

Not One Story: Multiple Historical Trauma and the Politics of Remembering

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Abstract

South Korea carries two mass traumas from the last century: the forced mobilization of Koreans under Japanese colonial rule (1937 to 1945) and the killing of civilians during the Korean War (1950 to 1953). This paper asks whether the two left different marks on how people vote today. I turn two colonial-era forced-labor registries and a compiled record of Korean War civilian killings into geocoded points, weight each point by its local intensity, and measure how heavily each township was exposed to each kind of violence. Addressing endogenous administrative boundary changes, I map every measure onto a fixed 1945 township-level boundary layer and link it to the results of nine cycles of South Korean democratic presidential elections (1987 to 2025). The preliminary results show that colonial and wartime trauma carry distinct, separable associations with the vote that a pooled measure would obscure. Multiple victimhood does not produce a single political legacy; each layer must be identified on its own.

1 Introduction

South Korea carries more than one memory of mass suffering. Colonial rule and the Korean War both left deep marks on the population, and both remain active in national identity and party politics. Most political science research on historical trauma looks at one event at a time. This project asks a different question: when a society holds several traumas at once, how do they relate to each other, and do they leave different traces on how citizens vote now?

I build a measure of colonial trauma as Japanese forced-mobilization persons per 1,000 residents at the township (eup/myeon) level, the finest unit the colonial records support, from two government registries. I build a measure of wartime trauma as Korean War civilian

killings per 1,000 residents at the city-county (si/gun/gu) level, drawn from the news outlet Newstapa’s compilation of two rounds of Truth and Reconciliation Commission reporting: 57,742 victims across 615 events. I record the killings separated by perpetrator—the Korean military, U.S. and UN forces, and North Korean and communist forces—since the political meaning of a death may depend on who caused it.

Every record is geocoded to the centroid of its 1945 administrative township, and a township’s exposure is the distance-weighted sum of nearby intensity under a bisquare kernel, computed at three bandwidths of 10km, 20km, and 30km. I link these trauma measures to presidential electoral outcomes at the city-county level (nine elections from 1987 to 2025) and the township level (six elections from 2002 to 2025). Administrative boundaries were redrawn many times after 1945, and not at random. Boundary change is thus endogenous to the outcome, so I project every trauma measure and every election onto one fixed 1945 township layer. Trauma and votes then sit on the same fixed geography across every election.

Two assumptions underpin the measurement. First, I treat exposure as a continuous intensity rather than a binary indicator, since a binary marker would count a township with one death and one with three thousand as equally exposed. Second, I allow exposure to cross administrative boundaries, since a massacre or forced mobilization one township away still affected the families and communities nearby. I therefore weight each event by its distance from a township and let that weight decay with a kernel. How far this reach extends is itself an empirical question, which is why I estimate exposure at several bandwidths.

2 Historical Background

Japan’s mobilization of Korean labor intensified between 1937 and 1945, as the Second Sino-Japanese War expanded into the Pacific War and the colony was reorganized into a wartime labor reserve. Estimates that count repeated conscriptions place the cumulative total mobilized at 798,043 (Duró and Palmer, 2024), while Japanese official sources record 667,848 Koreans conscripted for labor in Japanese firms after 1939 (Underwood, 2006). The mobilized were assigned predominantly to mines, war-related factories, and unpaid corvée labor, and the same administrative apparatus conscripted an estimated 50,000 women into military sexual slavery under the “comfort women” system. Because mobilization was implemented through the colonial administration at the local level, it generated village-level records that permit a spatially disaggregated measure of exposure.

For the Korean War, no official count of total civilian deaths exists. In its place I use Newstapa’s compiled record of civilian killings, comprising 615 perpetrator-attributed events. The events with a resolvable place name aggregate to 235 city-county units and 57,742

victims: 40,995 attributed to state forces, 8,814 to U.S. forces, and 7,933 to communist forces. This is a record of documented, officially investigated massacres, those most likely to persist in local collective memory, rather than an estimate of total wartime casualties.

The two traumas are not mobilized by the same political camp. The progressive camp has politicized the colonial grievance, through consumer boycotts of Japanese goods, appeals to the forced-labor and comfort women issues, and recurring disputes over official apologies. When Japan removed South Korea from its trade whitelist in 2019, President Moon Jae-in publicly characterized Japan as the aggressor and declared that "South Korea will never again lose to Japan."¹ The conservative camp has claimed ownership of the national-security frame, which traces the standing threat directly to the war and to North Korea; in the 2017 presidential debates, conservative candidates repeatedly pressed the progressive front-runner to state whether North Korea constituted the "main enemy." (*jujeok*)² These episodes are recent, but they reflect a durable and partisan division of ownership: the colonial past is the frame the progressive bloc owns and mobilizes, while wartime security is the frame the conservative bloc claims.

3 Theory

Historical trauma has no fixed political meaning. It is transmitted across generations and reactivated long after the violence ends, carried through families, through local memory entrepreneurs, and through the visible markers they leave behind (Lupu and Peisakhin, 2017; Cremaschi and Masullo, 2024; Menon, 2023). Whether such an inherited grievance translates into votes plausibly depends on issue ownership: a trauma may move the electorate where a party has claimed the interpretive frame that gives it political meaning.

In the Korean case, as the previous section briefly showed, the two camps have historically claimed opposing frames, the colonial past on the progressive side and national security on the conservative. But the framing does not by itself determine whether these traumas leave a durable imprint on the vote, or in which direction each does. That is an empirical question, and it is complicated by one fact: almost every locality carries some colonial mobilization and some wartime killing. The traumas cannot be contrasted against untouched places; they are distributed, at varying intensity, across the entire country. When exposure is this pervasive, no single trauma has an obvious direction on its own, because examined one at

¹Moon Jae-in, remarks at an emergency cabinet meeting, August 2, 2019; see *Segye Ilbo*, Mun dae-tongnyeong 'dasineun Ilbon-e jiji ankgetda,' August 2, 2019, and *YTN*, August 3, 2019.

²Yoo Seong-min, remarks at the KBS presidential candidates' debate, April 19, 2017; see *Seoul Sinmun*, April 20, 2017. Yoo pressed: "It is stated in the Republic of Korea's official documents that North Korea is the main enemy. Are you saying the commander-in-chief cannot call the main enemy the main enemy?"

a time the measures overlap and their associations blur. Separating them is therefore not a nuisance to be controlled away but the analytical task itself. If each trauma carries a distinct political association, it should become identifiable only when the two are measured apart and entered jointly, each estimated while the other is held fixed; a pooled measure, or either trauma alone, would tend to average the pattern away. The paper therefore poses two questions: whether localized exposure to colonial forced mobilization and to Korean War civilian killing is associated with durable differences in presidential voting, and whether that association becomes identifiable only once the two traumas are separated and considered jointly.

The hypotheses build on one another: H1 splits the traumas, H2 splits the wartime perpetrators, and H3 puts them back together additively.

H1 (Baseline). *The colonial and wartime associations become identifiable only when the two are measured apart.* The two traumas carry distinct political associations, but because nearly every locality carries some of each, an undifferentiated “trauma” measure lets the opposing associations offset, leaving no stable estimate.

H2 (Perpetrator-specific). *The wartime association depends on who did the killing, so the perpetrators must also be measured apart.* Killing by the state, by U.S. and UN forces, and by communist forces need not carry the same political charge, and pooling them would let those signals offset. I separate the three and leave each one’s direction to the estimates.

H3 (Additivity). *Where colonial and wartime exposure coincide, they enter additively, each keeping its own direction and independent effect.* This is the paper’s central claim. The traumas are layered: across almost the whole country, neither can be estimated without conditioning on the other: identifying the association of one trauma requires holding the other fixed.

4 Data and Measurement

I compile two archives of historical trauma, Japanese colonial forced mobilization and Korean War civilian killings, as geocoded intensity points, normalize each by its 1949 population (1955 where 1949 is missing), convert the normalized counts into distance-weighted exposure, and project everything onto a matched 1945 boundary layer to address endogenous boundary change.

4.1 Colonial trauma measure

The colonial trauma measure come from two government rosters of mobilized Koreans held at the National Archives of Korea: a 1952 to 1953 roster (일정시 피징용자 명부) covering most of the south, and a 1957 to 1958 roster (왜정시 피징용자 명부) filling its gaps in North Jeolla, Gangwon, and Seoul. Together they record 12,561 village-level entries totaling 240,044 mobilized persons.

I place each entry on the 1945 township baseline by name, falling back to a broader administrative level only when a precise match fails, so that no entry with usable geography is discarded. This locates 228,108 of the 240,044 mobilized persons (95.0 percent): most at the township level across 1,176 townships, and a residual 466 entries at the centroids of 61 cities or counties where the roster recorded no finer location. The remaining 5.0 percent resolve only to a province, but these are almost entirely aggregate rows that carry no place name to begin with, so little geographic information is actually lost.

4.2 Wartime trauma measure

The wartime trauma measure comes from Newstapa’s compilation of two rounds of Truth and Reconciliation Commission reporting on Korean War civilian killings, comprising 615 perpetrator-attributed monthly events. These aggregate to 235 city-county units and 57,742 victims: 40,995 attributed to state forces, 8,814 to U.S. forces, and 7,933 to communist forces. This is not a count of total wartime casualties, and it is not intended as one. It derives from an official state commission that documented the place, timing, perpetrator, and death toll of each killing. This is what the measure needs: a killing tied to a specific place and perpetrator is what local memory retains. A complete death toll would not serve better, since documented and localized violence shapes that memory more than the raw number of deaths.

4.3 Normalization and exposure

Each trauma event is fixed to a point at its 1945 location centroid and converted into a relative intensity before it spreads. I divide each point’s raw count by the historical population of its own unit and scale to 1,000 residents, using the 1949 census (1955 where 1949 is missing): a colonial point is normalized by its township population, a wartime point by its city-county population. The same hundred deaths therefore weigh more where fewer people lived. Each point then spreads this per-1,000 intensity outward, decaying with distance under a bisquare kernel that reaches zero at the bandwidth H :

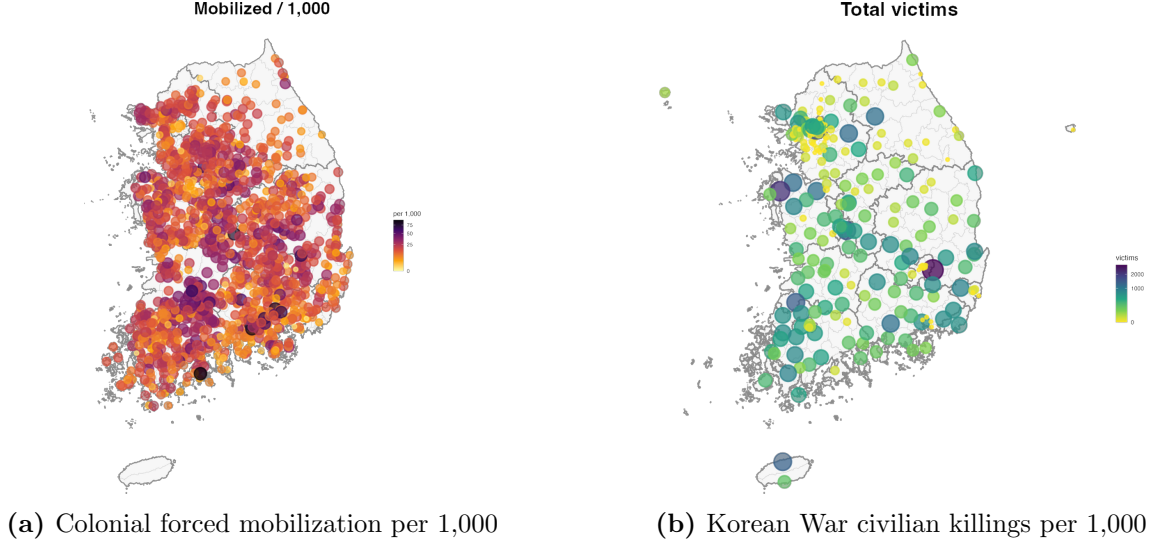


Figure 1: Geocoded trauma exposure across South Korea, projected onto 1945 township locations.

$$w(d; H) = \begin{cases} (1 - (d/H)^2)^2, & d < H, \\ 0, & d \geq H, \end{cases} \quad (1)$$

where d is the distance from a point to a township. A township's exposure is the kernel-weighted sum of every point within H , so a township ringed by many intense nearby events scores high and an isolated one low. Exposure across townships is then z-standardized, so each measure is in standard-deviation units and a coefficient reads as the change in the outcome per one standard deviation of local intensity. This gives every 1945 township a colonial and a wartime trauma measure. The two measures differ only in the spatial resolution of their points: colonial points are mostly at the township level, wartime points at the city-county level throughout. I compute exposure at bandwidths of 10, 20, and 30km and report the 20km results; the direction of the associations is the same across all three.

4.4 Outcome

The outcome is the progressive candidate's vote share, projected onto the same 1945 township baseline as the exposures. Election results from 2002 to 2025 exist at the township level and are matched directly; returns from 1987 to 1997 exist only at the city-county level and are split down to the townships inside each unit. Trauma exposure and vote share therefore sit on the same fixed geography in every election.

Table 1: Single-trauma associations (H1)

Measure	City-county (1987–2025)	Township (province FE)	Township (county FE)
Colonial mobilized	−0.398	+0.209	+0.238
Colonial deaths	−0.541	+0.867	+0.229
Colonial missing	+0.009	+0.682 [†]	+0.085
War: ROK state	+0.324	+0.278	+0.546*
War: U.S./UN	+0.984*	+0.507**	−0.009
War: NK/communist	−2.074***	−0.434	+0.155

Note: Each cell is a separate regression of progressive vote share (pp) on one z-standardized measure, with year fixed effects plus the fixed effect named in the column header. City-county spans 1987–2025, the township columns 2002–2025. SE clustered by county. [†] $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$.

5 Preliminary Results

The results follow the three hypotheses. Table 1 shows why the traumas cannot be read pooled or one at a time (H1). Table 2 separates them and finds that the wartime signal depends on the perpetrator (H2) and that the two traumas enter additively rather than merging (H3). The main analysis is at the township level with county fixed effects, using the 2002 to 2025 elections, whose returns carry township variation. Each exposure is standardized, so a coefficient is the change in progressive vote share in percentage points, per one standard deviation of local trauma intensity.

When I estimate each trauma measure on its own, none yields a stable association, exactly as H1 predicts (Table 1). The colonial measures are statistically insignificant throughout and reverse sign across specifications: colonial deaths, for instance, run negative at the city-county level (0.54) but positive within provinces (+0.87) and counties (+0.23). The single wartime measures are no steadier, shifting with the unit of analysis, from communist killing at 2.07 at the city-county level to U.S. killing at +0.98. Read in isolation, then, each trauma measure points in whatever direction the specification imposes rather than a consistent one. This is precisely the instability H1 anticipates when the measures are pooled or taken singly, and it is why the analysis must estimate them apart.

Disaggregating wartime killings by perpetrator yields the central result (Table 2). In the county fixed-effects specification, the three perpetrator-specific coefficients differ in both sign and significance. Killing by the South Korean state is positively associated with the progressive vote share and statistically significant: a one standard deviation increase in exposure predicts a 0.53 percentage-point increase of progressive presidential election vote share ($p < .05$), after adding geographic and urbanization covariates. The coefficients on

Table 2: Colonial trauma and wartime killing entered jointly (H2, H3)

	(1) Death (Province FE)	(2) Death (County FE)	(3) Mobilization (County FE)	(4) All colonial (County FE)
Colonial deaths	0.941 (0.787)	0.131 (0.261)		0.091 (0.405)
Colonial mobilized			0.115 (0.300)	0.077 (0.566)
Colonial missing				−0.047 (0.254)
War: ROK state	0.084 (0.343)	0.526* (0.243)	0.521* (0.240)	0.521* (0.239)
War: U.S./UN	0.519** (0.177)	−0.028 (0.352)	−0.027 (0.352)	−0.026 (0.353)
War: NK/communist	−0.541 (0.690)	0.024 (0.150)	0.030 (0.148)	0.024 (0.151)
Year FE	Yes	Yes	Yes	Yes
Province FE	Yes	—	—	—
County FE	—	Yes	Yes	Yes
Observations	9,234	9,234	9,234	9,234

Note: township level, 2002 to 2025. Wartime killing split by perpetrator. z-standardized coefficients, outcome in percentage points, SE in parentheses clustered by county. * $p < .05$, ** $p < .01$.

communist- and U.S.-perpetrated killing are both small and statistically indistinguishable from zero. This is the evidence for H2: a pooled wartime measure would average these opposing effects toward zero and mask the state association.

The U.S.-perpetrator coefficient is sensitive to the fixed-effects specification. It is null under county fixed effects but positive and significant under province fixed effects (0.52 percentage points, $p < .01$), indicating that its association with the progressive vote operates at the between-county rather than the within-county level.

Because Table 2 enters the colonial and wartime measures simultaneously, it also tests H3 directly. If the traumas were confounded, entering them jointly would distort or absorb one another's coefficients; instead, each retains its own magnitude, sign, and significance. The state-perpetrator coefficient holds at 0.52–0.53 percentage points ($p < .05$) whether colonial exposure is measured by deaths (column 2) or mobilization (column 3), and the U.S. and communist coefficients remain near zero throughout. Substituting one colonial measure for another, or entering all three jointly (column 4), leaves the wartime estimates essentially unchanged. The effects are therefore separable: each trauma, and each perpetrator, carries an independent association that survives the presence of the others. This is the additive

structure H3 predicts, and it is what licenses reading each trauma on its own.

Two limitations qualify these estimates. First, the colonial coefficient is not robust in sign. It is positive under province fixed effects, near zero under county fixed effects, and varies sharply across election years, from positive in 2007 and 2012 to negative in 2017 (-2.80 percentage points), indicating that the colonial association is conditional on the electoral context rather than a stable geographic effect. Second, the within-model R^2 is small throughout, so localized trauma exposure accounts for only a modest share of the variance in the progressive vote. The estimates identify a consistent perpetrator-specific pattern, not a large one.

6 Conclusion

The preliminary results support two findings. First, the association between wartime violence and the progressive vote depends on the perpetrator, not on exposure alone: killing by the South Korean state is positively and significantly associated with the progressive vote share, communist-perpetrated killing is indistinguishable from zero, and U.S.-perpetrated killing is significant only at the between-county level, so a measure that pools the three recovers no association. Second, and central to the design, the colonial and wartime exposures enter additively: each retains its own coefficient in sign, magnitude, and significance when the two are estimated jointly, so the layered traumas are separable rather than confounded. This is the evidence for H3, and it is what licenses reading each trauma independently. The colonial association itself is not robust in sign, and localized exposure explains only a modest share of the variance in the vote.

What this baseline cannot establish is the mechanism: whether the association operates through inherited family memory, local memory entrepreneurs, or elite framing at election time. Adjudicating between these channels is the next step, and the measurement developed here makes it tractable by fixing where each trauma fell, and at what intensity, on a spatial frame held constant across the democratic era.

7 Future Research

Two causal components and one qualitative validation build on this baseline, moving the project from historical pattern to individual mechanism to population dynamics. First, a survey experiment ($N = 2,000$) assigns colonial, wartime, combined, or control primes and measures nationalism, authoritarian and policy preferences, partisanship, and historical-justice attitudes, with region and ideology interactions, to recover the individual causal pathways.

Second, an agent-based and network simulation model asks whether memory effects persist or decay under social-contagion dynamics. Third, to test the transmission channel directly, I geocode the physical carriers of local memory: the Statues of Peace commemorating the “comfort women” and the monuments to colonial forced-labor victims. These are the visible carriers that memory-entrepreneur accounts point to, the objects through which local memory entrepreneurs keep a grievance alive on the ground (Cremaschi and Masullo, 2024; Menon, 2023). Mapping where these memorials stand, and setting their locations against the exposure measures built here, lets me check qualitatively whether trauma is remembered and marked where it fell, which is the local-transmission mechanism.

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