjudicate among these on the basis of aggregate fatality data alone. The framing reflects our interpretation of what the timing pattern most plausibly reflects, given the descriptive evidence on the campaign's rhetoric and the absence of any corresponding break in non-drug-war political violence.

3 Background

3.1 The Philippine drug war

Rodrigo Duterte campaigned for the presidency on an explicit promise to eliminate the drug trade in the Philippines within three to six months. His campaign rhetoric drew on his reputation as mayor of Davao City, where he had overseen a long-running anti-crime campaign widely linked to extrajudicial killings. Duterte won the May 9, 2016 presidential election with approximately 39 percent of the vote in a multi-candidate field and was inaugurated on June 30, 2016.

Upon taking office, Duterte appointed Ronald "Bato" dela Rosa—his former Davao City police chief—as Director General of the Philippine National Police (PNP). Dela Rosa immediately issued Command Memorandum Circular No. 16-2016, launching *Oplan Double Barrel*, the operational framework for the anti-drug campaign. The operation had two components: *Oplan Tokhang* (a portmanteau of Cebuano words meaning "knock and plead"), which targeted individual drug suspects at the community level, and *Oplan High Value Target*, which focused on larger drug networks.

3.2 Key personnel and geographic shifts

The drug war's geographic intensity was closely linked to personnel assignments. Duterte installed a network of police officers from Davao City—colloquially known as the "Davao Boys"—into key positions in the PNP hierarchy. Oscar Albayalde was appointed director of the National Capital Region Police Office (NCRPO) simultaneously with Dela Rosa's appointment, overseeing the initial concentration of violence in Metro Manila.

As the drug war attracted domestic and international scrutiny focused on Metro Manila, the geographic center of gravity shifted. Aaron Aquino, who had served as Central Luzon (Region 3) police director from May 2016, oversaw a sharp increase in drug-war killings in that region. When Aquino was promoted to head the Philippine Drug Enforcement Agency (PDEA) in August 2017, his successor and later Joel Napoleon Coronel—formerly head of the Manila Police District—continued aggressive operations in Central Luzon. By 2018, Central Luzon surpassed NCR in drug-war fatalities for the first time, a shift that journalists and human rights organizations attributed directly to these personnel transfers.

3.3 Policy shifts and institutional changes

The drug war's institutional framework underwent several formal changes. In January 2017, following the police killing of a South Korean businessman inside PNP

headquarters, Duterte briefly suspended PNP involvement in anti-drug operations and transferred authority to PDEA. This suspension was short-lived; the PNP was "reactivated" within weeks. In March 2017, Executive Order No. 15 created the Inter-Agency Committee on Anti-Illegal Drugs (ICAD). In October 2017, Duterte again announced a transfer of operations to PDEA, but this was similarly reversed.

4 Data

4.1 ACLED Philippines data

We use data from the Armed Conflict Location & Event Data Project (ACLED), which provides geocoded, event-level records of political violence and protest activity worldwide. ACLED's Philippines coverage begins on January 1, 2016. Our extract, downloaded on February 26, 2026, contains 25,783 events through January 1, 2022.

The January 1, 2016 start date deserves comment because it is plausibly related to the drug war itself. ACLED's Philippines coverage was rolled out as part of the project's expansion into Southeast Asia, with a January 2016 start that pre-dates Duterte's May 9 election victory by four months and the launch of *Oplan Double Barrel* by six months. I do not have evidence that ACLED initiated Philippines coverage in anticipation of the drug war specifically—Duterte's national candidacy was only formalized in late November 2015, and the regional expansion timetable likely reflected planning decisions made earlier. But two implications follow. First, the pre-treatment window is short (roughly 18 weeks before the May 9 election), which limits power for pre-trend tests and rules out long-run placebo dates. Second, and more substantively, source coverage in early 2016 was being built up at the same time as the events of interest: ACLED's media-monitoring infrastructure for the Philippines was new. If coverage intensity rose mechanically through the first half of 2016 as the project added sources, raw counts of any violence type would trend upward even absent a real change on the ground.

The within-country placebo neutralizes this concern. Both drug-war and non-drug-war violence are coded by the same ACLED team from the same media reports using the same methodology. Any time-varying shift in source coverage or scrutiny that affects one category of event also affects the other; in the difference-in-differences specification, the common time path β_k in Equation (2) absorbs that shift, and the differential coefficients δ_k identify the drug-war-specific component. The flat, statistically null pre-period in the placebo series (Section 7) is consistent with this argument: had source coverage been ramping mechanically, non-drug-war fatalities would also have trended upward through the first half of 2016, and they do not.

ACLED codes each event with an event type (Battles, Violence against civilians,

Explosions/Remote violence, Riots, Protests, Strategic developments), sub-event type, actors involved, geographic location (country, region, province, municipality), date, reported fatalities, and free-text notes. Critically for our purposes, ACLED applies a "war-on-drugs-related event" tag in the `tags` column to events linked to the Philippine drug war.

We do not use Dahas, the Drug Archive of the Philippine Drug Research Project housed at the Third World Studies Center of the University of the Philippines Diliman. Dahas compiles incident-level records of drug-related killings from local media and is widely cited in Philippine policy discussions. We restrict the analysis to ACLED for two reasons. First, Dahas access for bulk download has been a recurring constraint for outside researchers; the project has not, to our knowledge, made a machine-readable public extract available on the same footing as ACLED. Second, ACLED's coding methodology—including the explicit "war-on-drugs-related event" tag, the documented inclusion criteria, and the within-country comparison group of non-drug-war political violence—is purpose-built for the event-study and difference-in-differences design we employ. Triangulating with Dahas is a natural robustness exercise for future work, and we expect the broad temporal patterns to be similar given that both datasets draw on overlapping media sources.

4.2 ACLED coding decisions for drug-war events

ACLED's coding methodology for drug-related violence in the Philippines, documented in their publicly available coding decisions guide (ACLED, 2024), is central to interpreting our results. Several features of the coding are worth highlighting.

First, ACLED does not normally include criminal violence in its dataset, but makes an exception for drug-related violence in the Philippines given its "highly politicized nature," classifying it as political violence. Second, ACLED includes four categories of drug-related events: (i) killings of drug suspects by government security forces; (ii) clashes between armed drug suspects and state forces; (iii) killings of drug suspects by vigilantes; and (iv) violence between drug suspects, including inter-gang clashes.

Third, ACLED employs an "intentionally broad" definition of "drug suspect" that encompasses any actor reported to have a link to drugs "whether credible, fabricated, or mistaken." This design choice is important: it means the tag captures the full scope of the drug war as implemented, including cases where police accounts were later found to involve planted evidence or fabricated armed resistance.

Fourth, when police reports describe shoot-outs with drug suspects but only suspects were killed (with no state-force casualties), ACLED codes these as "Violence against civilians" rather than "Battles," reflecting the widely documented pattern of staged encounters. Events are coded as "Battles" only when state forces also suffered

casualties.

4.3 Sample construction

Our analysis sample is constructed as follows. We identify drug-war events by filtering for the "war-on-drugs-related event" tag, yielding 12,863 events with 16,970 total fatalities. For the comparison group, we restrict to the same violent event types present in the drug-war sample—Violence against civilians, Battles, and Explosions/Remote violence—but *not* tagged as drug-war-related, yielding 9,851 events. We exclude Protests, Riots, and Strategic developments from the comparison group to ensure comparability in the type of violence being measured.

We construct a balanced panel at the province-week level. For each of the 80 Philippine provinces observed in the data, we create observations for each week (Monday-anchored) in the estimation window. Fatalities are summed within each province-week cell separately for drug-war and non-drug-war events, with zeros assigned to province-weeks with no recorded events. Table 1 provides summary statistics.

5 Descriptive patterns

5.1 Time series of drug-war fatalities by region

Figure 1 plots monthly drug-war fatalities by Philippine region for the eight regions with the highest total death counts. Panel A reports raw counts; Panel B normalizes by 2015 PSA Census regional population to express fatalities per 100,000 residents. The count series is dominated by a massive spike beginning in July 2016, concentrated in the National Capital Region (NCR), Central Luzon, and Calabarzon—the three regions accounting for approximately two-thirds of all recorded drug-war deaths. After the initial surge, fatalities decline sharply across all regions by mid-2017, settling into a lower but persistent baseline through 2021. The per-capita view reorders the ranking: NCR's lead narrows, and Central Visayas and Central Luzon reach broadly comparable peak intensities once population is taken into account, which is informative for thinking about which regions experienced the most intense violence per resident as opposed to the largest absolute death tolls.

5.2 The July 2016 discontinuity

Figure 2 examines the onset of violence at daily and weekly resolution. The upper panel shows daily drug-war fatalities throughout 2016 with a 7-day rolling average. The lower panel aggregates to weekly totals and zooms in on the April–August win-

dow. The transition is stark: the week of June 22–30 averaged 5.6 fatalities per day, while the first week of July averaged 51.1—a 15-fold increase across the inauguration date. Compared to the January–June baseline of 1.5 fatalities per day, July ran at 54.8 per day, a 37-fold increase.

However, the break is not perfectly sharp at June 30. A pre-inauguration ramp begins around May 23—approximately two weeks after the May 9 election. Daily fatalities rise from approximately 0.7 per day (May 1–22) to approximately 5 per day (May 23–June 29). This anticipation effect is orders of magnitude smaller than the post-inauguration spike but is substantively important for the choice of event date in the formal event study.

5.3 Geographic shifts and personnel appointments

Figure 3 overlays key personnel appointments on the fatality time series for the four most-affected regions. The visual co-movement between appointment timing and violence intensity is suggestive, particularly in Central Luzon, where successive police directors with records of aggressive drug-war enforcement (Aaron Aquino, Joel Coronel) coincide with regional spikes. However, the appointments are clearly endogenous to the drug war's evolving strategy, limiting causal interpretation of this pattern.

6 Empirical strategy

6.1 Event study framework

We estimate event study models using the balanced province-week panel described in Section 4. Let $Y_{p,t}$ denote fatalities in province p in week t. We estimate:

$$f(Y_{p,t}) = \alpha_p + \sum_{k \neq -1} \beta_k \cdot \mathbf{1}[\text{event\_time}_t = k] + \varepsilon_{p,t} \tag{1}$$

where α_p are province fixed effects, $\mathbf{1}[\text{event\_time}_t = k]$ are indicators for each week relative to the event date, and $k = -1$ (the week before the event) is the omitted reference period. The coefficients $\{\beta_k\}$ trace out the time path of fatalities relative to the reference period, absorbing time-invariant province-level differences. Standard errors are clustered at the province level (80 clusters). All event studies are estimated in Stata via the `xtevent` package of Freyaldenhoven et al. (2021), with figures produced by the companion command `xteventplot`. Do-files reproducing every specification are bundled in `stata/` alongside this manuscript.

Given the high fraction of zeros in the outcome (approximately 72 percent for drug-war fatalities), we use the inverse hyperbolic sine (IHS) transformation $f(Y) =$

$\text{arcsinh}(Y) = \ln(Y + \sqrt{Y^2 + 1})$ as our primary specification. The IHS is approximately logarithmic for large values of Y and approximately linear near zero, avoiding the need to drop or arbitrarily recode zeros.

6.2 Difference-in-differences specification

To isolate drug-war-specific violence from common shocks, we stack the drug-war and non-drug-war panels and estimate:

$$f(Y_{p,t,g}) = \alpha_p + \gamma \cdot D_g + \sum_{k \neq -1} \left[\beta_k \cdot \mathbf{1}[t = k] + \delta_k \cdot \mathbf{1}[t = k] \cdot D_g\right] + \varepsilon_{p,t,g} \tag{2}$$

where $g \in \{\text{drug-war}, \text{non-drug-war}\}$, $D_g = 1$ for drug-war events, β_k captures the common time path, and δ_k captures the *differential* effect on drug-war violence relative to non-drug-war violence. The $\{\delta_k\}$ coefficients are the objects of interest: they net out any common shocks (seasonal patterns, ACLED source coverage changes, general political instability) and isolate the drug-war-specific component.

6.3 Choice of event date

We consider two candidate event dates: the presidential inauguration (June 30, 2016) and the presidential election (May 9, 2016). We evaluate both on pre-trend properties.

The choice matters for a methodological rather than a substantive reason. The event-study design is identified off the assumption that the pre-event period is a clean counterfactual for the treated unit's path absent the policy. If the policy onset is in fact earlier than the nominal event date, the pre-period absorbs treated observations and the reference week is contaminated; the pre-trend test then mechanically fails, and the post-event coefficients are biased toward zero (relative to a contaminated baseline). This is exactly the pattern we document in Section 7 when we anchor to the inauguration.

We emphasize that the date choice is a *precondition* for the design rather than the paper's substantive contribution. The substantive question is whether the violence spike is attributable to the drug war specifically, as opposed to general conflict trends, ACLED source-coverage shifts, or other simultaneous shocks. That question is answered by the within-country placebo—comparing drug-war-tagged fatalities against non-drug-war political violence—which we report in Section 7. The election-versus-inauguration analysis is logically prior to that comparison: it ensures that the pre-period used to anchor the event study is not already polluted by anticipation. Once the date is correctly chosen, the placebo comparison delivers the actual identifying evidence.

7 Results

7.1 Inauguration as event date: Pre-trend violation

Figure 4 presents the event study using the inauguration (June 30) as the event date with the week before inauguration as the reference period. Panel A shows drug-war fatalities: the post-inauguration spike is large and immediate, but the pre-period is severely violated. Of 25 pre-period confidence intervals, 19 exclude zero, and the pre-period coefficients are systematically negative (mean $= -0.172$). This pattern reflects the post-election anticipation effect documented in Section 5—the reference week ($t = -1$) is already elevated relative to the deep pre-period. The difference-in-differences interaction (not shown) exhibits a similar violation, with 20 of 25 pre-period CIs excluding zero.

7.2 Election as event date: Clean pre-trend

Figure 5 re-estimates the event study using the election (May 9) as the event date. The improvement is dramatic. In the drug-war specification, **0 of 18** pre-period confidence intervals exclude zero, and the pre-period mean coefficient is -0.003—essentially zero. The pre-election period is genuinely flat, consistent with the absence of drug-war violence before Duterte's election made the policy a certainty.

The post-election period reveals a two-phase onset. In the first 7 weeks after the election (the pre-inauguration anticipation phase), coefficients drift modestly upward. Around $t = 8$ (corresponding to the inauguration and launch of *Oplan Double Barrel*), there is a sharp vertical jump to peak levels. This pattern is consistent with local actors beginning to anticipate and act on the incoming administration's drug war mandate before the formal policy machinery was activated.

Panel B shows the non-drug-war placebo: no discontinuity is visible at either the election or inauguration, with noisy but statistically insignificant coefficients throughout. Panel C shows the DiD interaction, with only 2 of 18 pre-period CIs excluding zero and a clear post-election break.

7.3 Comparison of specifications

Figure 6 presents a side-by-side comparison of the inauguration and election specifications for both the drug-war outcome and the DiD interaction. The contrast in pre-trend quality is visually striking and confirmed by the summary statistics in Table 2.

7.4 Placebo event date

A natural concern with our preferred specification is that the May 9 break could be a mechanical artifact of the event-study design rather than a real treatment effect—for instance, if the design were prone to detecting spurious breaks at arbitrary calendar dates given the structure of the data. To rule this out, we re-estimate the event study using a fake event date that falls strictly inside the pre-election window.

We pick March 7, 2016 (week index 9) as the placebo date—roughly midway through the pre-election ACLED data—and restrict the panel to weeks on or before May 8, 2016 to prevent the real post-election ramp from contaminating the post-placebo window. We then estimate the same specifications (drug-war outcome and DiD interaction) on this truncated panel. Figure 7 reports the result.

The placebo specifications are flat. The drug-war series shows 1 of 8 pre-period confidence intervals excluding zero (consistent with the chance rate of 5 percent over 8 tests) and a mean pre-period coefficient of $+0.021$. The DiD interaction shows 0 of 9 pre-period CIs excluding zero and a mean of -0.056. Neither series produces a discontinuity at the placebo date, in contrast to the sharp break observed at May 9 in the main specification. This is consistent with our preferred specification capturing a genuine treatment effect rather than a mechanical artifact of the event-study design applied to this data structure.

We acknowledge that this is a single-date placebo rather than an exhaustive permutation test across all candidate pre-election dates. A more thorough exercise—running placebos at every Monday between January 2016 and the election and showing that the May 9 break is unique—is a natural robustness check we leave for future revisions.

8 Discussion

8.1 Identification

Our event study design exploits the sharp, largely unanticipated nature of the drug war's onset. Several features strengthen identification. First, the treatment is visually unambiguous: a 37-fold increase in daily fatalities is not a marginal effect susceptible to specification sensitivity. Second, the non-drug-war placebo directly addresses the most threatening confounders—ACLED coding changes, media coverage shifts, or general political instability—by showing that these factors did not produce a comparable break in non-drug violence. Third, the province-level panel with 80 clusters provides adequate degrees of freedom for cluster-robust inference, unlike the 17-region specification.

However, several identification challenges remain:

Anticipation. Drug-war violence began rising after the election but before the inauguration. We address this by using the election as the event date, which absorbs the anticipation phase into the post-period. This is appropriate if the election is the true information shock, with the inauguration serving as an intensification mechanism rather than the treatment onset.

Absence of a control group. The drug war was a nationwide, simultaneous policy. There is no untreated region that could serve as a counterfactual. Our comparison uses a different *type* of violence (non-drug-war political violence) rather than a different *place*. The parallel trends assumption therefore requires that drug-war and non-drug-war violence would have evolved similarly absent the policy, which is a behavioral claim.

Endogeneity of the tag. ACLED's "war-on-drugs-related event" tag is applied by coders based on media reporting. After the inauguration, media attention to drug-related violence increased dramatically, potentially leading to more complete tagging of events that might previously have gone untagged or uncoded. The flat pre-election period partially addresses this—there simply are very few events to tag—but we cannot rule out that post-election increases partly reflect improved source coverage rather than increased violence.

No treatment variation for the initial shock. Because all provinces are "treated" simultaneously, we cannot exploit cross-regional variation in treatment timing for the July 2016 shock.

Personnel appointments as treatment variation. A natural candidate for cross-regional variation is the staggered appointment of PNP commanders to regional offices. Using data on 109 key appointments under Duterte (July 2016 to May 2022), we attempted a staggered event study centered on each region's first appointment date, with region fixed effects and cluster-robust standard errors. The results are not supportive of a causal interpretation. The pre-trend is severely violated: 8 of 25 pre-period confidence intervals exclude zero, and the mean pre-period coefficient is positive (+0.33), indicating that regions receiving appointments were already experiencing elevated violence relative to their own mean. Post-appointment coefficients are negative on average (-0.14 for weeks 0–12), and most regions exhibit *declining* fatalities in the year following their first appointment—consistent with mean reversion or the broader de-escalation documented elsewhere, rather than an appointment-driven intensification.

Three features of the data compound the identification problem. First, appointments cluster in a few batch waves—January 27, 2017 (8 regions), March 16, 2017 (6 regions), January 18, 2018 (7 regions)—leaving very few not-yet-treated regions as controls in any given wave. Only one region (Bicol) never receives an appointment in the data, providing effectively no clean comparison group. Second, the appointments are clearly endogenous: commanders are rotated *in response to* the drug war's geographic

dynamics, not independently of them. The cross-sectional correlation between appointment intensity and post-appointment fatalities is weak ($r = 0.19$), and the direction of the pre-trend—positive and significant—is consistent with reverse causality (violence → appointment, not appointment → violence). Third, the national drug war policy already "treats" all regions simultaneously from mid-2016 onward; appointments represent changes in enforcement intensity, not a switch from no treatment to treatment, making the parallel trends assumption difficult to motivate. We report these results in Appendix Figure 11 for transparency but do not interpret them causally.

8.2 The pre-inauguration ramp and the question of attribution

The two-phase onset documented in Section 7—a modest rise in drug-war fatalities between the May 9 election and the June 30 inauguration, followed by a sharp jump after *Oplan Double Barrel* was operationalized—admits a superficially exculpatory reading: if killings began before Duterte assumed formal authority over the Philippine National Police, then perhaps the violence was driven by local police initiative rather than presidential direction, and the spike cannot be attributed to him. This reading is wrong, and it is worth saying so explicitly because the question of whether Duterte caused the killings is central to the ongoing International Criminal Court proceedings against him.

Three points cut against the exculpatory reading. First, the pre-inauguration ramp is more naturally interpreted as anticipatory compliance with a publicly announced policy program than as autonomous local action—what **?** calls "obeying in advance," the pattern by which authoritarian regimes are partly produced by subjects who comply with anticipated demands before any formal order is given. Duterte campaigned for the presidency on an explicit, repeatedly stated promise to kill drug suspects; the rhetoric was specific enough that local police and vigilante groups had clear guidance on what the incoming administration would condone. The fact that violence began rising approximately two weeks after the election—and not at random points before it—is itself evidence that the election outcome, not some independent local trend, was the trigger. The non-drug-war placebo, which is flat through the same period, reinforces this: there was no general post-election shift in Philippine political violence; the shift was specific to the category of violence Duterte had named.

Second, the legal standard of causation under the Rome Statute does not require that the principal hold formal authority at the moment a crime is committed. Modes of individual criminal responsibility include ordering, soliciting, inducing, and contributing to the commission of a crime by a group acting with a common purpose (Article 25(3)); command responsibility (Article 28) extends liability to a superior who "effectively" commands subordinates and fails to prevent or punish their crimes. These

are different standards from the statistical notion of causal effect that motivates this paper, and we are not in a position to render a legal judgment. But the timing pattern documented here—a ramp that begins immediately after a candidate who campaigned on extrajudicial killings wins office, in the absence of any other shock that could explain it—is at least consistent with the kind of causal contribution the legal standards contemplate. It is not consistent with the claim that the killings were a spontaneous local phenomenon to which Duterte was incidental.

Third, the post-inauguration jump documented in our descriptive series and event study—a roughly fifteenfold increase in daily fatalities across the inauguration date and a thirty-sevenfold increase relative to the pre-election baseline—makes clear that the formal assumption of office was not merely ceremonial. The intensification mechanism (the appointment of dela Rosa as PNP chief, the launch of *Oplan Double Barrel*, the public defense of killings by the executive) was directly under Duterte's control and is contemporaneous with the largest part of the violence increase. The pre-inauguration ramp does not displace responsibility from the principal to the agent; if anything, the fact that the rhetoric alone was sufficient to begin generating killings is a stronger statement about the causal force of presidential signaling than a clean inauguration-day break would have been.

8.3 Interpretation of the IHS coefficients

The IHS transformation complicates direct interpretation. For large values of Y, $\text{arcsinh}(Y) \approx \ln(2Y)$, so IHS coefficients approximate log changes. The peak drug-war coefficient of approximately 0.80 at $t = 4$ corresponds roughly to an $e^{0.80} - 1 \approx 123$ percent increase relative to the reference week. However, given the mass of zeros in the data, this interpretation should be treated as approximate.

8.4 ACLED data limitations

Several features of the ACLED data warrant caution. The data begin in January 2016, providing only five months of pre-election observations. ACLED's Philippines coverage was initiated partly in response to the drug war itself, which means the data infrastructure was being built during the early treatment period. ACLED relies on media reporting, and under-reporting of drug-war killings—particularly vigilante killings in poor urban communities—is widely documented. The 16,970 fatalities in our sample should be treated as a lower bound.

9 Conclusion

This paper documents the sharp onset and geographic evolution of the Philippine drug war using event-level data from ACLED. Our event study analysis demonstrates that the election of Rodrigo Duterte on May 9, 2016—rather than his inauguration on June 30—represents the relevant treatment date for empirical analysis, as drug-war violence began escalating in the weeks between the two events. The inauguration and launch of *Oplan Double Barrel* dramatically intensified the violence, producing a discontinuity visible at daily resolution.

The within-country placebo comparison—showing that non-drug-war political violence exhibited no break at either date—strengthens the attribution of the violence spike to the drug war specifically, rather than to general conflict trends or data artifacts. The difference-in-differences specification isolating drug-war-specific violence yields clean pre-trends when anchored to the election date.

Our analysis establishes descriptive facts and a methodological framework for further causal work. A natural extension would exploit the staggered appointment of PNP regional commanders as a source of cross-regional variation. However, as we document in Section 8, a preliminary staggered event study using 109 key appointments yields a severely violated pre-trend and negative post-appointment coefficients, consistent with appointments responding endogenously to—rather than causing—regional violence dynamics. Identifying a credible instrument for the timing or intensity of personnel rotations remains an open challenge for future research.

Several other extensions are worth noting. First, the endogeneity of post-2016 appointments to drug-war dynamics could potentially be circumvented by looking backward: if police generals who later effected drug-war killings were already being rewarded with promotions *before* Duterte took office—in jurisdictions where extrajudicial killings of suspected drug users were already common—the pattern would be informative about whether the post-2016 commanders were jockeying for position by signalling enforcement style under prior administrations. Constructing a panel of PNP officer promotions and matching it to pre-2016 incident data (where available) would be one way to test this. Second, demographic mortality data offer an independent triangulation. Excess mortality among young, poor males in the regions and barangays where drug-war violence concentrated would be a complementary signal that does not depend on the "drug-war" tag being applied. Philippine Statistics Authority (PSA) vital-registration records on causes of death could in principle support such an exercise, although certificates and autopsies are known to be incomplete and any explicit "drug war" coding on death certificates is likely to be of poor quality; the more promising approach is probably an excess-mortality decomposition by age, sex, and small-area income proxies rather than direct reliance on cause-of-death tags.

Third, the 2018 increase in pay for the police, military, and other men and women in uniform (MUPs)—roughly doubling base pay for entry-level personnel—creates a candidate confounder for the post-2017 trajectory: if the salary increase changed the composition or behavior of frontline officers, the gradual de-escalation we and others document may partly reflect that compensation shock rather than purely the accountability mechanisms emphasized in Iglesias (2023). Disentangling these would require richer personnel data than we currently use.

References

ACLED (2024). Coding decisions on drug-related violence in the Philippines. Armed Conflict Location & Event Data Project. Last updated 14 October 2025.

Amnesty International (2017). Philippines: The police's murderous war on the poor. 31 January 2017.

Amnesty International (2019). They just kill: Ongoing extrajudicial executions and other violations in the Philippines' 'war on drugs'.

Balderas, E. B. O. and Bernardo, A. M. B. S. D. (2020). By his words alone: The economic consequences of Rodrigo Duterte. *The Philippine Review of Economics*, 57(1):71–100.

Freyaldenhoven, S., Hansen, C., Pérez Pérez, J., and Shapiro, J. M. (2021). Visualization, identification, and estimation in the linear panel event-study design. *NBER Working Paper No. 29170*. Stata package `xtevent` available via SSC.

Human Rights Watch (2017). License to kill: Philippine police killings in Duterte's 'war on drugs'. 2 March 2017.

Human Rights Watch (2020). Our happy family is gone: Impact of the 'war on drugs' on children in the Philippines. 27 May 2020.

Iglesias, S. (2023). Explaining the pattern of 'war on drugs' violence in the Philippines under Duterte. *Asian Politics & Policy*, pages 1–21.

International Criminal Court (2021). Situation in the Philippines: ICC Pre-Trial Chamber I authorises the opening of an investigation. 15 September 2021.

Jetter, M. and Molina, T. (2022). Persuasive agenda-setting: Rodrigo Duterte's inauguration speech and drugs in the Philippines. *Journal of Development Economics*, 156:102843.

Laebens, M. G. and Lührmann, A. (2021). What halts democratic erosion? The changing role of accountability. *Democratization*, 28(5):908–928.

Rappler (2019). Central Luzon: New killing fields in Duterte's drug war. *Rappler*.

Schmidheiny, K. and Siegloch, S. (2023). On event studies and distributed-lags in two-way fixed effects models: Identification, equivalence, and generalization. *Journal of Applied Econometrics*, 38(5):695–713.

Sun, L. and Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 225(2):175–199.

Tables

Table 1: Summary statistics.

	Drug-war events	Non-drug-war events
Total events	12,646	9,851
Total fatalities	16,936	13,820
Unique provinces	74	78
Date range	Jan 2016–Dec 2021	Jan 2016–Jan 2022
Event types		
Violence against civilians	12,038 (95.2%)	3,922 (39.8%)
Battles	594 (4.7%)	5,110 (51.9%)
Explosions/Remote violence	14 (0.1%)	819 (8.3%)
Province-week panel (estimation window)		
Provinces	80	80
Weeks	79	79
Observations	6,320	6,320
Mean fatalities/province-week	1.42	0.73
Median fatalities/province-week	0	0
Fraction zeros	0.85	0.90

Notes: Drug-war events are those tagged "war-on-drugs-related event" in ACLED. Non-drug-war events are Violence against civilians, Battles, and Explosions not tagged as drug-war-related. Estimation window: 26 weeks before to 52 weeks after the event date.

Table 2: Pre-trend diagnostics across specifications.

Specification	Outcome	CIs excl. zero	Pre-period mean
Inauguration, $t = -1$ ref.	Drug-war	19/25	-0.172
Inauguration, $t = -1$ ref.	DiD	20/25	-0.272
Election, $t = -1$ ref.	Drug-war	0/18	-0.003
Election, $t = -1$ ref.	DiD	2/18	$+0.093$
Election, binned endpoints	Drug-war	0/8	-0.006
Election, binned endpoints	DiD	0/8	$+0.071$
Placebo (Mar 7, 2016)	Drug-war	1/8	$+0.021$
Placebo (Mar 7, 2016)	DiD	0/9	-0.056

Notes: "CIs excl. zero" counts the number of pre-period event-time coefficients whose 95% confidence interval does not contain zero. All specifications use 80-province panel, IHS outcome, province FE, and cluster-robust SE. "Binned endpoints" aggregates distant periods into single bins to reduce noise. The placebo specification (last two rows) uses a fake event date (March 7, 2016) on a panel restricted to weeks $\leq$ May 8, 2016; see Section 7.4.

Figures

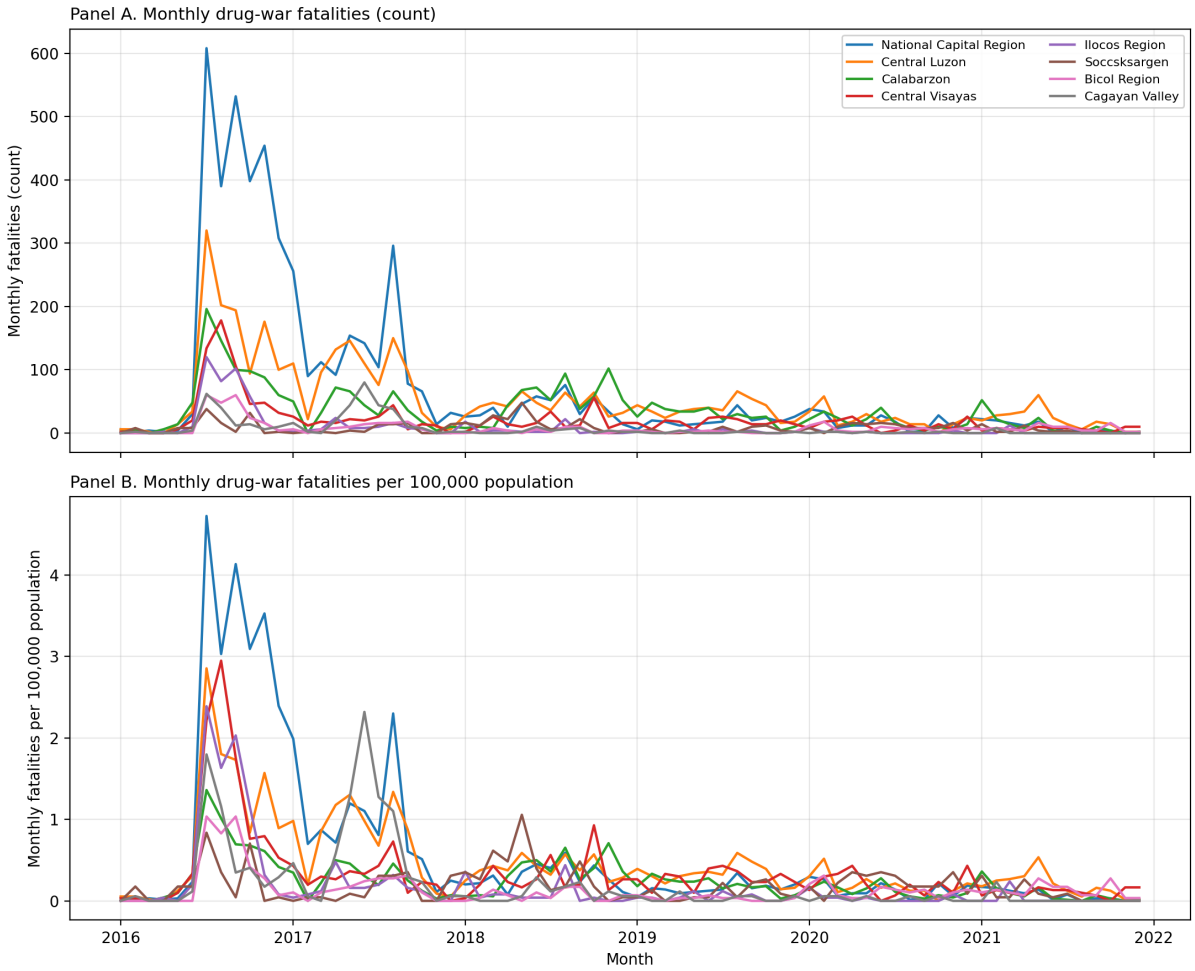

Figure 1: Monthly drug-war fatalities by region, 2016–2021. Panel A: counts. Panel B: per 100,000 population.

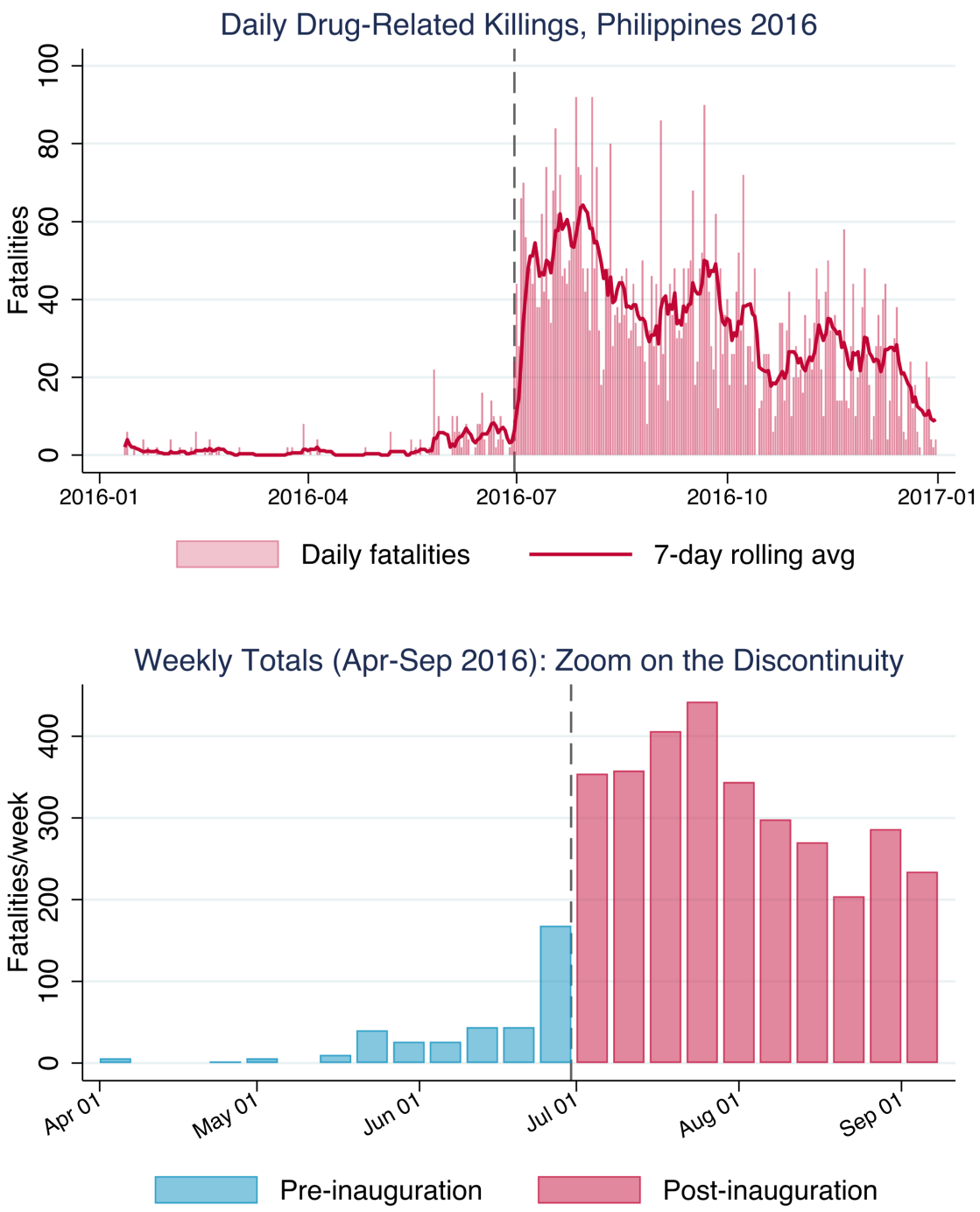

Figure 2: Daily drug-war fatalities, 2016: The July discontinuity.

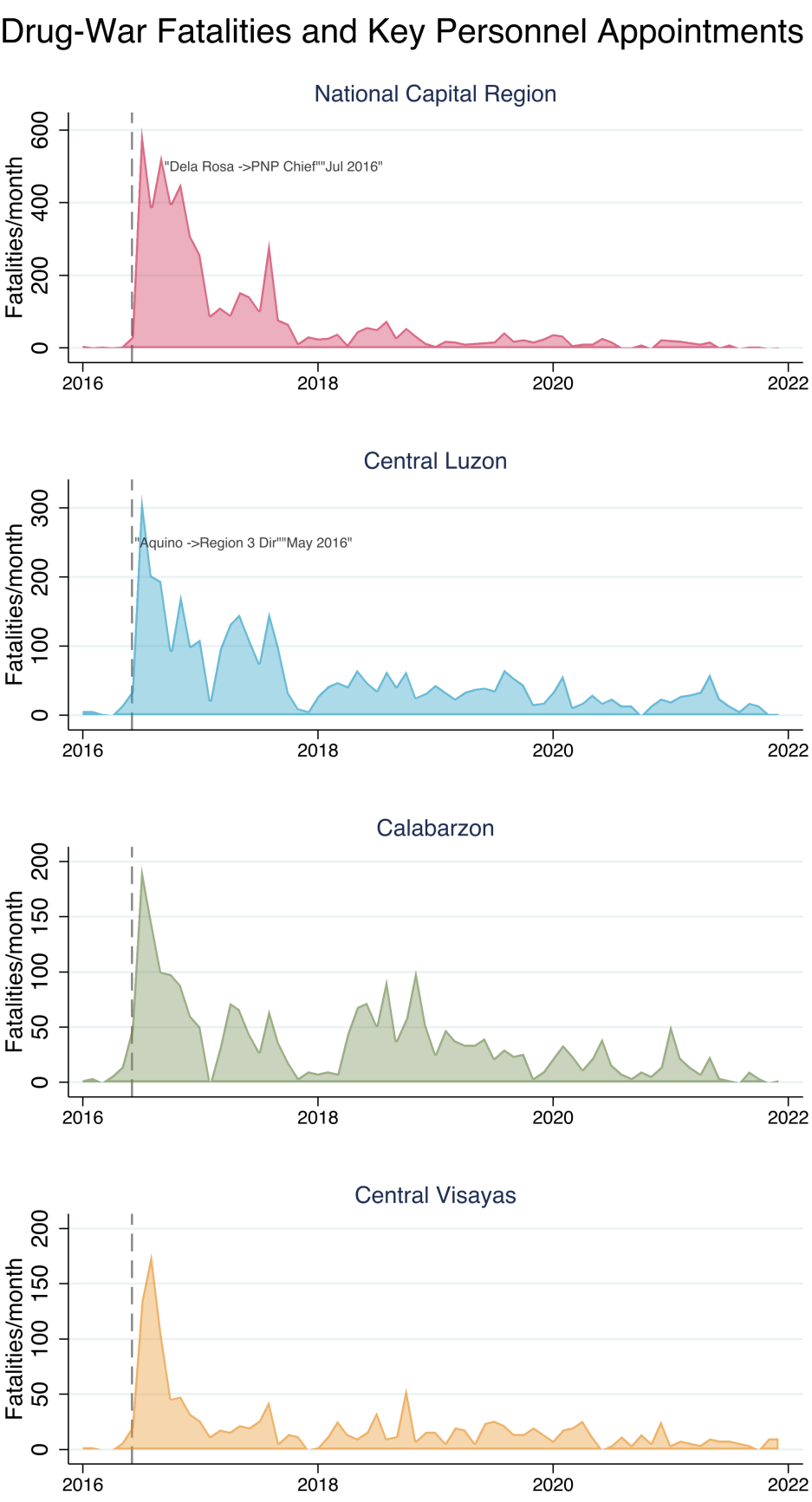

Figure 3: Drug-war fatalities and key personnel appointments.

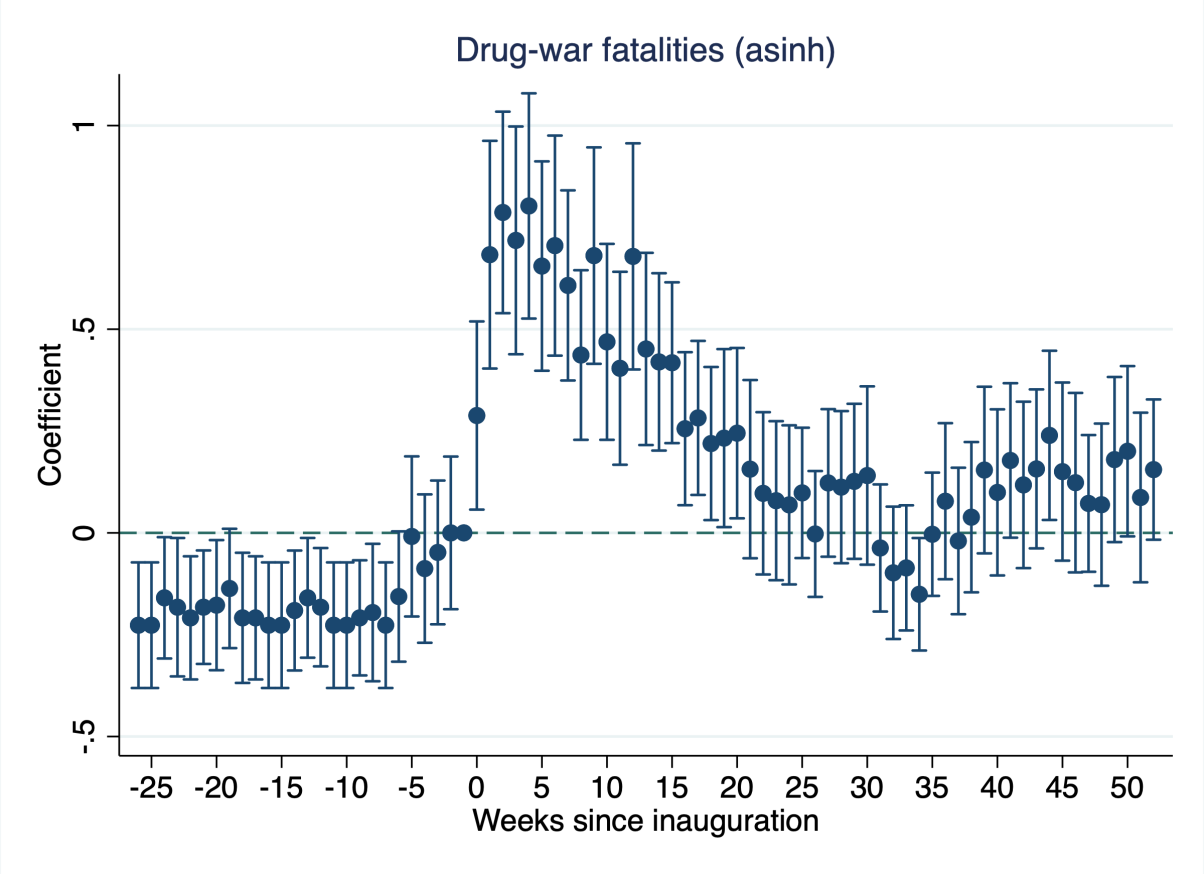

Figure 4: Event study: Inauguration as event date (June 30, 2016). Estimated with `xtevent`; outcome is arcsinh(drug-war fatalities), province FE absorbed, SE clustered at the province level. Reference period $k = -1$.

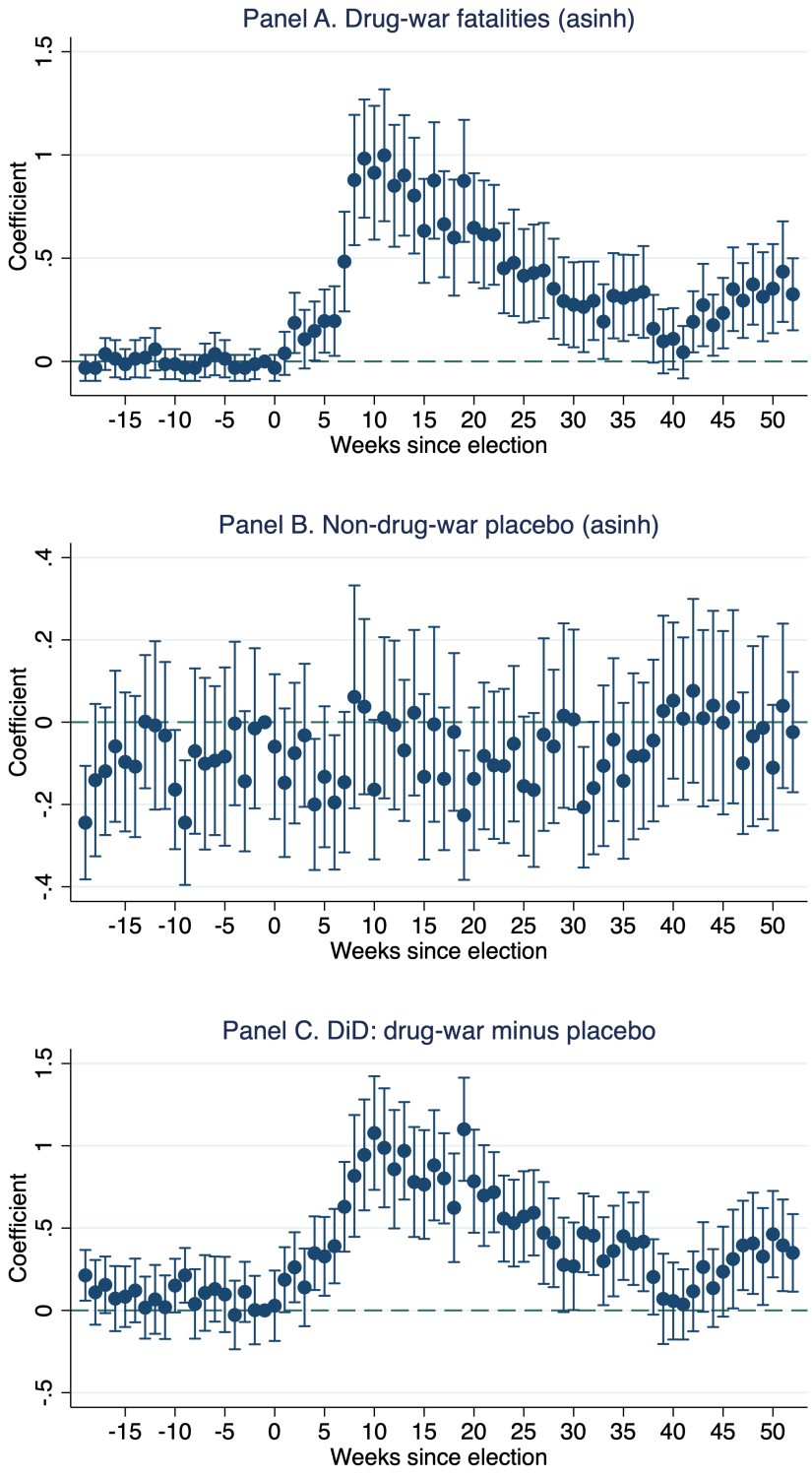

Figure 5: Preferred specification: election as event date (May 9, 2016). Three stacked panels produced by `xtevent+xteventplot` (Panel A: drug-war fatalities, asinh; Panel B: non-drug-war placebo, asinh; Panel C: DiD interaction, $\text{arcsinh}(\text{DW}) - \text{arcsinh}(\text{non-DW})$). Province FE; SE clustered at the province level.

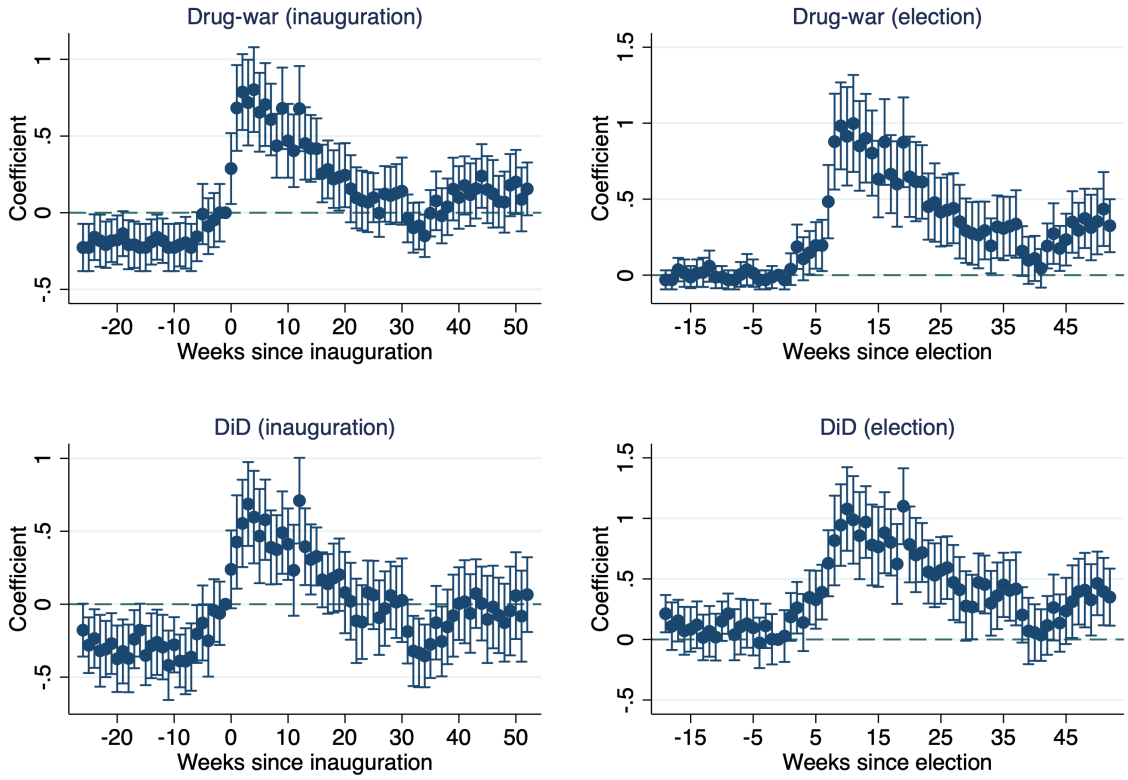

Figure 6: Comparison: inauguration vs. election as event date. Top row shows the drug-war asinh outcome; bottom row shows the DiD interaction. Left column uses the inauguration as the event date; right column uses the election. All four panels are estimated with `xtevent` and combined via `graph combine`.

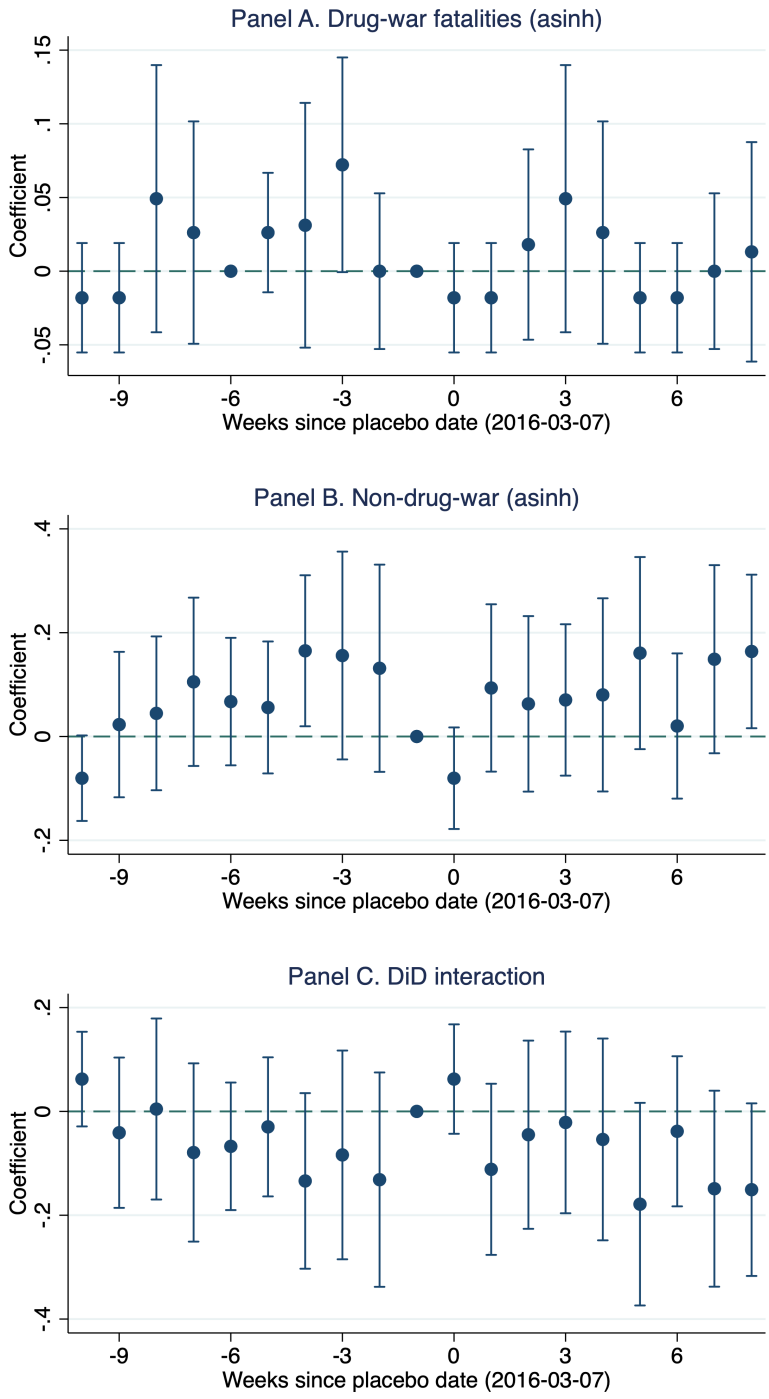

Figure 7: Placebo event study with fake event date of March 7, 2016. Top panel: drug-war fatalities (asinh). Middle panel: non-drug-war placebo (asinh). Bottom panel: DiD interaction. Panel is restricted to weeks on or before May 8, 2016.

A Additional figures

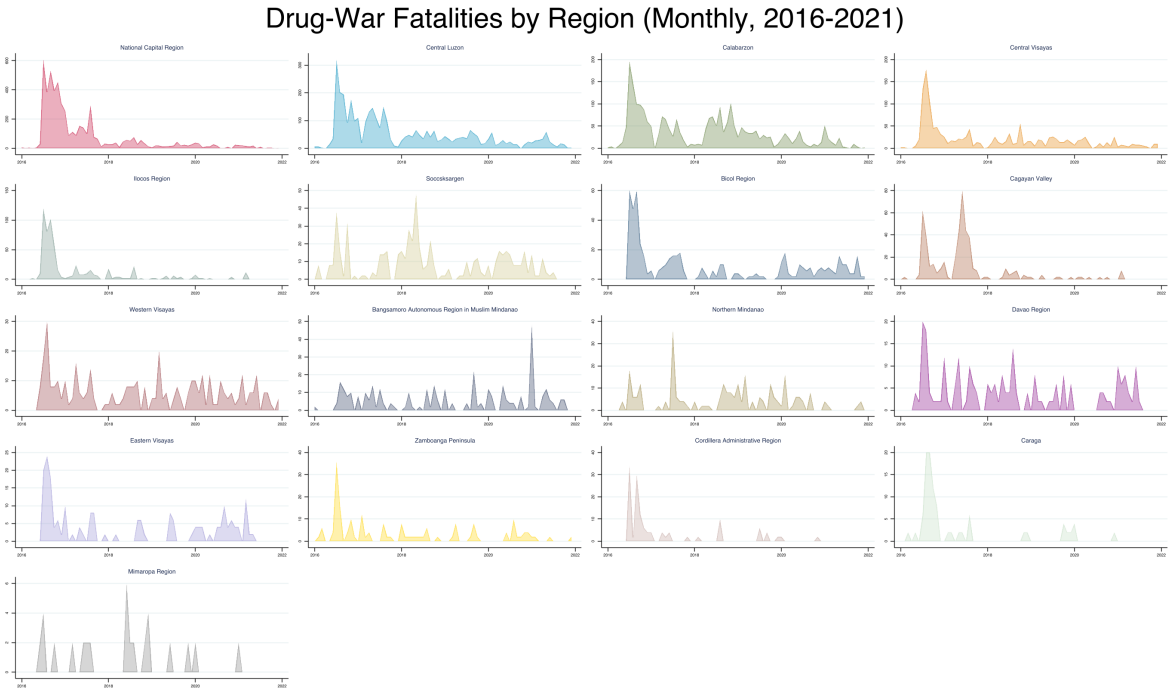

Figure 8: Drug-war fatalities by region: Small multiples (monthly, 2016–2021).

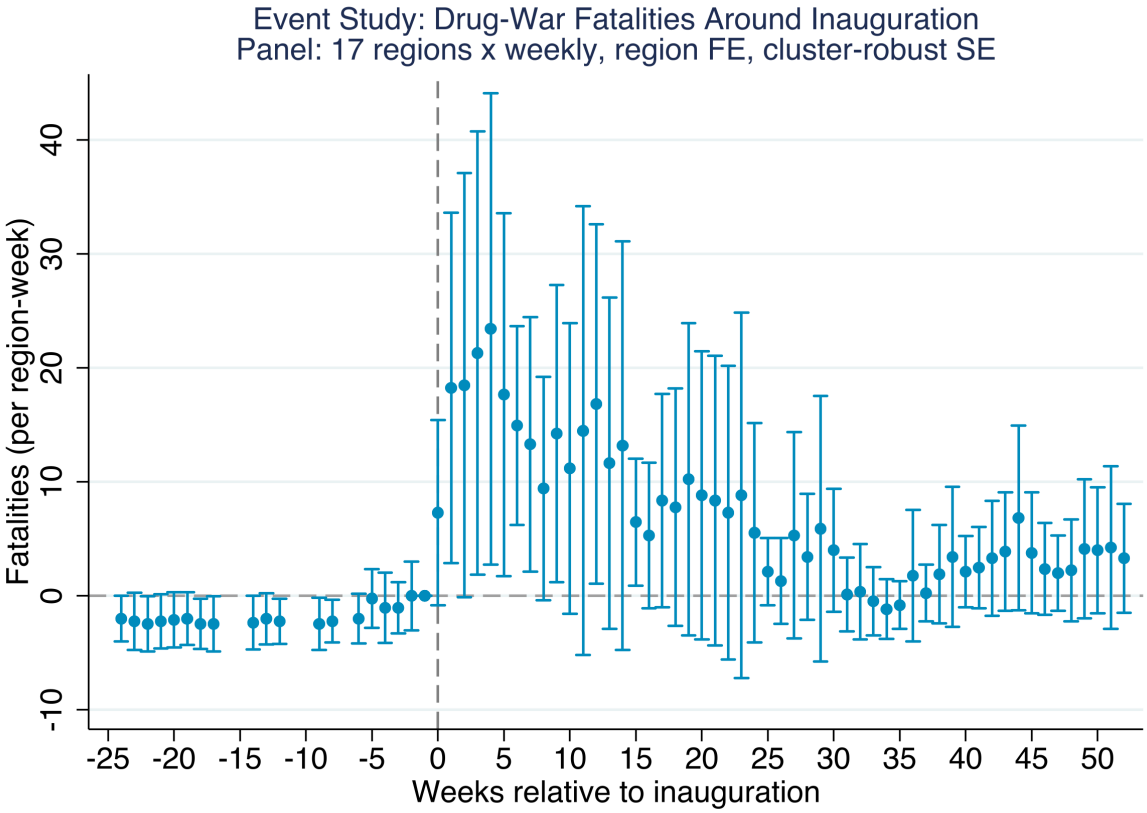

Figure 9: Event study (levels): Region-level panel, inauguration as event.

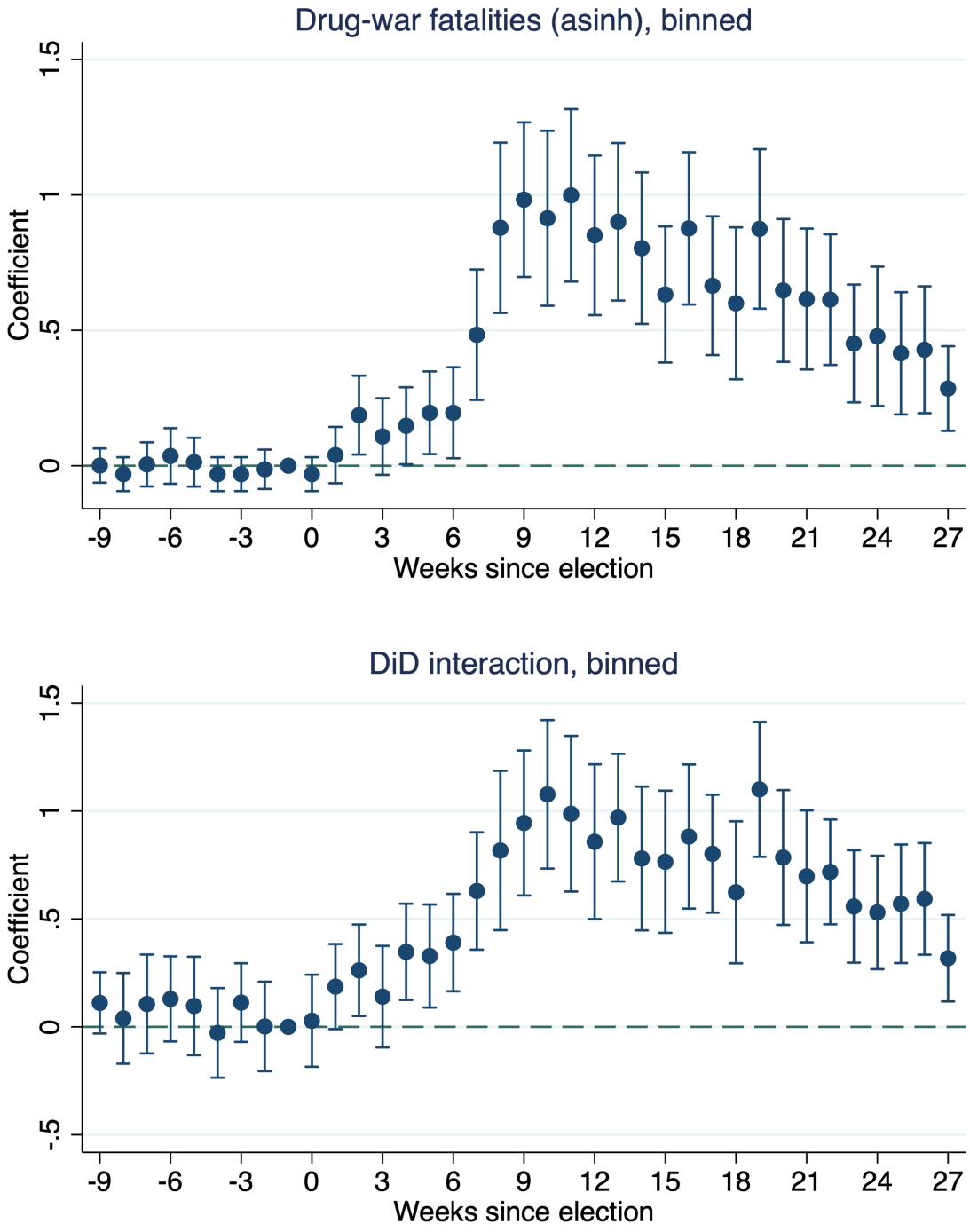

Figure 10: Event study with binned endpoints (election as event date). Top panel: drug-war asinh outcome. Bottom panel: DiD interaction. Both estimated via `xtevent` with `lwindow(8)` and `rwindow(26)`, which automatically bins observations beyond the window into the endpoint indicators.

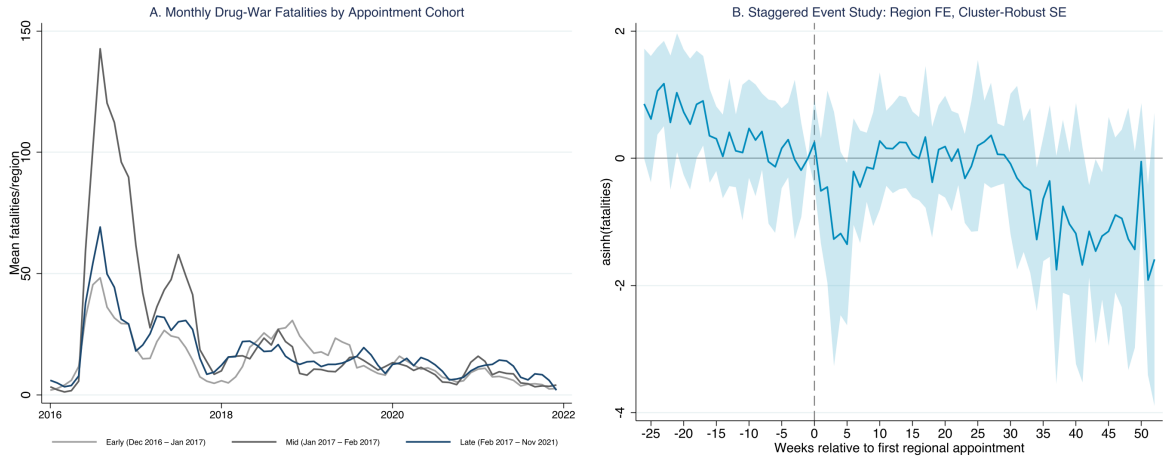

Figure 11: Personnel appointments and drug-war fatalities. Panel A plots average monthly drug-war fatalities by appointment cohort (3-month moving average). Panel B shows the staggered event study centered on each region's first appointment, with region fixed effects and cluster-robust standard errors (16 clusters). The violated pre-trend (8/25 CIs excluding zero, positive mean) and negative post-period coefficients are consistent with reverse causality.