

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

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Abstract

When a society carries multiple historical traumas, what determines which of them shapes contemporary political behavior? Examining South Korea's layered histories of Japanese colonial mobilization and Korean War civilian killings, I argue that a trauma's political endurance depends not on its severity, but on two conditions: partisan ownership and the register of memory (local vs. national). To test this, I construct the first quantified dataset of colonial forced-labor registries, merge it with comprehensive Truth and Reconciliation Commission records of wartime killings, and project both onto a fixed 1945 township layer across six presidential elections (2002–2025). The results reveal a stark divergence. Killings by U.S. forces—claimed by the progressive bloc—significantly raise the progressive vote share in affected townships. In contrast, killings by the South Korean state leave no electoral trace because their legacy is contested by both blocs. Furthermore, colonial mobilization, despite dominating national public discourse, leaves no geographic electoral trace because its memory is institutionalized at the national level. I demonstrate that layered traumas do not merge or erase one another; rather, they influence the political behavior of local communities independently, offering a new framework for understanding how overlapping historical legacies shape contemporary voting behavior.

Keywords: historical trauma, historical legacies, collective memory, colonialism, political violence, political attitude

1 Introduction

What happens to the vote when a society carries several historical traumas at once, and in which direction? Research on the political legacies of historical trauma typically examines one traumatic event at a time, showing how a single event shapes politics decades later, from Stalin’s repressions in Ukraine (Rozenas et al., 2017) to Nazi occupation in Italy (Fontana et al., 2023) to slavery in the American South (Acharya et al., 2016). However, much less is known about what happens when traumatic memories coexist. Like many post-colonial states, South Korea bears the legacy of multiple historical traumas. Specifically, Japanese colonial rule (1910–1945) and the Korean War (1950–1953) profoundly impacted the populace, continue to shape national identity and partisan competition, and are pervasively yet unevenly distributed across the territory. I argue that which of these traumas shapes a locality’s vote today is determined not by how much the locality suffered but by what happened to each trauma afterward: whether a contemporary political actor claimed it (*Issue Ownership*), whether its memory was kept locally or nationally (*Register*), and whether the grievance could be settled (*Resolution*). Traumas that differ on these properties do not merge into a single grievance or displace one another (*Coexistence*).

I measure colonial trauma as forced-mobilization persons per 1,000 residents at the township (*eup/myeon*) level, from two government registries at the National Archives of Korea, and wartime trauma as Korean War civilian killings per 1,000 residents, from Newstapa’s compilation of two rounds of Truth and Reconciliation Commission reporting: 57,742 victims across 615 monthly events. I record the killings separated by perpetrator, since the political meaning of a death plausibly depends on who caused it and who has since claimed it. The two archives differ in resolution. Colonial records carry a 1945 township identifier and are placed in that township; wartime records are compiled at 615 place-month-perpetrator entries and are placed at the county centroid with county population as the denominator. A township’s exposure is the distance-weighted sum of nearby intensity under a bisquare kernel at bandwidths of 5, 10, 20, and 30 kilometers. Because boundaries were redrawn repeatedly after 1945, and not at random, boundary change is endogenous to the outcome; I therefore project every measure and every election onto one fixed 1945 township layer, and estimate on the six cycles of presidential election from 2002 onward for which returns exist at township resolution.

The results follow the four propositions in order. First, issue ownership: within provinces, exposure to killings by U.S. and UN forces, the one wartime grievance claimed without contest by the progressive bloc, predicts a higher progressive vote share in all twenty-four specifications crossing three outcome definitions, four bandwidths, and two samples, with coefficients from +0.264 to +0.891 percentage points per standard deviation. Killings by the South Korean state, whose legacy both blocs contest, and by communist forces show no comparable effect, and a measure pooling the three perpetrators reports a smaller coefficient

than the U.S.-specific estimate while concealing that the components differ in sign. Direction follows the claimant, and pooling across claimants blurs it.

Second, register: colonial forced labor mobilization, the grievance that dominates Korean public discourse, leaves no geographic trace at any bandwidth under any specification with geographic fixed effects. This is the prediction of the argument rather than a failure of it. The colonial grievance has been carried to the national level through treaty disputes, court rulings, and boycotts, where it serves as an interpretive frame for the electorate as a whole rather than as an inheritance held by particular localities.

Third, resolution: one colonial measure behaves differently. Persons recorded as missing are the one colonial measure with a positive association at short distances, clearest at 5 kilometers though just short of conventional significance, whereas total mobilization and deaths predict the vote at no distance. All three have received state redress, but only disappearance functions as a grievance that compensation cannot close. A payment to the family of a person who never returned and whose death was never confirmed settles nothing, and the grievance stays with the locality where the person was taken.

Fourth, coexistence: the layered traumas influence the vote independently. The U.S. coefficient moves only from 0.373 to 0.409 whether estimated alone or alongside any combination of colonial measures, so neither trauma biases the estimate of the other, and the two register at different scales, the wartime trace strengthening as the kernel widens and the colonial trace confined to the immediate vicinity. Korea's townships carry two historical traumas at once, and each shapes the vote on its own terms.

2 Historical Trauma and Multiple Traumas

2.1 The Political Legacies of Historical Trauma

Historical trauma shapes political behavior long after the violence that produced it has ended, but what it does is not determined by the trauma itself. Exposure to war reliably raises political participation and collective action (Bellows and Miguel, 2009; Blattman, 2009; Wang, 2021; Bauer et al., 2016); which party that participation benefits, however, is not fixed by the violence, and the direction the vote takes depends on perpetrator identity. One line of research finds a retaliatory pattern: voters punish the political successors of those responsible. For instance, Stalinist repression in Ukraine reduced subsequent support for Russian affiliated parties (Rozenas et al., 2017). Nazi occupation shifted Italy's postwar vote leftward for four decades (Fontana et al., 2023). Pinochet-era repression raised anti-regime voting in Chile (Bautista et al., 2023), and victimization in the Spanish civil war hardened identities against the perpetrator's side (Balcells, 2012, 2017). In each case the vote moves against the political heirs of the perpetrator: what predicts electoral behavior is who committed the violence, not how many were affected.

A second body of literature identifies a contrasting electoral response to violence. Under specific conditions, victimization can induce political compliance or social consolidation. For example, Stalinist engineered famines generated political loyalty where institutional structures incentivize compliance over opposition (Rozenas and Zhukov, 2019). Similarly, Bosnian wartime violence consolidated ethnic bloc voting instead of triggering retaliatory punishment against specific political actors (Hadzic et al., 2020).

The findings are therefore mixed, and the pattern of the mixture is informative. The same regime, and in some cases the same repertoire of violence, produced punishment in one setting and loyalty in another, so severity of the violence cannot explain the divergence. What explains it is the political context *after* the violence (Menon, 2023; Rozenas and Zhukov, 2019). Historical trauma influences voting behavior through political framing instead of passive memory, operating under two conditions. First, a party must own the frame that gives the trauma political meaning. Second, the underlying grievance must remain unresolved; a settled claim has nothing left to mobilize. Issue ownership theory supplies the first condition. Parties compete by emphasizing the issues that voters trust them to handle (Petrocik, 1996; Budge and Farlie, 1983). Ownership can also be associative, so that a party owns an issue through its identity rather than through perceived competence (Walgrave et al., 2012), and it converts into votes only among voters for whom the issue is salient (Bélanger and Meguid, 2008; Egan, 2013; Chong and Druckman, 2007).

This second condition extends issue ownership from campaign strategy to historical legacy by identifying when an appropriated grievance loses its electoral utility. Forced migration provides the clearest evidence of this dynamic. Displaced populations cultivate enduring victim identities that span generations independently of their receiving communities (Lupu and Peisakhin, 2017; Dinas et al., 2021a). West German regions absorbing large numbers of postwar expellees disproportionately supported the radical right across multiple decades (Menon, 2023). However, this correlation remained contingent on the grievance remaining unresolved. The effect vanished following societal acknowledgment in the early 2000s and resurfaced only when radical right parties reclaimed the issue. Resolution, in other words, demobilizes a legacy as effectively as forgetting: a settled grievance loses its predictive power over the vote regardless of how intense the original violence was, which is consistent with broader evidence that contemporary politics governs when legacies activate (Charnysh, 2015; Fouka and Voth, 2023; Rozenas and Zhukov, 2019). This is the interpretive rule the analysis below relies on. A null result for a historical trauma can reflect political settlement rather than the absence of severity.

However, these two conditions have only been validated in contexts featuring singular traumatic events. The literature typically analyzes a single grievance, its potential appropriation by a single party, and its linear path toward resolution or neglect. Because these foundational cases lack overlapping legacies, it remains unclear whether these ownership and resolution mechanisms operate similarly when a second layer of trauma is present. Whether

they do requires knowing how memory is transmitted, where it stays, and what happens when multiple memories occupy the same territory. The next section addresses these spatial and interactive dimensions.

2.2 Memory Transmission, Localization and Multiple Traumas

Before asking how multiple traumas operate together, it is necessary to establish how the memory of a single event is transmitted. Scholarship recognizes collective memory as an active practice rather than a passive inheritance (Halbwachs, 1992; Olick and Robbins, 1998), distinguishing between two primary modes of transmission. Communicative memory, transmitted orally within families and communities, is bounded by the lifespan of those who can attest to the event, roughly three generations; cultural memory, institutionalized in monuments, textbooks, and commissions, is not so bounded and can persist after every witness is gone (Assmann and Czaplicka, 1995; Nora, 1989; Wertsch, 2002). This distinction underlies the notion of register used in this paper: a memory held in the local register lives mainly in communicative transmission and local carriers, whereas a memory carried into the national register has been institutionalized as cultural memory. That communicative memory of political violence persists across generations at the individual level is well established (Lupu and Peisakhin, 2017; Dinas et al., 2021a; Wayne and Zhukov, 2022), but whether it stays in place, in the villages where the violence fell, is a separate question.

This spatial localization of memory is conditional. Descendants must remain near the site, or physical carriers must be built there: monuments and memorials hold grievances at the places of violence (Cremaschi and Masullo, 2025; Menon, 2023), local environments must support mobilization for victimization to have durable political consequences (Villamil, 2021; Jelin, 2003), and latent memories translate into votes when elites activate them (Ochsner and Roesel, 2024; Fouka and Voth, 2023). When these conditions hold, the prediction is spatial: the electoral trace of the memory should be strongest near where the violence fell and weaken with distance. When instead a memory is taken up by parties and media as a national frame, the prediction inverts. Such a memory reaches a voter in a minimally exposed township on the same terms as one in a highly exposed township, so it should remain politically potent while correlating weakly with the geography of the violence. Which pattern a given trauma follows is therefore an empirical question that distance can answer, not a property to be assumed in advance.

This account of a single trauma makes it possible to pose the question of multiple traumas. Layered trauma is not a rare configuration. Any society that passed through colonial rule and then through war carries at least two, and much of the postcolonial world passed through that sequence. In these cases the traumas do not belong to rival groups facing each other across a boundary; they sit within the same population, often within the same families and the same townships, so that most people are exposed to both, even if indirectly. The literature

nonetheless offers only two frames for societies carrying multiple traumas, and each was built for a different problem. Competitive victimhood holds that groups who see their suffering as unique vie for primacy (Noor et al., 2012); among descendants of the German expellees, recognition as victims predicts prejudice toward contemporary refugees (Dinas et al., 2021b). The frame presumes rival constituencies, one per trauma, and that presumption fails here, where the same families carry both. Multidirectional memory rejects the scarcity premise: memories of distinct traumas are not zero-sum rivals but cross-referential, each supplying language and frames for the other (Rothberg, 2009, 2011; Vollhardt and Bilali, 2015; Subotić, 2019). It is the closer frame to the coexistence this paper predicts, but it was developed for memories traveling across communities and borders, and it makes no statistical claim about what two traumas do inside one electorate.

I therefore test the joint structure directly. If the traumas compete, exposure to one should diminish the electoral effect of the other, which appears as attenuated main effects and a negative interaction. If they accumulate, they should reinforce each other where they overlap, which appears as a positive interaction. If they coexist, each should retain its coefficient whether or not the other is included, and their interaction should be indistinguishable from zero.

3 South Korean Case: Ideological Cleavages and the Ownership of Trauma

South Korean politics offers an unusually clean division of issue ownership. The regional cleavage between Yeongnam (Gyeongsang-do) and Honam (Jeolla-do) has organized the vote since democratization and is itself a political construction (Kwon, 2004; Moon, 2005; Kang, 2008, 2016). Onto this structure the camps have mapped opposing historical frames: the progressive bloc owns the colonial trauma and the demand for historical justice (Jo, 2022; Ko and Kim, 2023), while the conservative bloc owns the national-security frame tracing the standing threat to the war and to North Korea (Shin, 2017).

The division of ownership has historical roots on both sides, not just electoral logic. South Korean conservatism grew out of post-liberation anti-communist state-building and the export-led industrialization of the authoritarian era; for this bloc the North Korean threat remains the core of national security, which sustains a pro-defense posture built on the U.S.–South Korea alliance and working relations with Japan (Cha, 1999; Snyder, 2018). The progressive bloc, by contrast, traces its intellectual roots to the democratization struggles against authoritarian rule, above all the Minjung movement of the 1980s. This movement cultivated a deep skepticism toward the United States for its tacit support of military dictatorships, and it viewed the authoritarian regimes’ normalization of relations with Japan as a compromise that undermined historical accountability for colonial rule. From that history

the progressive camp takes an anti-imperialist and anti-colonial stance, prioritizing inter-Korean reconciliation and the establishment of a peace regime over military confrontation (Lee, 2007; Moon, 2012; Choi, 2012; Jo, 2022).

Korea is also one of the few settings where archival material exists on both sides. Wartime victimization has long-run political consequences that run through stigma as well as trauma, since the postwar state treated victims as suspect (Hong and Kang, 2017). Closest to the present design, Kang and Hong (2017) exploit the fact that South Korean, U.S., and North Korean forces all killed civilians, and find that attitudes toward each actor diverge accordingly. That result is individual-level and attitudinal; whether the same perpetrator distinction survives in aggregate voting, and whether it holds once colonial exposure is measured alongside it, is open. Village-level historical treatments also map onto contemporary Korean voting (Hong et al., 2023), and colonial-era administrative records can be reconstructed for quantitative use (Hong and Paik, 2018). What has not been done is to bring the colonial mobilization registries into this framework and estimate both traumas on a common geography.

Japan’s mobilization of Korean labor intensified between 1937 and 1945, as the Pacific War turned the colony into a wartime labor reserve (Palmer, 2013). A Welfare Ministry Labor Bureau tally of October 1945 records 667,684 Korean civilians conscripted to work for private firms in Japan after 1939 (Underwood, 2006), and adding military conscripts brings the number mobilized outside Korea to roughly 940,000. The mobilized went to mines, war factories, and unpaid *corvée* labor, while the same administrative apparatus conscripted tens of thousands of women into military sexual slavery (Soh, 2008). Because mobilization ran through local colonial channels, it generated the village level records this empirical design requires (Eckert, 1991; Shin and Robinson, 1999).

When it comes to wartime civilian killings, the total number of civilians killed in the Korean War remains uncertain. Official figures produced by the two Korean governments diverge widely, were compiled for political purposes, and do not disaggregate deaths by perpetrator or locality. What is well established is that the mass killing of civilians was a defining feature of the war on all sides (Cumings, 2010; Kim, 2004; Hwang, 2016). South Korean military and police carried out large scale preemptive executions of suspected leftists, U.S. forces killed civilians in ground operations and air attacks, of which No Gun Ri is the best documented case (Hanley et al., 2001), and North Korean forces and local leftist militias executed those they classified as reactionaries during the occupation and retreat. Apart from a parliamentary inquiry in 1960 that was cut short by the military coup the following year, the South Korean state did not systematically investigate these killings until the Truth and Reconciliation Commission, Republic of Korea (TRCK).¹

¹The TRCK was established under the 2005 Framework Act on Clearing Up Past Incidents for Truth and Reconciliation. Its mandate covers unlawful civilian deaths, injuries, and disappearances from August 15, 1945, through the Korean War. The TRCK relied primarily on petitions from victims’ relatives, alongside

The two traumas are finally, are not mobilized by the same camp. The progressive bloc has politicized the colonial grievance through boycotts of Japanese goods, appeals to the forced-labor and comfort women issues, and disputes over official apologies (Jo, 2022; Ko and Kim, 2023; Lind, 2008; Dudden, 2008; Arrington, 2016); when Japan removed South Korea from its trade whitelist in 2019, President Moon Jae-in called Japan the aggressor and declared that South Korea would never again lose to it.² The conservative bloc has claimed the national-security frame, pressing the progressive front-runner in the 2017 debates to say whether North Korea was the “main enemy” (*jujeok*).³ That division of ownership is durable, grounded in ethnic-national identity on one side (Shin, 2006; Hur, 2022) and anticommunist state-building on the other (Shin, 2017).

4 Hypotheses

I use *historical trauma* for the general phenomenon, *colonial trauma* and *wartime trauma* for its two components, and *event* for a single documented instance of either.

The argument rests on two premises developed in the previous section: that the political meaning of a historical trauma is assigned by the contemporary political actors who claim it as an issue, and that the memory of a trauma is organized at the level, national or local, at which redress is pursued. From these premises I derive four hypotheses, each stated as a general proposition and then given its implication for the Korean case. Korea is suited to the test because nearly every locality was exposed to both traumas, so that the analytical question is which memory each locality’s exposure has entered rather than whether it was exposed at all.

4.1 Issue Ownership

Existing research on the legacy of violence treats violence primarily as a quantity whose effects scale with its magnitude (Rozenas et al., 2017; Zhukov and Talibova, 2018; Acharya et al., 2016) and, more recently, as an experience whose effects depend on the identity of the perpetrator (Balcels, 2012; Lupu and Peisakhin, 2017; Rozenas and Zhukov, 2019), while another body of work shows that these effects depend on whether contemporary actors activate memory (Ochsner and Roesel, 2024; Villamil, 2021; Cremaschi and Masullo, 2025). I build on the second point and specify the mechanism: perpetrator identity matters because it determines which contemporary actor can claim the violence as an issue (Petrocik, 1996;

direct investigations into major incidents. The first commission operated from 2005 to 2010, while the second launched in 2020. By mid 2023, the two rounds published 252 case reports, which form the basis of the data used here (Korea Center for Investigative Journalism-Newstapa, 2023).

²Moon Jae-in, remarks at an emergency cabinet meeting, August 2, 2019; see *Segye Ilbo* and *YTN*, August 2–3, 2019.

³Yoo Seong-min, KBS presidential candidates’ debate, April 19, 2017; see *Seoul Sinmun*, April 20, 2017.

Walgrave et al., 2012). A killing becomes usable in electoral politics when a party incorporates it into a frame the party owns, so that the direction of its electoral effect follows that party, and violence that no actor has claimed should have no electoral effect regardless of its severity.

H1 (Issue Ownership). The electoral effect of a historical trauma follows the contemporary political actor that has claimed it. Traumas claimed by no actor have no electoral effect.

In Korea, wartime killings by South Korean and U.S. forces have been claimed by the progressive bloc, through transitional justice and through criticism of the alliance (Jhee, 2008; Kim, 2013), while killings by communist forces have been claimed by the conservative bloc through the national-security frame (Shin, 2017). H1 therefore implies that exposure to the first two raises progressive vote share and exposure to the third raises conservative vote share, and, since the components point in opposite directions, that a measure pooling all wartime killings will understate every effect. Issue ownership is not equally secure across the three: the legacy of state killings is claimed by the progressive bloc but defended by the conservative bloc, and redress has been intermittent (Kim, 2013), so where ownership is contested in this way the effect should be weaker than where it is uncontested.

4.2 Register

Traumas differ in how their memory is maintained. When redress is sought against an external state through treaties, courts, and diplomatic disputes, the grievance is institutionalized on behalf of the nation as a whole. Localities that supplied more victims have no distinct claim, and local variation in exposure does not become local variation in memory. By contrast, when redress is pursued case by case, with particular families petitioning about particular places often following decades of political suppression, the memory stays attached to those places (Nora, 1989; Jelin, 2003).

H2 (Register). Traumas whose redress is organized nationally against an external party show no local variation in electoral effect. Traumas whose redress is organized locally do.

Colonial labor mobilization has been contested through bilateral treaties, court rulings against Japanese firms, and national boycotts (Jo, 2022; Ko and Kim, 2023), whereas wartime killings have been investigated village by village and petitioned by individual families (Kim, 2013, 2014). H2 therefore implies that a geographic design will detect the wartime trace and miss the colonial one, so that a null result for colonial mobilization is a prediction of the argument and would not show that colonial memory lacks political weight.

4.3 Resolution

Grievances also differ in whether they can be settled. A settlement discharges a claim, since once compensation has been paid or a judgment rendered the grievance loses its capacity to mobilize (Arrington, 2016; Dinas et al., 2021b), but some grievances cannot be discharged because a disappearance leaves no remains, no date of death, and nothing that a treaty or a fund can resolve.

H3 (Resolution). Grievances that have been formally settled lose their electoral effect. Grievances that cannot be settled retain it.

Colonial conscription registers provide three distinct measures of exposure: total mobilization, deaths, and disappearances. All three have been the object of state redress: mobilization claims through the 1965 treaty, the 2018 Supreme Court judgments, and the compensation funds that followed, and death and disappearance through consolation payments to families under the 2007 and 2010 support acts. The difference lies in what redress can close. A payment to a survivor, or to the heirs of a documented death, discharges the claim; a payment to the family of a person who never returned and whose death was never confirmed settles nothing. Because of this lack of closure, H3 predicts that disappearances will retain a persistent local electoral effect, unlike mobilization and deaths, even as H2 anticipates the overall nationalization of colonial memory.

4.4 Coexistence

While multiple historical traumas frequently overlap geographically, legacy of violence research traditionally estimates single events (Cirone and Pepinsky, 2022; Walden and Zhukov, 2020). When multiple traumas are evaluated together, three distinct dynamics are possible. They may be cumulative, increasing an aggregate stock of grievance as assumed by magnitude scaling models (Rozenas et al., 2017; Zhukov and Talibova, 2018). They may be competitive, whereby recognizing one grievance crowds out another as groups contest singular victim identities (Volkan, 2001; Noor et al., 2012). Or they may coexist, with each trauma retaining its own political meaning without displacing the other (Rothberg, 2009). The two premises of this paper specify when each outcome should occur. Competition arises when the same actor mobilizes two traumas for the same audience, so that one frame must give way to the other (Chong and Druckman, 2007), or when two traumas are kept in the same register, so that they draw on the same channel of memory. When different actors claim different traumas and keep them in different registers, neither condition holds, and each trauma should enter the vote independently.

H4 (Coexistence). Where multiple traumas overlap, each retains its own electoral effect. The effects are independent rather than cumulative or competitive.

Korea satisfies both conditions for coexistence. Colonial trauma is claimed by the progressive bloc as a grievance against Japan and is maintained in the national register through treaty disputes, court rulings, and boycotts (Jo, 2022; Ko and Kim, 2023; Arrington, 2016), whereas wartime trauma is divided between the two blocs by perpetrator (Jhee, 2008; Shin, 2017) and maintained in the local register through village level investigation and family petitions (Kim, 2013, 2014). The two traumas therefore have different owners and different registers, and H4 implies that they should enter contemporary voting independently. Empirically, coexistence appears as stability: the estimate for each trauma should be unchanged when the other is included in the model, and their interaction should be zero. Competition would instead appear as attenuation of one estimate when the other is included, and accumulation as a positive interaction between them.

5 Data and Measurement

I compile two archives as geocoded intensity points, normalize each by historical population, convert them into distance-weighted exposure, and project everything onto a matched 1945 boundary layer. This section describes the two archives and the measures derived from them; Section 5.3 describes how the colonial archive was converted into data.

5.1 Colonial Trauma

The colonial measures come from two government registers of Koreans conscripted for labor under Japanese rule, both held by the National Archives of Korea. The primary source is the 1952–1953 register (일정시피징용자명부) (National Archives of Korea, 1953); where its coverage is incomplete, in North Jeolla, Gangwon, and Seoul, I supplement it with the 1957–1958 register (왜정시피징용자명부) compiled by the Labor Bureau for the same purpose (National Archives of Korea, 1957).⁴ The 16,381 register pages I transcribed (Section 5.3) aggregate to 12,561 village-level entries, since a village with many conscripts runs across several pages, and these entries record 240,044 conscripted persons. Because the registers record each person’s outcome, they yield three measures: total conscripted, deaths during conscription, and persons recorded as missing.

I place each village entry on the 1945 township boundary by matching its recorded address to the historical township name, and fall back to the county level only when no township match is possible. This procedure locates 228,108 of the 240,044 persons, the large majority at township level across 1,176 townships.

⁴Both registers contain separate sections for labor conscripts (피징용자) and military conscripts (피징병자), each organized by village; I transcribe the labor conscript sections only, since military conscription raises distinct questions of redress and is left for future work.

5.2 Wartime Trauma

I measure wartime trauma using Newstapa’s systematic compilation of the TRCK case reports from both rounds of investigation (Korea Center for Investigative Journalism-Newstapa, 2023). Covering the active conflict period from June 25, 1950, to July 27, 1953, the data organize 57,742 verified victims into 615 entries, each defined by a place, a month, and a perpetrator, in three perpetrator categories: South Korean state forces (40,995 victims, or 71 percent), U.S. and UN forces (8,814 victims, or 15 percent), and communist forces (7,933 victims, or 14 percent).

I rely on this source not because it captures total wartime casualties, but because of its institutional properties. Based on formal victim petitions rather than exhaustive population counts, the TRCK findings provide the three essential features required for this design: precise locality, official perpetrator attribution, and statutory state adjudication. While petition based sourcing means these figures constitute a lower bound with varying subcounty coverage, no alternative archive matches its combination of official validation and perpetrator specificity. The implications of this selection are addressed in the subsequent discussion of inference.

5.3 Data Collection Strategy

Both sources are public records, but only the TRCK case reports had already been read, coded, and published as an interactive dataset by Newstapa, and I obtained the news agency’s permission to use its compiled file covering both rounds of the commission’s work (Korea Center for Investigative Journalism-Newstapa, 2023). The colonial mobilization rosters had not been quantified by anyone.⁵ They are held by the National Archives of Korea as scanned page images of the original ledgers, with no transcription, summary, or count attached. Utilizing this historical source therefore required constructing a novel geographic dataset entirely from scratch.

I conducted this data collection between January and May 2026 with two Political Science Lab undergraduate research assistants at UC Merced.⁶ Each volume is divided into sub-files, at least one per village, and each sub-file is a single ledger page on which every column records one person conscripted for labor, with fields for the person’s address, name, date of birth, dates of conscription and return, and, where applicable, marks for death (死亡), disappearance (行方不明), and residence abroad (國外在住). Figure 1A reproduces one sub-file with the five fields we coded numbered in the order they were recorded. Because neither assistant read Korean or Chinese characters, I prepared a slide guide identifying where each numbered field appears on the page and what to count, and I built the entry form in Qualtrics

⁵The Archives provide the volumes as scanned page images and maintain a name index for individual lookup, but the entries have never been aggregated by locality or by outcome.

⁶I thank Emily Acosta and Aidan Yu for their work on the transcription.

1) Sub-file # 일정시피정용자 (경산군 화양면 대조동) **2) Township address**

3) Each column is list of victims (mobilized total # of victims)

4) Count if checked (Dead)

5) Count if checked (Missing)

UCMERCED

Enter Sub-file # (As written, numbers only!)

Address

How many people are written in this sub-file (total # in this page) ?

How many people were marked as dead? (Just put 0 if there is none)
* Most of the case, you can find it in the 6th row, written as "死亡"

How many people were marked as missing? (Just put 0 if there is none)
* Most of the case, you can find it in the 7th row, written as "行方不明"

Continue to add more sub-files?

☐ Yes, add more subfiles' addresses of this file

☐ No, I finished this file

(a) A sub-file of the register, with coded fields numbered

(b) The Qualtrics entry form

Figure 1: Transcription of the colonial labor conscription register. Panel (a) shows one sub-file of the register as displayed on the National Archives of Korea portal, with the five fields coders recorded numbered on the page. Panel (b) shows the Qualtrics entry form, whose questions follow the same numbering so that coders could transfer each field directly (National Archives of Korea, 1953).

(Figure 1B) with one question per field in the same order, so that the same instrument served all three coders and produced a stacked long-form file without further processing. For each sub-file the coder entered the sub-file number, the village address, the total number of persons listed, the number marked dead, and the number marked missing, then indicated whether the volume contained further sub-files; recording the sub-file number first prevented skipped and duplicated pages. In total we transcribed 16,381 sub-files, converting the archive’s scanned images into village-level counts of mobilization, death, and disappearance.

The construction of this dataset represents an independent methodological contribution. Although the original registers have been publicly accessible, they remained unquantified, leaving the spatial distribution of colonial conscription unmeasured. The dataset released alongside this study provides the first village level aggregate counts of total mobilization, deaths, and disappearances derived from the complete register. This generates two distinct analytical advantages. First, it allows researchers to integrate colonial exposure with other historical legacies across a unified Korean spatial framework, overcoming a major limitation in the prior literature. Second, the digitization protocol offers broad generalizability. Because unprocessed scanned ledgers constitute the standard archival format for colonial administration across the former Japanese empire, this procedure provides a scalable solution. By pairing a visual field guide with a structured survey entry instrument, the protocol enables researchers without source language proficiency to systematically extract data, making the method transferable to similar archival contexts.

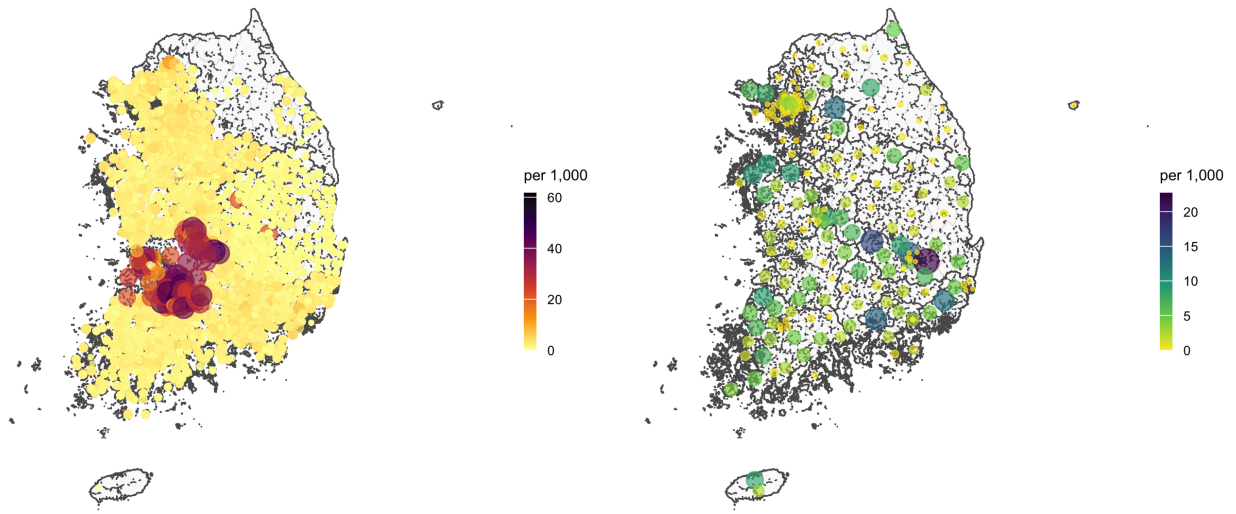
5.4 Normalization and exposure

Figure 2 illustrates the two spatial exposure surfaces. The procedure anchors each historical event to its 1945 geographic centroid and converts the raw frequency into a relative intensity. This raw count is divided by the historical population and scaled to an incidence per 1,000 residents based on the 1949 census, or the 1955 census when earlier records are unavailable. Because colonial points are normalized by township populations and wartime points by aggregate city and county populations, an identical number of casualties generates a higher intensity in less populous areas.

Each point then spreads this per-1,000 intensity outward, decaying with distance under a bisquare kernel that reaches zero at the bandwidth H :

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

where d is the distance from a point to a township centroid. A township’s exposure is the kernel-weighted sum of every point within H , so a township near many high-intensity events scores high, while an isolated township scores low.



(a) Colonial mobilization per 1,000

(b) Korean War civilian killings

Figure 2: Geocoded historical trauma exposure on the 1945 layer. Colonial points are township-level intensities per 1,000 residents; wartime points are victim counts at county centroids.

This spatial approach rests on two core assumptions. First, trauma exposure operates as a continuous intensity rather than a binary indicator, avoiding the distortion of equating a jurisdiction with a single casualty to one with thousands. Second, exposure permeates administrative borders because localized violence inevitably impacts adjacent populations. The precise geographic limit of this diffusion is an empirical question, which is why the analysis estimates and reports effects across four distinct bandwidths. All exposure metrics are standardized to z scores, allowing coefficients to represent percentage point changes per standard deviation. The result is six exposure measures per township, three colonial and three wartime, differing only in the resolution of the underlying points.

5.5 Outcome and boundary harmonization

The primary outcome is the progressive vote share. Boundaries were redrawn many times after 1945, driven by urbanization, industrial siting, and political reorganization, all of which correlate with the vote, so boundary change is endogenous to the outcome. I address it by projecting every measure and every election onto one fixed 1945 township layer. I calculate vote shares only after spatially apportioning raw votes. This requires intersecting modern reporting units with 1945 boundaries, assigning votes by area proportion, and aggregating them within the historical townships. The analysis incorporates elections from 2002 to 2025 because they offer the township level data necessary to exploit subcounty variation. Returns from 1987 to 1997 are excluded since their district level aggregation would artificially impose within county homogeneity, and enter only a long-run robustness check that treats them at their native resolution (Appendix table A7). The final dataset encompasses six elections with township resolution, comprising 9,458 township election observations across 1,577 historical units.

6 Empirical strategy

For township i in county c , province p , and election year t , I estimate

$$Y_{ipt} = \beta' \mathbf{C}_i + \gamma' \mathbf{W}_i + \delta_t + \alpha_g + \varepsilon_{ipt}, \quad (2)$$

where Y_{ipt} is the progressive vote share in percentage points, $\mathbf{C}_i$ collects the standardized colonial exposures, $\mathbf{W}_i$ the standardized wartime exposures by perpetrator, δ_t is an election-year fixed effect, and α_g is a geographic fixed effect. Standard errors are clustered by 1945 county.

The choice of α_g is the central specification decision, and I report three versions rather than choosing one. The *pooled* specification includes no geographic fixed effect. The *province* specification compares townships within the same province, removing the Yeongnam–Honam

cleavage that dominates Korean electoral geography (Kwon, 2004; Moon, 2005). The *county* specification compares townships within the same 1945 county. These represent three unique estimands, and their diverging results yield important theoretical insights.

Applying county fixed effects absorbs an enormous share of spatial variation. The median 1945 county covers about 600 square kilometers, effectively a 14 kilometer radius, meaning that 20 and 30 kilometer spatial kernels project well outside county boundaries. Removing county means leaves barely one fifth of the exposure variation intact at a 20 kilometer radius. If an effect holds at the province level but fails at the county level, the dynamic is fundamentally operating across counties rather than within them. This restrictive specification interacts asymmetrically with the underlying archives. Colonial records contain township identifiers, ensuring that within county variation reflects the true spatial incidence of conscription. Because wartime records lack resolution below the city and county level, their within county variation reflects nothing more than distance to a county centroid. Consequently, county fixed effects test colonial measures against actual subcounty geography but test wartime measures against an artifact of measurement. I therefore treat the province specification as the baseline for wartime trauma, although all county estimates remain fully documented.

7 Results

The dependent variable across all models is the township level progressive vote share measured in percentage points for the six presidential elections from 2002 to 2025. The specifications include election year fixed effects and cluster all standard errors by the 1945 county. All exposure variables are statistically standardized to ensure that each coefficient indicates the predicted change in electoral support per one standard deviation increase in local trauma intensity, and $N = 9,458$ throughout. Except when spatial bandwidth is the primary subject of inquiry, the reported tables feature the 20 kilometer kernel.

Province fixed effects serve as the primary specification to address a structural feature of the Korean electoral landscape.⁷ Post democratization Korean politics are organized around a severe regional cleavage, contrasting a 75.5 percent progressive vote in the Honam region with a 23.3 percent share in the Yeongnam region (Kwon, 2004; Moon, 2005). By nesting within regions, province fixed effects thoroughly absorb this regional polarization and restrict identification to townships within the same province. Expanding the geographic scope to broader macro regional fixed effects yields consistent results. By consolidating the two Jeolla provinces into a single Honam bloc, the two Gyeongsang provinces into a Yeongnam bloc,

⁷Geography is fixed at 1945 boundaries, encompassing eight provinces: Gyeonggi, Gangwon, North and South Chungcheong, North and South Jeolla, and North and South Gyeongsang. Hwanghae townships south of the 38th parallel lack post armistice election returns and are excluded. Metropolitan cities were not yet independent entities, and Jeju remained part of South Jeolla until 1946.

and grouping the remaining provinces together, this expanded specification absorbs only the macro regional divide. Under these regional fixed effects, the U.S. coefficient reported below is +0.566, against +0.476 with province fixed effects (Table 2). Narrowing the geographic scope further to county fixed effects restricts comparisons to townships within the same 1945 county. This strict approach removes all between county differences but mechanically destroys most exposure variation, leaving only about one fifth intact at a 20 kilometer radius. Furthermore, because the wartime archival record lacks subcounty resolution, the surviving within county variation merely reflects distance to a county centroid rather than actual killing locations. Thus, while county level estimates are reported throughout, they serve strictly as tests of spatial scale rather than definitive evidence of effect existence.

7.1 Issue Ownership (H1)

H1 predicts that the electoral effect of wartime killing follows the specific political bloc that claims each perpetrator’s victims. Since the three wartime perpetrators are claimed by divergent blocs, an empirical corollary follows: a measure that pools all wartime killings will misrepresent the overall effect, understating its magnitude and concealing its source. Table 1 tests this corollary first by entering three pooled totals individually, mirroring how most research on the legacies of violence would incorporate them. Pooled colonial exposure and the comprehensive all trauma total predict nothing. Pooled wartime killing achieves significance within provinces (+0.447, $p < .05$), but this pooled coefficient is distinctly smaller than the U.S. specific estimate it dilutes (+0.476 in Table 2), and it conceals that the three underlying components differ in sign. A pooled design would merely report a modest generalized effect of wartime violence, whereas the disaggregated design demonstrates that the effect belongs strictly to one perpetrator.

Table 1: Pooled trauma exposure and the progressive vote

Measure entered alone	Province FE	County FE
Colonial: total mobilization	+0.350 (0.648)	+0.319 (0.349)
Wartime killing: all perpetrators	+0.447* (0.224)	+0.532 (0.270)
All historical trauma pooled	+0.563 (0.462)	+0.611 (0.311)

Note: Six separate regressions, each with election-year fixed effects. The last row standardizes the colonial and wartime layers separately, then sums and restandardizes. * $p < .05$, ** $p < .01$, *** $p < .001$.

Table 2 enters the three perpetrator measures simultaneously across three levels of geographic control. Within provinces, exposure to killings by U.S. and UN forces predicts a progressive vote share 0.476 points higher per standard deviation ($p < .01$). This matches the direction H1 predicts for a trauma claimed by the progressive bloc, and the estimate increases to +0.566 ($p < .01$) when only the regional divide is absorbed. The finding is also

highly stable regarding how the other perpetrators are handled; when entered alone, the coefficient is +0.517 ($p < .01$) and moves by less than half a standard error across every combination (Appendix Table A1). Because the three coefficients differ in sign and only one is precisely estimated, the pooled measure in Table 1 severely blurs what the disaggregated data reveals.

Table 2: Wartime killing by perpetrator (H1)

	Region FE	Province FE	County FE
War: state killing	+0.297 (0.415)	+0.338 (0.231)	+0.572* (0.233)
War: U.S. killing	+0.566** (0.175)	+0.476** (0.166)	+0.079 (0.359)
War: communist killing	-0.134 (0.664)	-0.394 (0.703)	+0.041 (0.131)

Note: Three regressions, each entering all three perpetrator measures simultaneously, with election-year fixed effects. Region fixed effects distinguish Honam, Yeongnam, and the rest of the country; estimates with region-by-year fixed effects are identical to three decimal places. * $p < .05$, ** $p < .01$, *** $p < .001$.

H1 is therefore supported for the perpetrator whose ownership is uncontested, leaving the two remaining perpetrators requiring separate readings. State killing is null within provinces (+0.338, $p = .14$), exactly as the scope condition in Section 4 anticipates. Its legacy is claimed by the progressive bloc but defended by the conservative bloc, meaning a trauma with contested ownership translates into votes much more weakly than one with a single owner. The only specification where state killing reaches significance is the county model (+0.572, $p < .05$). Because identification there rests on distance to the county centroid rather than the actual location of killings, I note this result without building on it. Communist killing carries the predicted negative sign within provinces, but its standard error (0.703) is more than four times that of the U.S. estimate. This occurs because communist killings are few (7,933 victims against 40,995 by state forces) and geographically concentrated, registering 0.50 victims per thousand in Honam against 0.02 in Yeongnam (Appendix Table A3), leaving few counties with the variation on which a county clustered estimate depends. The data do not reject H1 for communist killing; they simply cannot test it with precision. Furthermore, this electoral effect predates the investigations that produced the data. In the three elections from 1987 to 1997, before the TRCK existed, the U.S. estimate is +1.415 ($p < .001$; Appendix Table A7), demonstrating that the TRCK documented a memory that was already shaping votes rather than creating one.

7.2 Register (H2)

H2 predicts that colonial mobilization, whose redress is structured nationally against an external perpetrator, exhibits no local electoral variation, whereas wartime killings, whose redress is organized locally due to political suppression, drive distinct spatial differences. The empirical evidence for the colonial expectation appears in the first row of Table 1 and the

colonial rows of Table 4. Here, total mobilization and documented deaths predict nothing within provinces at any bandwidth (Appendix Table A4), whether entered independently or alongside wartime measures. Conversely, the wartime expectation is confirmed by the robust U.S. estimate in Table 2. This stark contrast perfectly aligns with H2. A geographic research design successfully captures traumas preserved village by village but structurally absorbs traumas elevated to a national scale. The colonial null result therefore reveals how state institutionalization has shaped colonial memory, rather than suggesting it lacks political weight.

Table 3: The full model estimated at every bandwidth (H3)

	5 km	10 km	20 km	30 km
War: U.S. killing	+0.280** (0.096)	+0.385** (0.123)	+0.505** (0.182)	+0.742** (0.229)
Colonial: disappearance	+0.475 (0.246)	+0.737 (0.442)	+1.183 (0.829)	+1.574 (1.091)

Note: Four regressions, one per bandwidth, each containing all six trauma measures with province and election-year fixed effects. The remaining four measures reach $p < .05$ at no bandwidth (Appendix Table A4). * $p < .05$, ** $p < .01$, *** $p < .001$.

7.3 Resolution (H3)

H3 predicts a crucial exception within the colonial historical record. While formal redress successfully closes the political claims surrounding total mobilization and documented deaths, disappearances remain fundamentally unresolved. Financial compensation issued to the family of an individual who never returned and whose death remains unconfirmed provides no genuine closure. Therefore, disappearances should exert a persistent local electoral effect, whereas the other colonial metrics should not. Table 3 estimates the complete six measure model across every kernel bandwidth and highlights the two specific variables central to this hypothesis. The empirical evidence for this predicted exception is directionally consistent yet statistically modest. The disappearance coefficient is positive across all bandwidths and reaches its maximum value relative to its standard error at the tight 5 kilometer radius (+0.475, SE 0.246, $p = .054$). Although it fails to cross the conventional significance threshold, total mobilization and deaths yield strict null results at all distances (Appendix Table A4). This specific short bandwidth pattern also appears in a separately constructed panel (Appendix Table A8), lending directional backing without absolute confirmation. The U.S. estimate behaves entirely differently, strengthening from +0.280 at 5 km to +0.742 at 30 km and remaining significant at every width. Consequently, I interpret H3 as receiving suggestive empirical backing exactly at the highly localized distances the theory implies, but nothing more definitive.

Table 4: Wartime estimates are invariant to the colonial measures included (H4)

	(1) war only	(2) + mobilization	(3) + deaths	(4) + disappearance	(5) + all three
Colonial: total mobilization		+0.347 (0.679)			−2.712 (1.773)
Colonial: deaths			+1.066 (0.822)		+2.681 (1.559)
Colonial: disappearance				+0.741 (0.402)	+1.183 (0.829)
War: state killing	+0.338 (0.231)	+0.250 (0.331)	+0.060 (0.343)	+0.202 (0.256)	+0.107 (0.342)
War: U.S. killing	+0.476** (0.166)	+0.494** (0.172)	+0.532** (0.170)	+0.493** (0.162)	+0.505** (0.182)
War: communist killing	−0.394 (0.703)	−0.418 (0.679)	−0.481 (0.685)	−0.374 (0.696)	−0.393 (0.615)

Note: Province and election-year fixed effects throughout. Standard errors, clustered by 1945 county, in parentheses. Total mobilization and deaths are highly collinear ($r = 0.84$), which is why their joint coefficients in column (5) are large and opposite in sign; entered singly, each is close to zero. * $p < .05$, ** $p < .01$, *** $p < .001$.

7.4 Coexistence (H4)

H4 predicts that these layered historical traumas shape electoral outcomes independently. Thus, introducing one trauma into the model should not alter the estimate of the other, and any interaction between them should equal zero. Table 4 tests the first part by adding the colonial measures to the perpetrator model one at a time and then together. The U.S. coefficient runs +0.476, +0.494, +0.532, +0.493, and +0.505 across the five columns, a spread of 0.056 points against a standard error of 0.166, and the state and communist coefficients are equally robust, so that including the colonial layer does not attenuate the wartime estimates. Table 5 tests the second part. The interaction between pooled colonial exposure and pooled wartime exposure is statistically insignificant (-0.339 , $p = .11$). Among the nine pairwise interactions, eight are indistinguishable from zero. While the ninth interaction between disappearance and U.S. killing appears significant initially (-0.378 , $p = .03$), does not survive the specification sweep: across 216 province-level fits varying outcome, bandwidth, and sample, 6 reach $p < .05$ (2.8 percent, chance level) and none reaches $p < .01$ (Appendix Table A5). Ultimately, the two traumas coexist geographically but mobilize voters on separate terms. This independence precisely matches the theoretical prediction for historical grievances that rely on different political owners and distinct registers of redress.

Table 5: Interactions between colonial and wartime exposure (H4)

Colonial measure	Wartime measure	Interaction	<i>p</i>
Total mobilization	State killing	−0.020 (0.305)	.95
Deaths	State killing	−0.262 (0.251)	.30
Disappearance	State killing	−0.404 (0.375)	.28
Total mobilization	U.S. killing	+0.297 (0.446)	.51
Deaths	U.S. killing	−0.519 (0.361)	.15
Disappearance	U.S. killing	−0.378* (0.171)	.03
Total mobilization	Communist killing	−1.444 (0.962)	.14
Deaths	Communist killing	+0.122 (0.459)	.79
Disappearance	Communist killing	−1.302 (1.038)	.21
All colonial (pooled)	All wartime (pooled)	−0.339 (0.213)	.11

Note: Each row is a separate regression at 20 km with province and election-year fixed effects, containing all six measures as main effects plus the listed interaction. The last row interacts the two pooled layers. The one interaction at $p < .05$ does not replicate across the specification sweep (Appendix Table A5). * $p < .05$, ** $p < .01$, *** $p < .001$.

8 Discussion

The results line up with the propositions unevenly, and the unevenness is worth stating before interpreting it. H1 is supported where ownership is uncontested and untestable with precision where exposure is too concentrated; H2 is supported by the contrast between the wartime effect and the colonial null; H3 receives directional but not conclusive support; H4 is supported in full. What remains is to ask what else could produce this pattern, and what evidence would separate the explanations. That is the purpose of this section.

8.1 The colonial null and testing the causal mechanism

Colonial forced mobilization, the trauma that anchors Korean historical politics, has no stable association once province or county fixed effects enter, at any bandwidth. Death in mobilization behaves the same way. Disappearance does not. Migration is one possibility. Mobilization removed people from their villages and many did not return (Palmer, 2013), so a place based measure of a trauma defined by displacement attenuates when the affected population disperses. This bears hardest on the mobilization count and least on disappearance, whose families stayed behind. No instrument for the initial allocation exists here, as it does for German expellees (Menon, 2023), and that is a real constraint on the design.

The reading the argument supplies is nationalization: mobilization is a national frame rather than a local inheritance, litigated in the Supreme Court, negotiated in treaties, and mobilized through nationwide boycotts (Jo, 2022; Ko and Kim, 2023; Arrington, 2016), and experimental reminders of colonial repression move Korean respondents emotionally regardless of where they live, without changing attitudes directly (Han, 2026). Alternatively,

the anti-Japan frame may function as a valence issue invoked by both blocs. For instance, in 2012, conservative President Lee Myung-bak visited the disputed Dokdo islets just before Liberation Day, explicitly tying the trip to Japan’s lack of apology for colonial rule (The Korea Times, 2012). Identity claims against Japan cut across the partisan divide in a way claims about the wartime state do not (Glosserman and Snyder, 2015). Both readings predict the same null for mobilization, since a grievance every party can invoke gives no locality a distinct partisan signal. They part on the rest of the archive: a valence account predicts the same null for every colonial measure, whereas this register-based account predicts the disappearance exception. The short-distance disappearance estimate therefore leans toward the register account, but its marginal significance means the valence reading cannot be closed here.

The long-run record argues against reading either result as an artifact of recent data. In the three elections of 1987 to 1997, before the TRCK existed and before transitional justice became government policy, the U.S. killing estimate is roughly three times its 2002–2025 value (+1.415, $p < .001$; Appendix Table A7). The progressive bloc’s claiming of these killings therefore did not create the local memory; it found a memory the localities already held and translated it into national partisan terms. The same ordering answers the most natural reverse-causality concern, that the commission investigated where progressives were already strong: the electoral trace was present two decades before the commission selected its first case. The estimates are also insensitive to how the historical geography was reconstructed, since an independently built panel returns the same coefficients to within a few hundredths (Appendix Table A8).

The disappearance was never nationalized and was never settled. The theoretical framework suggests it stays with the family and the village instead of entering a treaty dispute, and unresolved claims are what keep a legacy politically live (Menon, 2023). Communicative memory carries claims of exactly this kind (Assmann and Czaplicka, 1995), and it is local by construction. The bandwidth profile is consistent with this expectation, though the evidence is suggestive rather than conclusive: the disappearance estimate is positive at every bandwidth, clearest relative to its uncertainty at 5 km, where it falls just short of conventional significance ($p = .054$), and the same short-distance profile appears in the independently constructed panel. The wartime measure behaves in the opposite way, strengthening as the kernel widens. The test is a demanding one, since it asks whether the smallest measure in the data still marks a five-kilometer neighborhood after seventy years of urbanization, and finer-grained sources, including the second round of TRCK petitions, are what would settle it.

The geographic contrast relies on ecological evidence. To test transmission mechanism directly, a survey experiment ($N = 2,000$) has been designed and is currently awaiting IRB approval before fielding. The instrument records each respondent’s migration history, where they lived during their political formative years and where their parents came from, and

assigns colonial and wartime primes. The design yields a clean division of outcomes. If treatment effects are heterogeneous, stronger for respondents whose formative years or parental origins lie in exposed localities, transmission is localized, running through family and community, which is what the register account requires. If effects are uniform across origins, the memory operates as a national frame, which is what the valence account predicts. Either result identifies the channel this aggregate design can only bound.

8.2 Why the U.S. perpetrator

The surviving result is not the one the descriptive record would predict, and the reason is ownership rather than scale. State killings are the largest category, 40,995 of 57,742 victims, and indict the anticommunist order most directly, yet the smaller U.S. and UN category is what leaves a mark. Both camps claim the anticommunist state’s legacy in some form: the conservative bloc defends it, the progressive bloc has pursued transitional justice for it, and that justice has been intermittent, with the commission’s standing shifting by administration (Kim, 2013). Killings by U.S. forces instead map onto a stable and asymmetric divide over the alliance, criticism of which is a durable strand of Korean progressive politics (Jhee, 2008). Where a frame is stably owned, local exposure can translate into votes; where ownership is contested, it cannot (Petrocik, 1996; Bélanger and Meguid, 2008). Commemoration is the other candidate, since No Gun Ri became a national story with a place attached (Hanley et al., 2001), and that infrastructure is what lets a legacy survive (Jelin, 2003; Villamil, 2021; Cremaschi and Masullo, 2025). These data cannot separate the two, which is why the next stage geocodes memorials directly.

8.3 Limitations

These are associations. Places where U.S. forces killed civilians differ from others in ways correlated with terrain, transport corridors, and the path of the 1950 retreat, and nothing here delivers as if randomization (Kocher and Monteiro, 2016). The wartime record is also selected, covering cases families reported and a time limited commission investigated, so if reporting propensity correlates with contemporary alignment, fixed effects will not repair it.

Clustering by 1945 county absorbs spatial dependence at the scale of the historical county, but finer-scale spatial correlation may remain, and no correction for it is attempted here. The long-run evidence carries its own caveat: returns for 1987 to 1997 exist only at the city-county level and are apportioned to townships by area, so the early estimates rest on coarser geography than the main analysis.

Two further points bound what the nulls mean. The estimator uses between township contrast, so a measure with little of it cannot be distinguished from a measure with no effect; I read the mobilization null as the absence of a detectable geographic gradient rather than as

evidence that colonial memory fails to move votes. And an aggregate design cannot identify who is responding or why; the survey experiment described above is designed to recover what this design can only bound.

9 Conclusion

Layered historical traumas retain their political force. Seventy years after the violence, exposure to wartime killings by U.S. forces still moves the vote in the townships where the killings happened, and it moved the vote before the state ever investigated them. The colonial layer, larger by an order of magnitude, moves nothing locally, not because it is forgotten but because its memory is kept at the national level. Which trauma a locality votes on depends on who claims it, where its memory is kept, and whether it can be settled, and traumas that differ on these properties coexist in the same townships without erasing one another.

The paper’s second contribution is the agenda it makes testable. The mechanisms behind each property are now concrete research targets: the survey experiment described above separates family transmission from national framing; geocoding memorials separates commemoration from ownership; finer petition data can settle the resolution hypothesis that this design leaves marginal. The village-level conscription dataset released with this paper, and a transcription procedure that transfers to similar ledgers across the former Japanese empire, make the same questions answerable outside Korea. Most places carry more than one story of trauma; this paper provides the framework and the data to study how those stories coexist.

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Appendix

Table A1: Each perpetrator entered alone versus jointly

	Province FE		County FE	
	alone	joint	alone	joint
War: state killing	+0.306 (0.211)	+0.338 (0.231)	+0.585* (0.225)	+0.572* (0.233)
War: U.S. killing	+0.517** (0.164)	+0.476** (0.166)	+0.111 (0.347)	+0.079 (0.359)
War: communist killing	−0.351 (0.688)	−0.394 (0.703)	+0.164 (0.127)	+0.041 (0.131)

Note: “Alone” columns are separate regressions with one perpetrator measure; “joint” columns enter all three, as in Table 2. * $p < .05$, ** $p < .01$, *** $p < .001$.

Table A2: The two central measures across the geographic-control ladder, full six-measure model

	Region FE	Region $\times$ year FE	Province FE	County FE
War: U.S. killing	+0.580** (0.177)	+0.580** (0.177)	+0.505** (0.182)	+0.098 (0.361)
Colonial: disappearance	+0.470 (0.606)	+0.470 (0.607)	+1.183 (0.829)	−0.075 (0.276)

Note: Four regressions, each containing all six trauma measures at 20 km. Region distinguishes Honam, Yeongnam, and the rest of the country. * $p < .05$, ** $p < .01$, *** $p < .001$.

Table A3: Regional concentration and within-province variation of wartime exposure

	Honam victims per	Yeongnam 1,000 residents,	Other 20 km	Within-province SD share of total SD
War: state killing	1.44	2.24	1.20	0.97
War: U.S. killing	0.05	0.34	0.67	0.97
War: communist killing	0.50	0.02	0.32	0.92

Note: Township-level raw intensities. The last column is the standard deviation of the standardized measure after province fixed effects, divided by its unconditional standard deviation.

Table A4: All six measures at every bandwidth, full model with province fixed effects

	5 km	10 km	20 km	30 km
Colonial: total mobilization	−1.175 (0.901)	−1.561 (1.192)	−2.712 (1.773)	−3.543 (2.027)
Colonial: deaths	+1.086 (0.757)	+1.666 (1.090)	+2.681 (1.559)	+3.520 (1.855)
Colonial: disappearance	+0.475 (0.246)	+0.737 (0.442)	+1.183 (0.829)	+1.574 (1.091)
War: state killing	−0.050 (0.221)	+0.143 (0.239)	+0.107 (0.342)	−0.398 (0.455)
War: U.S. killing	+0.280** (0.096)	+0.385** (0.123)	+0.505** (0.182)	+0.742** (0.229)
War: communist killing	−0.210 (0.216)	−0.106 (0.300)	−0.393 (0.615)	−0.359 (0.737)

Note: Four regressions, one per bandwidth, each containing all six measures. Standard errors, clustered by 1945 county, in parentheses. Total mobilization and deaths are highly collinear ($r = 0.84$); entered singly each is close to zero (Table 4). * $p < .05$, ** $p < .01$, *** $p < .001$.

Table A5: Significance of colonial $\times$ wartime interactions across the specification sweep

Geographic control	Fits	$p < .05$	Share	$p < .01$
Province FE	216	6	.028	0
County FE	216	32	.148	6

Note: Each fit interacts one colonial measure with one wartime measure (nine pairs) for three progressive outcomes, four bandwidths, and two samples. Under the null of no interaction, 5 percent of fits reach $p < .05$ by chance. The six province-level fits at $p < .05$ fall in two blocks, disappearance $\times$ U.S. killing at 20 km (2 of 6 cells, -0.386 , SE 0.183) and at 30 km (4 of 6 cells, -0.672 , SE 0.297); no block is significant in every cell and none reaches $p < .01$. The elevated county-level share arises where within-county identification rests on distance to the county centroid and is not interpreted.

Table A6: The U.S. estimate across outcomes, bandwidths, and samples

Outcome	Sample	5 km	10 km	20 km	30 km
Main candidate share	full	+0.269*** (0.069)	+0.362*** (0.100)	+0.517** (0.164)	+0.729*** (0.210)
Progressive camp	full	+0.301*** (0.077)	+0.397*** (0.111)	+0.555** (0.175)	+0.779*** (0.227)
Two-party share	full	+0.337*** (0.082)	+0.446*** (0.118)	+0.632** (0.193)	+0.891*** (0.249)
Main candidate share	cov. $\geq .5$	+0.264*** (0.070)	+0.358*** (0.102)	+0.511** (0.166)	+0.723*** (0.213)
Progressive camp	cov. $\geq .5$	+0.296*** (0.079)	+0.392*** (0.113)	+0.549** (0.178)	+0.773** (0.231)
Two-party share	cov. $\geq .5$	+0.332*** (0.083)	+0.442*** (0.121)	+0.628** (0.195)	+0.889*** (0.254)

Note: Single-measure regressions with province and election-year fixed effects. Standard errors, clustered by 1945 county, in parentheses. Outcomes: main progressive candidate share of valid votes; progressive camp share; progressive share of the two-party vote. Samples: full panel; unit-years with 1945 polygon coverage ≥ 0.5 . * $p < .05$, ** $p < .01$, *** $p < .001$.

Table A7: The perpetrator model over nine elections, 1987–2025

Sample	State	U.S.	Communist	N
1987–1997 (sigungu-apportioned)	−0.087 (0.235)	+1.415*** (0.159)	−0.267 (1.011)	4,416
2002–2025 (township returns)	+0.338 (0.230)	+0.470** (0.168)	−0.392 (0.706)	9,301
1987–2025 (nine elections)	+0.199 (0.222)	+0.768*** (0.157)	−0.341 (0.779)	13,717

Note: Joint three-perpetrator model, province and election-year fixed effects, 20 km, coverage ≥ 0.5 . Standard errors, clustered by 1945 county, in parentheses. Returns before 2002 exist only at the city-county level and are apportioned to 1945 townships by area, so the early sample is coarser and county fixed effects are not available for it. * $p < .05$, ** $p < .01$, *** $p < .001$.

Table A8: Cross-pipeline validation: headline estimates on an independently constructed panel

	Main panel (areal, R)	Validation panel (Python)
U.S. killing, joint model	+0.476** (0.166)	+0.463** (0.170)
U.S. killing, entered alone	+0.517** (0.164)	+0.507** (0.168)
U.S. killing, full six-measure	+0.505** (0.182)	+0.485** (0.186)
Disappearance, full model, 10 km	+0.737 (0.442)	+0.753 (0.440)
U.S. killing, nine elections	+0.768*** (0.157)	+0.696*** (0.150)

Note: The validation panel was constructed independently in a separate pipeline: it parses the same election returns and joins them to the same 1945 layer, but with its own unit matching (1,539 townships, $N = 9,234$ for 2002–2025) and stores allocated vote counts directly. Estimates use the same specification as the main text; the nine-election row uses the coverage ≥ 0.5 sample on the main panel and the full panel on the validation panel. Agreement across the two constructions indicates that the results do not depend on the apportionment implementation. * $p < .05$, ** $p < .01$, *** $p < .001$.