

Firearm-specific suicide mortality across two decades of opposing firearm legislation: a controlled interrupted time-series analysis

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Abstract

Purpose: To estimate whether Brazil's 2003–2004 restrictive and 2019 permissive firearm-policy shifts were followed by opposite changes in firearm-specific suicide mortality relative to non-firearm suicide, and to assess method substitution and the prespecified subgroup aged 60 years or older.

Methods: We conducted a preregistered controlled interrupted time-series analysis of monthly Brazilian mortality data from January 2000 through December 2023. Firearm and non-firearm suicides were jointly modelled using negative-binomial regression with 12-month heteroskedasticity- and autocorrelation-consistent inference. Uncertainty from probabilistic redistribution of deaths of undetermined intent was propagated across 200 completed datasets. The primary estimand was the 36-month cumulative differential rate ratio (cDRR).

Results: After the restrictive shift, firearm suicide declined relative to non-firearm suicide (cDRR, 0.899; 95% CI, 0.834–0.970), corresponding to 401.1 fewer fitted than counterfactual deaths (95% CI, 107.9–694.3 fewer). After the permissive shift, the cDRR was 1.054 (95% CI, 0.991–1.122), corresponding to 164.6 excess deaths (95% CI, 25.1 fewer to 354.3 excess). The between-shift cDRR ratio was 1.172 (95% CI, 1.071–1.283). Both directional national tests passed preregistered gatekeeping, although the permissive-shift estimate was imprecise and power-limited. Non-firearm suicide did not move oppositely, and the pattern was not reproduced among adults aged 60 years or older.

Conclusion: Firearm-policy direction was associated with firearm-specific suicide in the hypothesised directions, more clearly after restriction than permissiveness. However, residual temporal structure, limited falsification specificity, and pandemic overlap constrain causal interpretation; directional symmetry was partially supported, not confirmed.

1. Introduction

Firearm access is consistently associated with suicide mortality across individual and population-level studies. Individual access and living in a firearm-owning household are associated with higher suicide risk [1, 2], while state-level ownership and less secure storage correlate with higher firearm and overall suicide rates [3, 4]. Cross-lagged evidence also indicates that higher ownership precedes subsequent firearm-suicide increases more strongly than the reverse association (Morral et al., 2024). These observational findings support an availability pathway, but evidence on policy effects is more mixed: some studies associate waiting periods, purchaser licensing, child access prevention laws, and certain background-check regimes with lower firearm suicide rates [5–7], whereas others find null or inconsistent associations for several laws, including some background-check policies [8, 9].

The evidence base is also geographically concentrated: 81% of 467 studies in a recent scoping review were conducted in the United States [10]. This matters particularly because method-specific suicide trends are not globally uniform. For instance, although the global age-standardised mortality rate for self-harm mortality declined by 36.4% between 2000 and 2023, rates in Latin America continued to climb

[11]. In this context, Brazil is analytically informative because it combines major, opposing firearm-policy shifts with substantial regional heterogeneity in both firearm mortality and suicide trends [12, 13]. The December 2003 Disarmament Statute restricted civilian firearm registration, possession, commercialisation, and carrying [14]. Firearm mortality subsequently declined, and states with higher firearm collection rates saw larger reductions in violent firearm deaths [15, 16]. However, firearm-specific suicide was already declining nationally during much of the 2000–2017 period, with marked variation across regions and methods, which limits simple causal readings of descriptive pre/post trends [11, 17].

A June 2019 decree package relaxed acquisition rules expanded access to previously restricted calibres, and raised acquisition limits, particularly for collectors, sport shooters, and hunters. Registered privately held firearms increased from 1.3 million in 2018 to 2.9 million in 2022 [18], but whether this reversal altered firearm-specific suicide beyond contemporaneous changes in other suicide methods remains an open empirical question. This question is especially important because overall suicide trends in Brazil have generally been stable or rising across many state-age-sex units, making method-specific analysis essential [19, 20].

We conducted a preregistered controlled interrupted time-series analysis of monthly Brazilian mortality from 2000 to 2023. We estimated 36-month firearm-specific differential changes after the 2003–04 restrictive shift (I1) and 2019 permissive shift (I2), using non-firearm suicide as a within-population comparator. We hypothesised a reduction after I1 and an increase after I2, tested their opposite-direction contrast, evaluated method substitution and total suicide, and examined adults aged 60 years or older as the only prespecified confirmatory subgroup. Geographic, sex, and age heterogeneity and temporal specificity were exploratory.

2. Methods

2.1. Design, data, and registration

This population-level controlled interrupted time-series study used monthly deaths in Brazil from January 2000 through December 2023. De-identified records came from the Mortality Information System (SIM/DATASUS) and were aggregated by month, federative unit, sex, age group, and mechanism. Firearm suicide comprised ICD-10 X72–X74; non-firearm suicide comprised X60–X84 excluding X72–X74; and total suicide was their sum. Firearm and non-firearm series were stacked for the controlled analysis. Population denominators came from the 2024 revision of Brazilian Institute of Geography and Statistics projections, interpolated linearly from annual 1-July estimates to monthly midpoints and entered as a log offset.

The protocol was registered on 26 June 2026 before outcome modelling (<https://osf.io/k82j5/overview>). A pre-outcome amendment on 11 July 2026 (<https://osf.io/x7f8k/overview>) replaced deterministic fractional redistribution with stochastic integer completion across 200 datasets because the preregistered negative-binomial likelihood required integer counts. Dates, estimands, multiplicity rules,

and core sensitivities were unchanged. The study used public, de-identified data; completed reporting checklists for routinely collected health data and interrupted time-series studies are provided in the supplement.

Only the first author accessed record-level SIM files. A scripted, audited pipeline verified source-file coverage, required a valid month and federative unit of residence, aggregated underlying-cause records to prespecified monthly cells, and reconciled each completed dataset to national totals. Supplementary Tables S1 and S2 provide the record-selection cascade, data-quality exclusions, ICD-10 coding, and variable definitions. No person-level or institution-level linkage was performed.

2.2. Redistribution of deaths of undetermined intent

The primary outcome redistributed eligible deaths of undetermined intent (Y10–Y34) using the prespecified S4 mechanism-specific algorithm; Y87.0, Y87.2, R99, and other R-coded deaths were not redistributed. Within annual strata, the probability that an undetermined death represented suicide was estimated from deaths of known intent. A fixed hierarchy selected the most specific stratum with at least 20 known-intent deaths, progressing from year × federative unit × sex × age × mechanism to pooled national period × mechanism strata; a parallel hierarchy without mechanism was used when mechanism was uninformative (Supplementary Table S3).

For each of 200 completed datasets, one probability was drawn per selected annual stratum and shared across its monthly cells; cell allocations were then drawn from binomial distributions. Y22–Y24 allocations contributed to firearm suicide and other Y10–Y34 allocations to non-firearm suicide. Completed counts were non-negative integers and preserved firearm plus non-firearm suicide equal to total suicide. Deterministic expected-S4 and raw X-coded series were retained as prespecified sensitivities.

2.3. Primary model and estimands

I1 was modelled from January 2004, following Lei n° 10.826, and I2 from July 2019, the first full month after Decretos n° 9.844–9.847. Each interruption had step and post interruption ramp terms. The model also included linear calendar time, two series-specific Fourier harmonic pairs, a common COVID-19 step from March 2020, and a March 2020 pulse. Baseline level and trend, policy terms, and seasonality could differ between firearm and non-firearm suicide. The August 2023 restrictive reversal was evaluated only in a five-month exploratory sensitivity because follow-up ended in December 2023.

A negative-binomial log-link model was fitted to each completed dataset. Firearm-by-step and firearm-by-ramp interactions estimated firearm-specific changes beyond contemporaneous non-firearm changes. Inference used calendar-month score-based Newey–West covariance with Bartlett weights and a prespecified 12-month lag. The primary estimand was the 36-month cumulative differential rate ratio (cDRR): predicted firearm deaths divided by predictions from a counterfactual setting only the indexed interruption's firearm interactions to zero. Accordingly, cDRRs estimate mechanism-specific differential change, not the total policy effect on suicide.

Within each imputation, 10,000 coefficient draws propagated model uncertainty to cDRRs, fitted and counterfactual counts, and their differences. Log-cDRRs and count translations were pooled across datasets using Rubin's rules, including within- and between-imputation uncertainty. Ninety-five per cent confidence intervals were calculated on the log scale for ratios.

2.4. Confirmatory and secondary analyses

Confirmatory inference used preregistered fixed-sequence gatekeeping. A two-sided test first compared I2 with I1. Only after rejection were one-sided national tests of $cDRR(I1) < 1$ and $cDRR(I2) > 1$ evaluated with Holm adjustment. The ≥ 60 -year contrast and directional tests followed in their eligible branches. Single-series, level-and-slope, substitution, heterogeneity, sensitivity, and falsification analyses were secondary or diagnostic.

The ≥ 60 -year analysis replayed the frozen S4 stream and primary model. Separate single-series models for firearm, non-firearm, and total suicide estimated 36-month cumulative rate ratios to examine substitution and net change. A pre-outcome simulation calibrated on January 2000–December 2003 estimated the cDRR required for 80% directional power. After outcome analysis, the simulation was corrected because its original Newey–West lags followed stacked row order rather than calendar month; this disclosed correction did not affect the outcome model.

Diagnostics assessed residual dispersion, influence, and serial dependence. Prespecified sensitivities varied outcome construction, seasonality, HAC lag, cumulative window, population interpolation, COVID handling, intervention dates and wash-in, pre-I1 curvature, and early regional data coverage. Generalised least-squares models with AR(1), AR(2), or ARMA(1,1) residual structures evaluated alternative autocorrelation assumptions. Temporal and geographic influence was assessed by leaving out each year or macroregion. Twenty fixed-seed false dates tested temporal specificity; firearm assault/homicide and land-transport mortality served as positive and negative comparator outcomes, respectively. Full specifications are provided in the preregistration and supplement.

The raw-count and deterministic-S4 analyses tested sensitivity to intent classification. Window analyses used 24 and 48 months; COVID analyses removed the March 2020 pulse or excluded March 2020–December 2021 outcomes; timing analyses varied legal dates, shifted interruptions by ± 3 months, excluded intervention months, and modelled I2 without an immediate step. Population analyses varied interpolation and early-2000 handling. The August 2023 reversal was added to the national model with step and ramp terms and evaluated over the five observed months only.

2.5. Heterogeneity and reproducibility

Exploratory models were fitted by sex, 20 age groups, five macroregions, and 27 federative units using the primary specification. Strata were pooled only when all 200 imputation-specific fits were valid; estimates were reported with two-sided confidence intervals without multiplicity adjustment. All analyses were scripted in R 4.5.2. Seeds, manifests, convergence checks, quality-control summaries, and

immutable SHA-256 inventories were frozen in analysis release v2 on 16 July 2026. Publication tables and figures were assembled from frozen outputs without refitting.

Sex analyses excluded 80 records with unknown sex and age analyses excluded 1,224 records with unknown age; geographic totals retained all source cells. Completed stratum grids included explicit zero counts and had to aggregate to the corresponding national totals. The analysis used MASS 7.3–65 and a bespoke audited calendar-score HAC implementation. Separate manifests documented the false-date, August 2023, counterfactual-count, sensitivity, and heterogeneity workflows.

The 20 age groups were 0–4, 5–9, 10–14, 15–19, 20–24, 25–29, 30–34, 35–39, 40–44, 45–49, 50–54, 55–59, 60–64, 65–69, 70–74, 75–79, 80–84, 85–89, and 90 years or older; the two terminal source groups (90–94 and ≥ 95 years) were combined to match the population denominators.

3. Results

3.1. Analytic series

All 648 federative-unit–year files passed structural audit. Among 29,062,440 all-cause records, 499 (0.0017%) with missing or invalid month were excluded. The national series comprised 288 months and 257,843 raw X-coded suicides (26,296 firearm; 231,547 non-firearm). The S4-eligible pool comprised 281,312 Y10–Y34 deaths; unspecified mechanisms accounted for 55.0%, whereas hanging and poisoning together contributed 55.7% of expected reallocations.

No record had a missing federative unit of residence or a date–year mismatch. Unknown sex and age were retained in national and geographic analyses but excluded only from the corresponding stratified analyses (80 and 1,224 raw records, respectively); no primary national analysis used person-level complete-case exclusion.

Across 200 completed datasets, mean cumulative suicides numbered 296,340.5 (27,349.1 firearm; 268,991.4 non-firearm). Redistribution added a mean 38,497.5 suicides, increasing firearm counts by 4.0% and non-firearm counts by 16.2%. All count and identity audits passed. Annual counts and rates are shown in Table 1 and monthly policy-era trends in Figure 1.

3.2. Primary national analysis

The 36-month cDRR after I1 was 0.899 (95% CI, 0.834–0.970), a 10.1% firearm-specific differential reduction. After I2, the cDRR was 1.054 (95% CI, 0.991–1.122), a 5.4% differential increase whose two-sided interval included 1. The I2-to-I1 ratio was 1.172 (95% CI, 1.071–1.283; $P = .0006$). I1 was characterised by an immediate level reduction (RR, 0.893; 95% CI, 0.840–0.951) without a clear slope change; I2 had no clear level change but a monthly slope increase (RR, 1.0033; 95% CI, 1.0020–1.0046) (Supplementary Table S4; Figure 2A).

Over 36 months, the model estimated 3,587.4 fitted versus 3,988.6 counterfactual firearm-suicide deaths after I1 (difference, -401.1; 95% CI, -694.3 to -107.9) and 3,199.9 versus 3,035.3 after I2 (difference, 164.6; 95% CI, -25.1 to 354.3) (Figure 2B–C).

Both national directions passed fixed-sequence gatekeeping: Holm-adjusted $P = .006$ for $cDRR(I1) < 1$ and $P = .048$ for $cDRR(I2) > 1$. The I2 evidence was less precise on the conventional two-sided scale.

3.3. Confirmatory subgroup and secondary outcomes

Among adults aged 60 years or older, the pattern was not reproduced: I1 $cDRR$, 1.072 (95% CI, 0.822–1.398); I2 $cDRR$, 0.972 (95% CI, 0.897–1.054); and ratio, 0.907 (95% CI, 0.696–1.181; $P = .468$).

Directional subgroup tests were also null, with point estimates opposite the hypotheses (Figure 2A).

In secondary single-series models, I1 cumulative rate ratios were 0.878 (95% CI, 0.807–0.956) for firearm, 0.976 (95% CI, 0.910–1.048) for non-firearm, and 0.963 (95% CI, 0.900–1.030) for total suicide. Corresponding I2 estimates were 1.264 (95% CI, 1.139–1.402), 1.164 (95% CI, 1.111–1.220), and 1.179 (95% CI, 1.120–1.240). Non-firearm suicide did not move oppositely at either interruption (Figure 3; Supplementary Table S5).

3.4. Robustness, falsification, and diagnostics

Point estimates retained the prespecified directions across core sensitivities (Figure 4A). Raw X-coded $cDRRs$ were 0.898 (95% CI, 0.834–0.966) for I1 and 1.080 (95% CI, 1.015–1.148) for I2; deterministic expected-S4 estimates were 0.900 (95% CI, 0.841–0.965) and 1.058 (95% CI, 0.997–1.123). Alternative seasonality, HAC lags, cumulative windows, COVID specifications, and population interpolation yielded I1 $cDRRs$ of 0.897–0.902 and I2 $cDRRs$ of 1.033–1.076.

Across timing and wash-in variants, I1 $cDRRs$ ranged from 0.887 to 0.913, I2 $cDRRs$ from 1.042 to 1.069, and contrast ratios from 1.151 to 1.188; the I2 two-sided interval included 1 in five of seven variants. Excluding North and Northeast data before 2006 strengthened I1. Preperiod curvature was detected ($P = .0077$), and a nonlinear-trend model produced I1 $cDRR$ 0.755 (95% CI, 0.614–0.929), indicating specification-dependent magnitude. Leave-one-region and leave-one-year analyses preserved point-estimate directions (Figure 4B; Supplementary Figure S1).

Temporal and comparator specificity were limited. Sixteen of 20 false-date joint tests remained significant after Holm adjustment. Firearm assault/homicide declined after I2, opposite its prespecified positive-comparator direction, while land-transport mortality also changed (Figure 4C–D). Residual serial dependence persisted in all primary fits. $AR(2)$ estimates retained the national directions, but the I2-to-I1 ratio was imprecise (1.144; 95% CI, 0.940–1.392), showing that interval inference depended on the residual-correlation specification. The corrected 80%-power threshold was 0.900 for a constant I1 effect and 1.075 for I2; the observed I2 estimate was therefore power-limited.

Across leave-one-region analyses, I1 cDRRs ranged from 0.844 to 0.941 and I2 cDRRs from 1.049 to 1.067; leave-one-year ranges were 0.880–0.952 and 1.037–1.076. For firearm assault/homicide, cumulative rate ratios were 0.859 (95% CI, 0.803–0.917) after I1 and 0.676 (95% CI, 0.609–0.751) after I2. Land-transport estimates were 1.053 (95% CI, 0.978–1.135) and 0.867 (95% CI, 0.766–0.982), respectively. Primary fits showed only mild overdispersion, but Breusch–Godfrey-style and Ljung–Box tests rejected in every imputation for both series.

3.5. Exploratory analyses

Men more closely resembled the national pattern than women. I2-to-I1 intervals excluded 1 in Central-West and Southeast, but not in the other macroregions (Figure 5; Supplementary Table S6). Only 29 of 54 granular strata had all 200 valid fits, limiting age- and state-specific interpretation; all such analyses were multiplicity-unadjusted. For the August 2023 restrictive reversal, the exploratory five-month cDRR was 1.056 (95% CI, 0.991–1.126), providing insufficient follow-up to characterise the subsequent trajectory (Supplementary Table S7).

Among men, cDRRs were 0.878 (95% CI, 0.814–0.947) after I1 and 1.059 (95% CI, 0.994–1.128) after I2; among women, they were 1.016 (95% CI, 0.835–1.235) and 1.058 (95% CI, 0.908–1.233). The I2-to-I1 interval excluded 1 in Central-West (ratio, 1.600; 95% CI, 1.245–2.057) and Southeast (1.255; 95% CI, 1.115–1.413).

Only 8 of 20 age groups and 14 of 27 states pooled across every imputation. Among pooled age groups, I2 intervals excluded 1 at ages 15–19, 30–34, and 60–64 years, and the I2-to-I1 interval excluded 1 only at ages 15–19 years. Because model completion was selective and no multiplicity adjustment was applied, these estimates were treated as hypothesis-generating.

4. Discussion

4.1. Main findings

The primary model estimated a 10.1% firearm-specific differential reduction over 36 months after the 2003–04 restrictive shift and a smaller, less precise 5.4% increase after the 2019 permissive shift. This pattern is directionally consistent with a broader literature in which stricter firearm regulations are generally associated with modest reductions in firearm-specific suicide, while causal certainty remains limited because much of the evidence is ecological and methodologically heterogeneous [21, 22]. The between-shift contrast and both one-sided national tests passed preregistered gatekeeping which strengthens the internal coherence of the result, but not its causal interpretation. However, non-firearm suicide did not move oppositely, and the ≥ 60 -year confirmatory subgroup did not reproduce the pattern, so that the findings might be more consistent with a method-specific association than with a generalized suicide effect and should be interpreted cautiously given the mixed evidence on substitution and overall suicide impacts [23, 24].

These cDRRs compare firearm with non-firearm suicide and should not be read as total policy effects. The national directions are consistent with an availability pathway and with observational evidence linking firearm access and permissive environments to suicide [1, 2, 25, 26], but the Brazilian policy effects are not identified by those studies. Within this analysis, pre-I1 curvature, persistent residual autocorrelation, frequent false-date rejection, and an inconsistent positive comparator restrict the gatekeeping result to internal consistency with the prespecified model.

The I1 direction is coherent with Brazil's documented decline in firearm mortality and firearm-suicide share after 2003, but joinpoint and age–period–cohort analyses show that firearm-suicide rates were already falling beforehand [11, 15, 17]. Conversely, the I2 period combined rapid firearm accumulation with the pandemic and a continuing rise in non-firearm suicide, making attribution to policy alone difficult because broader suicide trends in Brazil were already worsening and remained heterogeneous across regions and groups [19, 20, 27]. The within-country opposition of policy directions strengthens temporal comparison relative to a single interruption, but it does not remove time-varying confounding.

4.2. Interpretation of the policy shifts

The I1 reduction was smaller than the uncontrolled decline previously reported for firearm suicide [11], consistent with much of the raw decline reflecting forces shared across suicide methods. This interpretation is broadly consistent with the wider literature linking reduced firearm access to lower firearm suicide, while also showing that estimated effects vary by policy design and study specification [21, 28]. It appeared mainly as an immediate level change and corresponded to approximately 401 fewer fitted than counterfactual deaths. Direction was stable across sensitivities, but magnitude changed materially under nonlinear baseline trends, which argues for interpreting the result as suggestive rather than point-precise [29]. The January 2004 change is temporally compatible with Lei nº 10.826 but precedes the later national buyback and cannot be uniquely attributed to either intervention.

I2 corresponded to approximately 165 more fitted than counterfactual deaths, but its count and two-sided cDRR intervals included the null. The estimate was power-limited and less stable than I1: raw X-coded data produced a larger estimate whose interval excluded 1, whereas S4 redistribution attenuated it. Its positive slope rather than level change is compatible with gradually accumulating exposure as firearm registrations increased, a mechanism that is at least directionally consistent with evidence that greater firearm availability is associated with higher suicide risk [30]. At the same time, the 36-month window was dominated by the COVID-19 pandemic, and pandemic-period firearm violence shifted sharply in ways that were heterogeneous across place and time [31, 32]. Common pandemic indicators cannot absorb a firearm-specific pandemic effect, and firearm policy evaluations are especially vulnerable to residual confounding when concurrent shocks and co-occurring policies are incompletely modeled [33, 34]. The permissive-shift hypothesis is therefore compatible with the primary model but remains vulnerable to coincident shocks, secular change, and residual confounding.

4.3. Method substitution and confirmatory subgroup

The absence of opposite movement in non-firearm suicide provides only limited evidence against method substitution, because partial substitution remains possible in the presence of rising background suicide risk. That said, the broader means-restriction literature generally finds that substitution, when it occurs, is incomplete rather than one-for-one [35, 36], and several firearm-policy studies similarly report declines in firearm suicide without corresponding increases in non-firearm suicide [37, 38]. After I2, firearm, non-firearm, and total suicide all increased in secondary models, a pattern more consistent with shared secular drivers than with a simple substitution account [39, 40]. The ≥ 60 -year subgroup produced imprecise estimates opposite the hypothesised directions. This may reflect limited mechanism-sensitive deaths or weaker effects of acquisition restrictions among cohorts with legacy firearm stocks. Exploratory age-specific signals were incomplete and multiplicity-unadjusted and do not test these explanations.

The older-adult rationale was drawn partly from high-income settings where firearm suicide is concentrated among older men [41–43], whereas Brazilian evidence places the male peak at younger ages [44]. This comparison should therefore be treated as a question of external validity rather than as a directly transferable age pattern. Older cohorts may also hold firearms acquired before either interruption, which could attenuate short-run policy sensitivity, although the present analysis does not directly measure legacy stock. The exploratory increase among adolescents is directionally compatible with a legacy-stock account but given that adolescent suicide risk is also shaped by firearm availability in the home [45, 46] and recent U.S. data show rising firearm-suicide trajectories in mid-adolescence [47], this subgroup result remains hypothesis-generating rather than confirmatory.

4.4. Strengths and limitations

Strengths include prospectively specified dates and estimands, preregistered gatekeeping, a transparent pre-outcome amendment, joint modelling with a within-population comparator, a fully specified redistribution hierarchy, uncertainty propagation across 200 datasets, frozen seeds and manifests, disclosure of the post-unblinding simulation correction, and extensive robustness and falsification analyses.

The ecological design precludes individual-level inference about firearm access, storage, or intent. More importantly, residual serial correlation and widespread false-date rejection indicate pervasive temporal structure rather than specificity to 2004 and 2019. The AR(2) contrast interval included 1, so confirmatory inference depends on the residual-correlation model. I1 magnitude depended on baseline-trend specification; I2 was underpowered and overlapped the pandemic. Firearm assault/homicide moved opposite the prespecified I2 comparator direction, and the ICD-based classification algorithms lacked study-specific validation. Taken together, these limitations argue against interpreting the estimates as isolated causal effects and support placing greatest weight on I1 direction, least on I2 magnitude, and intermediate, model-contingent weight on the contrast.

5. Conclusions

Brazilian firearm-policy direction was associated with firearm-specific suicide in the hypothesised directions, but asymmetrically and with uneven evidential strength. Firearm suicide declined relative to non-firearm suicide after the 2003–04 restriction and increased modestly after the 2019 permissive shift; the latter estimate was imprecise, underpowered, and overlapped the pandemic. The between-shift contrast was present in the primary model but not robust to an alternative residual-correlation specification. Non-firearm suicide did not move oppositely, and the confirmatory older-adult subgroup failed.

The results partially support, rather than confirm, directional symmetry. Continued surveillance should report both raw and redistributed outcomes because handling of undetermined intent affected I2. Accumulating follow-up after the August 2023 restrictive reversal will permit a prospectively specified within-country replication, while individual-level linkage of firearm acquisition and mortality records is needed to address ecological confounding.

For surveillance, the divergence between raw-count and redistributed I2 estimates supports presenting both series. For practice, the population-level analysis does not estimate individual benefit, but the broader literature identifies firearm access and storage as modifiable correlates relevant to means-safety assessment [1, 2, 48, 49]. For research, the post-2023 period offers a third prospectively specifiable policy interruption and an opportunity to test whether the national directional pattern replicates.

Declarations

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Competing interests

ICP has served as consultant, adviser, or speaker for Johnson & Johnson, Daiichi Sankyo, EMS, and Aché. ICP receives authorship royalties from Springer Nature and Artmed. BdSS declares no competing interests.

Data availability

The complete R analytical pipeline utilised for data preprocessing, along with every figure and table, is openly available on Open Science Framework (<https://osf.io/x3tdy>). Requests for access to data not included in the article, Supplementary Materials, or OSF page should be directed to the corresponding author.

Declaration of generative AI and AI-assisted technologies in the manuscript preparation process

During the preparation of this work, the authors used Anthropic Claude and OpenAI ChatGPT to support language editing, readability, and code documentation. After using these tools, the authors reviewed and edited the content as needed and take full responsibility for the published article.

Ethics statement

This study used publicly available, de-identified administrative mortality data from SIM/DATASUS and aggregate GBD estimates. No participants were recruited or contacted, and no individual-level identifiable data were accessed. Institutional ethics review was not required under applicable regulations for analyses of publicly available de-identified administrative data.

Author contributions

BdSS: Conceptualization, Data Curation, Formal Analysis, Investigation, Software, Visualization, Writing – original draft, Writing – review & editing. ICP: Funding Acquisition, Supervision, Validation, Writing – review & editing.

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Table

Table 1. Annual firearm-, non-firearm-, and total-suicide mortality in Brazil, 2000–2023				
Year	Firearm suicide, mean n (rate per 100,000)	Non-firearm suicide, mean n (rate per 100,000)	Total suicide, mean n (rate per 100,000)	Firearm share, %
2000	1,425.4 (0.82)	6,685.4 (3.83)	8,110.7 (4.64)	17.6
2001	1,479.1 (0.84)	7,610.9 (4.30)	9,090.1 (5.14)	16.3
2002	1,432.1 (0.80)	7,714.8 (4.30)	9,146.9 (5.10)	15.7
2003	1,382.5 (0.76)	7,720.6 (4.26)	9,103.1 (5.02)	15.2
2004	1,297.1 (0.71)	8,066.8 (4.40)	9,363.9 (5.10)	13.9
2005	1,267.5 (0.68)	8,770.5 (4.73)	10,038.0 (5.41)	12.6
2006	1,182.3 (0.63)	8,758.3 (4.67)	9,940.6 (5.30)	11.9
2007	1,172.1 (0.62)	9,024.8 (4.76)	10,196.9 (5.38)	11.5
2008	1,159.0 (0.61)	9,622.7 (5.03)	10,781.7 (5.64)	10.7
2009	1,104.1 (0.57)	9,808.5 (5.08)	10,912.7 (5.65)	10.1
2010	993.5 (0.51)	9,821.0 (5.04)	10,814.5 (5.55)	9.2
2011	939.0 (0.48)	10,332.0 (5.27)	11,270.9 (5.75)	8.3
2012	1,015.5 (0.51)	10,698.5 (5.41)	11,714.0 (5.93)	8.7
2013	1,063.4 (0.53)	10,807.5 (5.42)	11,870.9 (5.96)	9.0
2014	998.6 (0.50)	10,964.1 (5.46)	11,962.7 (5.96)	8.3
2015	928.8 (0.46)	11,562.7 (5.71)	12,491.5 (6.17)	7.4
2016	979.2 (0.48)	11,825.2 (5.80)	12,804.4 (6.28)	7.6
2017	988.2 (0.48)	12,955.9 (6.31)	13,944.0 (6.80)	7.1
2018	1,035.0 (0.50)	13,510.1 (6.54)	14,545.1 (7.04)	7.1
2019	1,038.7 (0.50)	14,892.5 (7.16)	15,931.2 (7.66)	6.5
2020	1,027.4 (0.49)	15,131.7 (7.24)	16,159.1 (7.73)	6.4
2021	1,103.2 (0.53)	16,723.4 (7.96)	17,826.6 (8.49)	6.2
2022	1,143.2 (0.54)	17,817.4 (8.45)	18,960.6 (8.99)	6.0
2023	1,194.5 (0.56)	18,166.2 (8.58)	19,360.7 (9.15)	6.2
Total, 2000–2023	27,349.1 (–)	268,991.4 (–)	296,340.5 (–)	9.2

Legend. Annual counts are means across 200 datasets completed using the prespecified stochastic S4 redistribution of eligible deaths of undetermined intent (Y10–Y34). Counts were summed across the 12 months within each completed dataset and then averaged across imputations; consequently, means may be non-integer. Rates were calculated using the mean of the 12 corresponding monthly all-age, both-sex national population estimates from the 2024 revision of the Brazilian Institute of Geography and Statistics population projections. Total suicide equals firearm plus non-firearm suicide, and firearm share was calculated as firearm suicide divided by total suicide. Values are descriptive. The restrictive-policy interruption was January 2004. The permissive-policy interruption was July 2019; therefore, 2019 includes months before and after the interruption. The restrictive reversal occurred in August 2023; therefore, 2023 includes months before and after the reversal.

Figures

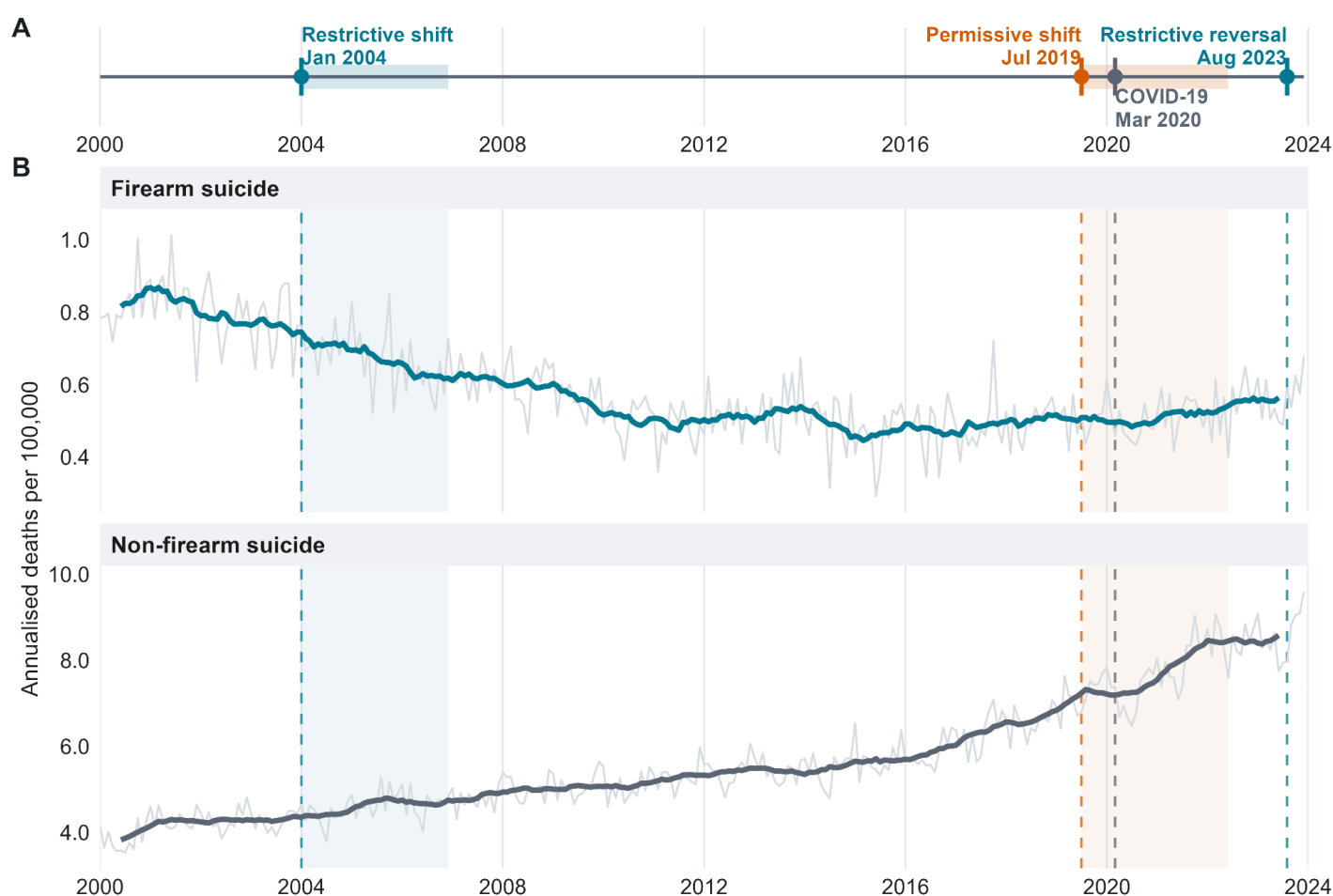


Figure 1

National firearm- and non-firearm-suicide rates across firearm-policy eras, Brazil, 2000–2023

Legend. Monthly national suicide mortality rates per 100,000 population from SIM/DATASUS after S4 completion of eligible Y10–Y34 deaths. Thin lines show the mean rate across 200 completed

datasets; darker lines show centred 12-month moving means. Vertical markers denote the January 2004 restrictive shift, July 2019 permissive shift, and August 2023 restrictive reversal; background shading distinguishes the policy eras, and pale bands mark the prespecified 36-month cDRR windows after I1 and I2. No 36-month band is shown after August 2023 because follow-up ended in December 2023. Firearm and non-firearm panels use independent y-axes because their absolute rates differ. Population denominators are from IBGE 2024 projections. cDRR indicates cumulative differential rate ratio; I1, first interruption; I2, second interruption; SIM, Sistema de Informações sobre Mortalidade.

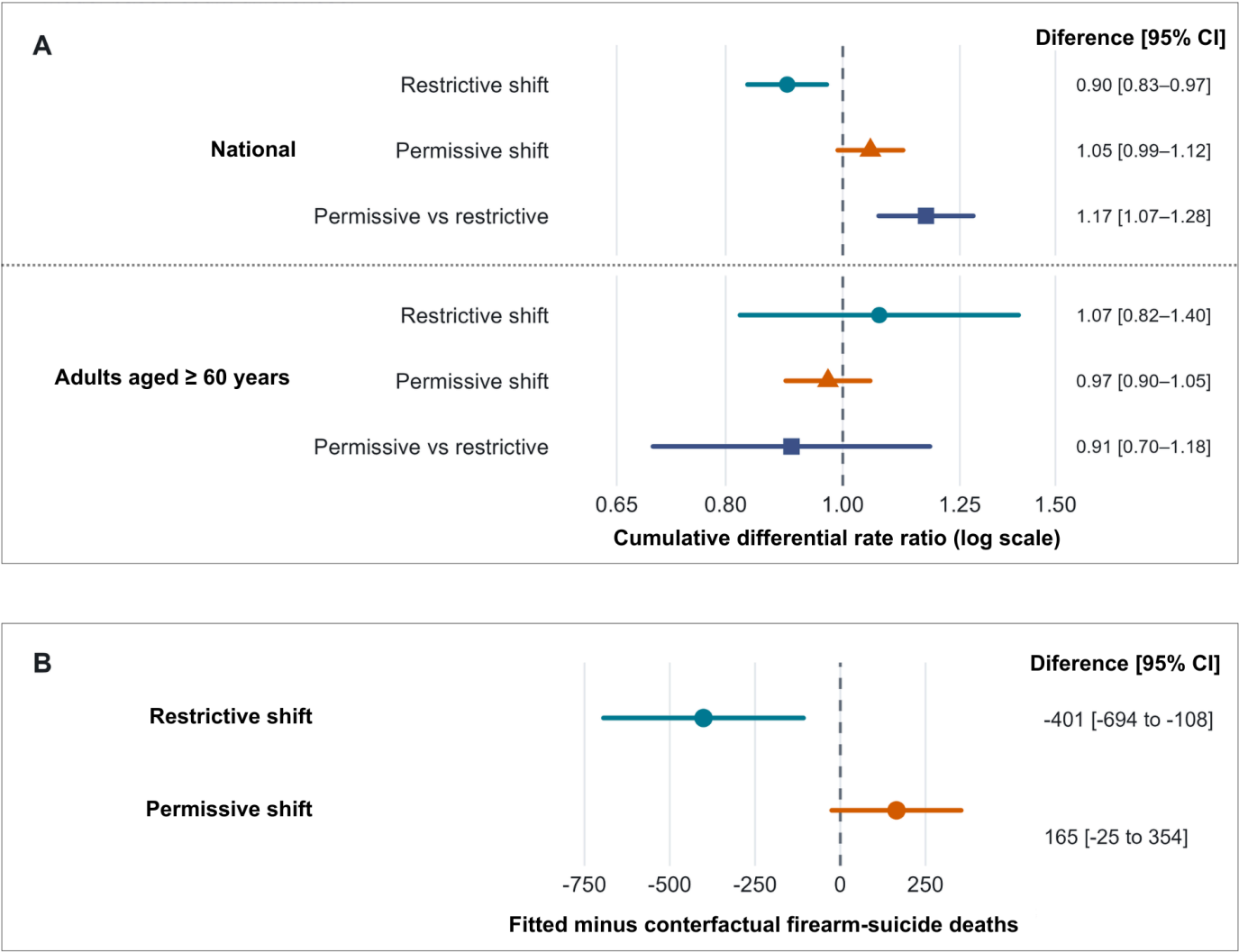


Figure 2

Primary 36-month controlled interrupted time-series estimates and counterfactual count translation

Legend. Panel A shows Rubin-pooled national and age-60-years-or-older cDRRs for I1, I2, and the I2-to-I1 ratio across 200 completed datasets. Panels B and C show model-based fitted and interruption-specific counterfactual cumulative firearm-suicide deaths and their fitted-minus-counterfactual differences over the corresponding 36-month windows. Points denote pooled estimates; error bars are two-sided 95% CIs. The null reference is a rate ratio of 1 in panel A and a count difference of 0 in panels

B and C. cDRR indicates cumulative differential rate ratio; CI, confidence interval; I1, January 2004 interruption; I2, July 2019 interruption; ITS, interrupted time series.

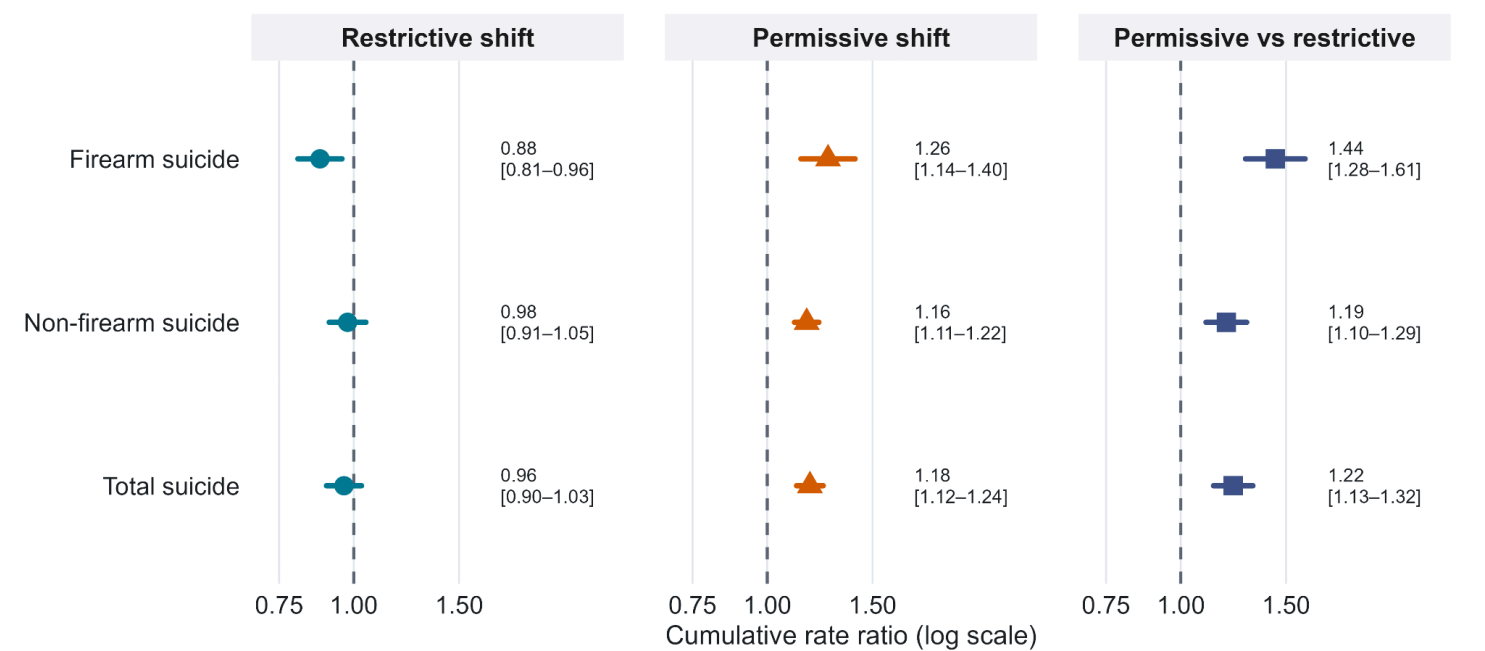


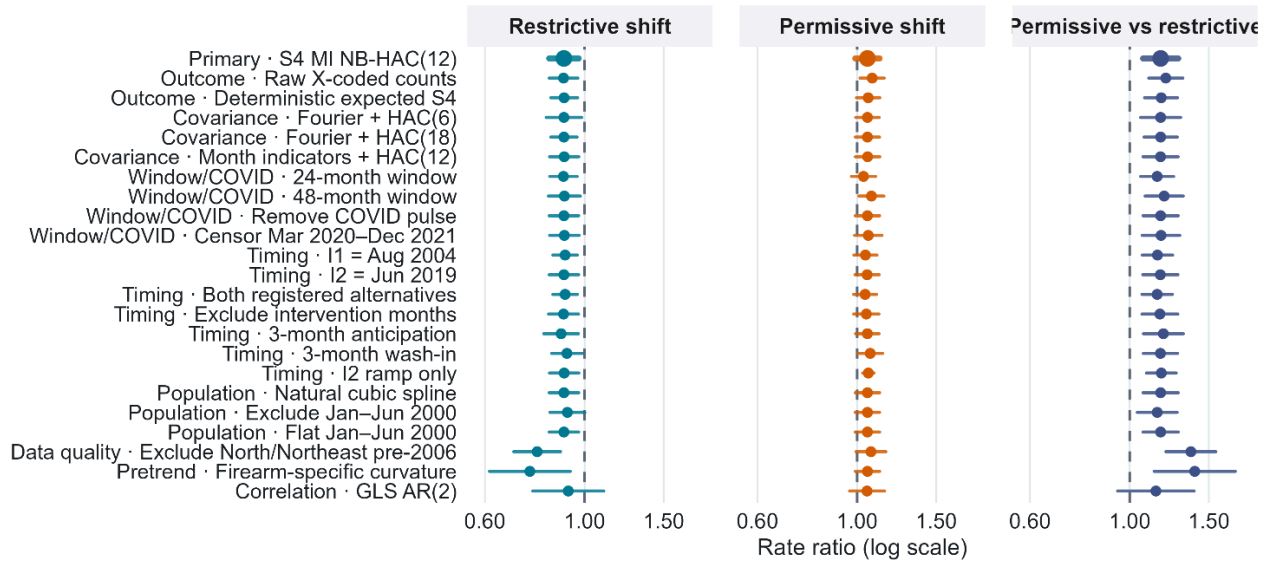
Figure 3

Secondary 36-month cumulative rate ratios by suicide outcome

Legend. Single-series estimates for firearm, non-firearm, and total suicide; these are secondary outcome-specific estimates rather than controlled primary effects. Points and horizontal ranges are Rubin-pooled estimates and two-sided 95% CIs across 200 completed datasets. The dashed reference line denotes a rate ratio of 1. The final column shows the ratio of the I2 and I1 cumulative rate ratios. CI indicates confidence interval; I1, January 2004 interruption; I2, July 2019 interruption.

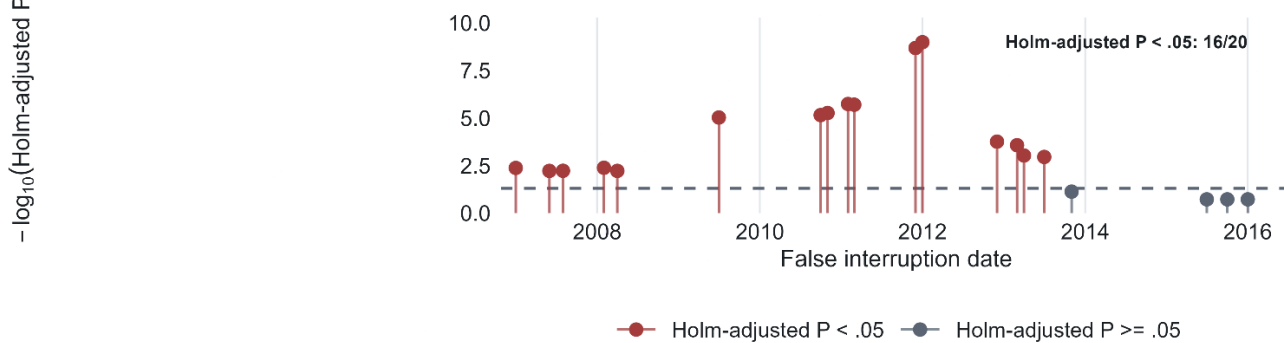
A Specification atlas

Frozen national specifications; primary model shown with heavier marks.



B False-date diagnostics

16 of 20 false dates rejected after Holm adjustment; dashed line denotes $P = .05$.



C Comparator-outcome diagnostics

Single-series estimates shown for diagnostic interpretation only.

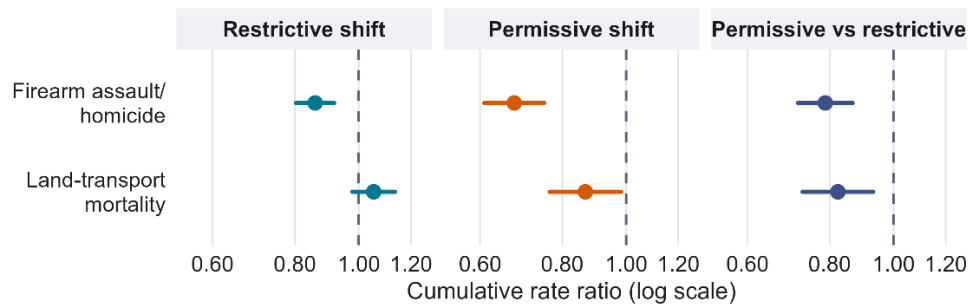


Figure 4

Sensitivity, deletion-influence, false-date placebo, and comparator-outcome analyses

Legend. Panel A shows core outcome-construction, model, seasonality, lag, window, timing, population, and trend sensitivities. Panel B shows leave-one-macroregion-out and leave-one-year-out influence analyses. Panel C shows joint tests at 20 false interruption dates, with Holm-adjusted significance

indicated. Panel D shows cumulative rate ratios for the positive firearm-assault or homicide comparator and negative land-transport comparator. Points and intervals are taken from the corresponding frozen model-specific outputs; intervals are two-sided 95% CIs. Reference lines denote a rate ratio of 1 or the applicable null test threshold. These analyses are diagnostic and do not modify the preregistered confirmatory family. CI indicates confidence interval; I1, January 2004 interruption; I2, July 2019 interruption.

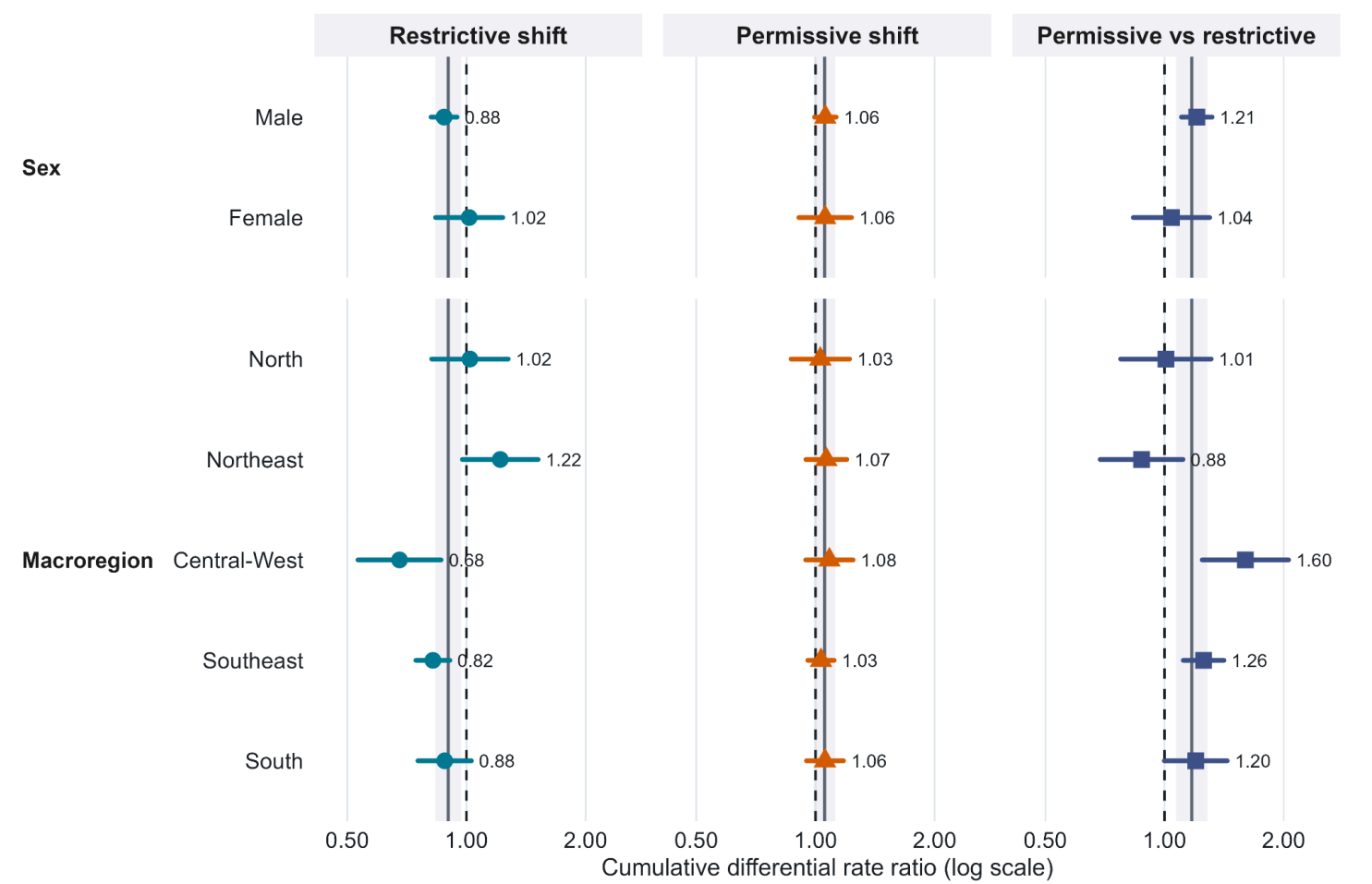


Figure 5

Exploratory 36-month controlled interrupted time-series estimates by sex and macroregion

Legend. Panels show the I1 cDRR, I2 cDRR, and I2-to-I1 cDRR ratio for sex and macroregion strata. Points and horizontal ranges are Rubin-pooled estimates and two-sided 95% CIs across 200 completed datasets. The pale vertical band and solid line show the corresponding national 95% CI and point estimate; the dashed line denotes the null rate ratio of 1. No multiplicity adjustment, formal interaction test, or confirmatory claim was made. Macroregions follow the standard IBGE geographic order. cDRR indicates cumulative differential rate ratio; CI, confidence interval; I1, January 2004 interruption; I2, July 2019 interruption.

Supplementary Files

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