# Civilian Killings and Long-Run Development: Evidence from the Korean War*

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**Abstract**

This study examines the economic legacy of civilian killings during the Korean War, which disproportionately targeted local elites, educated individuals, and their families. For identification, we exploit plausibly exogenous variation in the spatial distribution of killings driven by unanticipated UN military operations. Evidence suggests that local exposure to civilian killings had a persistently negative impact on contemporary development. These effects are not driven by immediate, direct influence on local economic outcomes. Instead, we highlight one possible explanation: targeted killings appear to have discouraged educational investment, leading to reduced human capital accumulation and slower structural transformation across generations. Our findings shed light on the potential long-term legacies of (small-scale) strategic killings.

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# 1 Introduction

Conflicts are viewed as inherently destructive to the economy (Gates et al., 2012). Yet, there is little consensus about the long-term relationship between conflict and development. Much of the literature, focusing on the effects of large-scale bombings and capital destruction, has found no long-lasting effects (Feigenbaum et al., 2022; Davis and Weinstein, 2002; Miguel and Roland, 2011).[1] In contrast, many modern conflicts are characterized less by indiscriminate violence but more by selective violence: the targeting of specific individuals on the basis of strategic calculations.[2] While a series of studies have examined the conditions under which armed actors resort to selective violence (Kalyvas, 2006; Balcells, 2010; Downes, 2011; Bertazzini and Giorcelli, 2022), we know much less about their long-term development consequences.

This paper asks: What are the long-run effects of civilian killings that selectively targeted individuals with high education and social status? We hypothesize that targeted killings, despite their small number, may have had a lasting effect on economic development by amplifying broader negative externalities from the destruction of human capital. To answer this question, we examine a critical juncture during the Korean War (1950-1953). The war began when North Korean forces, backed by the Soviet Union, invaded South Korea. Although the peninsula was defended by the United States and a coalition of United Nations (UN) forces, North Korean forces nearly overran the entire peninsula. The tide turned with an unanticipated UN counter-offensive (the *Incheon Landing*), which cut off North Korean supply lines and forced a disorganized retreat, during which the North Koreans carried out targeted civilian killings. For identification, we leverage distance to a transitory offensive line formed by this counter-offensive as a plausibly exogenous shock to the spatial distribution of killing intensity over a brief, 2-3 week period. In terms of data, we digitize a hitherto unused source on the location of victims (Statistics Division of Republic of Korea, 1952), constructing a disaggregated township level dataset on the spatial distribution of killings, overcoming a key data limitation in the existing literature.

This study focuses on the effects of targeted civilian killings perpetrated by North Korean forces. Three key characteristics of these killings make them well-suited for answering our

[1] A notable exception is Riaño and Valencia Caicedo (2024) who finds a long-term negative effect of unexploded ordinances in Laos on economic development and structural transformation.

[2] For example, Pol Pot's targeting and massacre of Cambodians and Stalin's reign of terror, repression, and mass killings. The broader political science literature has studied the long-term political effects of Stalin-era repression (Zhukov and Talibova, 2018; Rozenas et al., 2017; Rozenas and Zhukov, 2019). In the economics literature, Rozenas et al. (2024) studies the effects of Stalin-era repression on the combat motivation of Red Army soldiers. Bühler and Madestam (2023) shows that greater exposure to Khmer Rouge killings led to higher anti-authoritarian vote shares and political competition, four decades later.

question. First, the majority of casualties occurred over a short period due to sudden shifts in the course of the war. Second, historical records suggest that North Korean killings primarily targeted local elites, intellectuals, and their families (Truth and Reconciliation Commission of Republic of Korea, 2010). This distinction allows us to better isolate the effects of targeted killings from other war-related consequences.[3] Last but not least, the number of civilian killings were relatively small (the average number of killings in a township was 1.73 per 1,000 people), suggesting that any effects we find are unlikely to result from the direct loss of population.

Building on these historical accounts, we collect and digitize a comprehensive set of individual-level records on killings that occurred during the Korean War. These records include geographic information on civilians reported killed by North Korean forces. We link this dataset to a wide range of development outcomes, including nightlight intensity, sectoral composition, industrial productivity, human capital, and demographics across time.

To address the potential endogeneity of the location and intensity of killings, we employ an instrumental variable (IV) that exploits two key regional differences shaped by the Incheon Landing, which influenced the spatial distribution of North Korean killings. First, we leverage the initial shape of a temporary offensive front-line (hereafter, the *offensive line*) formed by the meeting of the United Nations (UN) counter-offensive operations launched from Incheon (northeastern) and Busan (southwestern) (Figure 1). On September 15th 1950, a successful UN counteroffensive spearheaded by the Incheon Landing enabled (i) fresh UN forces from Incheon to march on to Seoul and (ii) embattled UN forces at Busan to break out from the Busan Perimeter and advance on to Seoul. These two pincer actions formed a temporary offensive line connecting the northwest (Seoul) and southeast (Busan) as UN and South Korean forces took control of key towns along this line.

Second, targeted killings by North Korean forces largely occurred west of the offensive line, with the intensity of killings increasing with westward distance from the line. Following the formation of the offensive line, North Korean authorities ordered retreating units to "eliminate all elements that could support the UN forces upon landing." The extent to which these orders were carried out varied depending on the retreat routes available to units on either side of the line. Units east of the line had viable overland retreat routes to North Korea due to the geographical contiguity between contested areas east of the line and North

[3]South Korean authorities were also responsible for perpetrating civilian killings within their own territory. However, civilian massacres perpetrated by South Korean forces differed from North Korean killings in two important respects. First, killings by South Korean were focused on the execution of *alleged* Communist sympathisers, rather than targeting individuals based on education or elite status. Second, these killings occurred selectively over an extended period, making causal inference challenging.

Korean territory, and largely prioritized withdrawal over compliance with kill orders. Units west of the line, trapped between the offensive line and the Yellow Sea with no safe overland retreat routes, more fully carried out the orders to eliminate targeted civilians.

Drawing on these historical accounts, our instrument is constructed as the interaction between distance to the offensive line and an indicator for whether a township was located West of the line. The validity of our identification strategy rests on two conditions. First, the formation and shape of the offensive line was unanticipated by North Korean forces. This assumption appears valid ex-ante given that the *Incheon Landing* was widely deemed improbable and was executed with extreme urgency and secrecy: while an amphibious attack was considered imminent, the time and location were undisclosed (Hanley, 2020). Second, townships further west of the offensive line experienced more intense killings. Our data supports this: as distance to the offensive line increases, townships west of the line experienced significantly higher killing intensity, while no such pattern is observed for townships east of the line.

A potential concern is that townships located further west of the offensive line may have experienced more civilian killings due to pre-existing local characteristics associated with economic development. For example, if such townships initially had more educated individuals, this could be linked to disproportionate targeting by retreating North Korean forces and to subsequent local development through a separate channel. To address such concerns, we show that our instrument is not correlated with a host of pre-war local characteristics likely to affect economic development, which are also included as controls in our specifications.

A related concern is that townships further west of the offensive line may have been occupied by North Korean forces for a longer duration. However, this is unlikely for three reasons. First, nearly the entire South Korean peninsula was under North Korean control for a short duration of two months prior to the Incheon Landing. Second, Allied forces liberated the rest of the country within two to three weeks, limiting variation in post-Incheon occupation to a matter of days. Third, our baseline results are robust to restricting the sample to townships in a narrow bandwidth around the offensive line for which variation in occupation duration, if any, would have been minimal. Overall, these suggest that our instrument is unlikely to reflect substantive differences in occupation duration.

Our evidence shows that, despite the absence of any immediate post-war demographic impact, civilian killings had a persistently negative effect on local development. Townships exposed to higher civilian killings continue to exhibit lower nightlight intensity and reduced industrial productivity today. We check robustness of these findings across various contexts, including alternative sample compositions and additional controls. Taken together,

our findings suggest that targeted civilian killings had a long-lasting negative impact on local economic development.

Given that North Korean forces disproportionately targeted elites, educated individuals, and their families, we hypothesize that a decline in demand for human capital accumulation was possibly a key driver of the long-term negative impacts of killings on development. Targeted killings may have discouraged investment in education through several channels: a decline in the perceived value of education due to the observed risk of violent persecution for the educated; the loss of educated networks, weakening positive externalities that encourage educational investment; or a broader erosion of confidence in education as a pathway to social mobility. If these channels hindered human capital formation in the postwar period, long-term effects on local development would follow.

In support of this, a cohort-level difference-in-differences analysis shows that negative effects on human capital accumulation (college education) emerged as early as the first cohort that grew up during the Korean War and have persisted to the present. Evidence from contemporary survey data is consistent with the demand-side channel: higher killing intensity predicts a significantly lower propensity to perceive education as a desirable determinant of wages. This pattern extends beyond labor market perceptions: when seeking advice on important matters, respondents in areas with higher killing intensity are less likely to seek out a college graduate for such advice, suggesting reduced value placed on educational background. The negative effects on attitudes towards education are consistent with, though do not prove, a decline in the perceived value of higher educational attainment as a key mechanism of our findings.

In addition, we examine the possibility of potential alternative mechanisms. First, supply-side shocks alone do not appear to explain our results. We find a small negative effect on the number of teachers in the immediate post–war period, but its magnitude is too small to account for the sharp decline in observed school enrollment rates. Second, we find no significant association between the intensity of killings and proximate indicators of local development in the immediate post-war period (the share of employment in manufacturing and in high-skilled occupations), which could have in turn affected human capital structures.

We also consider alternative pathways not directly related to human capital accumulation, but which might be important given the extant literature on conflict and development (Cassar et al., 2013; Dell and Querubin, 2018; Tur-Prats and Valencia Caicedo, 2020): (i) government policies; (ii) trust; and (iii) selective migration. We find little evidence that targeted killings led to differences in the receipt of central government programs (given local elites might have played an important role in channeling resources) or to a meaningful decline in social

trust. A heterogeneity analysis based on road access further shows that negative effects are not concentrated in areas from which it was easier to out-migrate. However, we do find indirect evidence of selective out-migration emerging in the mid-1990s, long after we observe initial negative effects on development. The timing suggests that selective out-migration more plausibly reinforced, rather than initiated, the observed divergence.

Lastly, we document negative effects of killing intensity on structural transformation, with a decline in human capital accumulation serving as a potential channel. The timing of evidence supports the role of human capital. The effect is not evident for 1970 prevalence but becomes significant by 1980, with higher killing intensity associated with a significantly larger agricultural sector. This pattern is consistent with the interpretation that the legacy of killings delayed structural transformation as affected cohorts reached working age. We further show that the negative effects of killings on manufacturing employment are concentrated in human capital-intensive industries, with no significant effects on less skill-intensive sectors.

Our findings extend the literature on the impacts of violence, such as mass killings or forced relocations, on human capital and long-run development (Acemoglu et al., 2011; Esteban et al., 2015; Bertoni et al., 2019; Fergusson et al., 2020; Huber et al., 2021). Closely related to our study, a growing literature highlights the impact of targeted violence against educated elites. For example, Grasse (2023) finds long-term negative effects of Khmer Rouge killings in Cambodia on human capital and village development. Bühler and Madestam (2023) shows that exposure to such killings persistently shaped political attitudes through a "fear of violence" channel. Conversely, areas that received expelled educated individuals are shown to experience persistent gains in human capital and economic growth (Becker et al., 2020; Toews and Vézina, 2025).[4] Building on these studies, we show how small-scale targeted killings can inhibit long-run development despite little immediate demographic impact.

This paper also adds to the literature on historical conflicts in Asia (Miguel and Roland, 2011; Dell et al., 2018; Dell and Querubin, 2018), which have been profoundly shaped by colonial rule and ideological conflict between Western powers.[5] The economic legacy of the Korean War, a key theater of the Cold War, remains understudied yet offers a valuable setting for examining how conflict can shape long-run development through its effects on structural

[4] A related body of work shows that the introduction of human capital through missionary-established schools generated lasting gains in educational attainment and development (Becker and Woessmann, 2009; Caicedo, 2018). Our findings speak to the same question from the opposite direction, reinforcing the importance of intergenerational transmission in perpetuating initial human capital shocks.

[5] While our analysis focuses on civilian killings during the Korean War (1950–1953), Korean society was also shaped by coercive institutions and policies imposed during the Japanese colonial period (1910–1945). A systematic analysis of the long-run effects of colonial-era institutions on Korean development remains an important avenue for future research—especially given recent evidence of the impact of Japanese military aggression on institutional change in China (Chen and Kung, 2026).

transformation.[6] We contribute in two ways. First, we digitize and geolocate a novel dataset of individual victim-killings at the village level, which represents the most spatially and temporally disaggregated dataset on wartime violence during the Korean War. Second, leveraging South Korea's rapid, postwar structural transformation, we provide the first quantitative evidence of the economic legacy of the Korean War. We show that targeted violence can generate persistent within-country divergence by discouraging human-capital accumulation and slowing the growth of higher value-added industries (Ciccone and Papaioannou, 2009). In doing so, our results speak to a longstanding question about the sources of South Korea's growth, providing quasi-experimental evidence that the accumulation of upper-tail human capital was an important input into local development.

The remainder of the paper proceeds as follows. Section 2 provides the historical background of the Korean War, with particular attention to the Incheon Landing and the formation of the UN offensive line. Section 3 describes the newly digitized dataset on civilian killings and outlines the identification strategy. Section 4 presents the main results on the long-run impacts of civilian killings on local development. Section 5 investigates potential mechanisms, emphasizing the role of human capital discouragement. Section 6 concludes.

# 2 Historical Background

This section describes the historical background of the Korean War. Following a general overview of the war, we discuss the formation of the UN offensive line in greater detail, which forms the basis of the identification strategy.

**The Progression of the Korean War (June 25th 1950 - July 27th 1953)** At the end of World War II, Korea was divided along the 38th Parallel, an internal border between North and South Korea. The South was occupied by American forces, while the North came under Soviet control. In 1948, separate elections were held in the North and South, leading to the establishment of two separate governments in the two Koreas. Political tensions subsequently escalated, culminating in an all-out war with the invasion of South Korea by North Korean armed forces on June 25, 1950.

Within the first two months, South Korean and U.S. forces were pushed back to a small area in the Southeast of the Korean peninsula. However, on September 15, 1950, United Nations forces launched an amphibious landing at the port of Incheon near Seoul (hereafter referred

[6] Existing research has largely focused on the long-term effects of the Korean War on contemporary attitudinal and political outcomes (Hong and Kang, 2017; Joo, 2023).

to as the Incheon Landing). This operation reversed the course of the war, cutting off North Korean troops and supply lines in South Korea, which forced the North Korean army into a disorganized retreat up the peninsula.[7]

Up until July 1951, both sides alternated between attacking and defending territory in the central regions of the Korean Peninsula. From July 1951, protracted peace negotiations began and there were few subsequent territorial changes. During this phase, the conflict devolved into a series of localized battles ("hill warfare") where both sides contested strategically insignificant hills under grueling trench-like conditions. Major battles were absent during this period, as the focus shifted to maintaining defensive lines and wearing down the opponent. The war concluded on 27 July 1953 with the signing of the Korean Armistice Agreement.

**The Incheon Landing (September 15th 1950) and the UN Offensive Line** The UN offensive line was formed as a result of two military operations, the Incheon Landing and the Battle of Busan Perimeter. Shortly after the Korean War began, defeated UN forces were pushed back to the Busan Perimeter, a temporary defensive line around Busan (a port city on the southeastern tip of the Korean Peninsula). At this point, UN forces were running short on both men and supplies and their only hope of turning the tide was a much-touted amphibious assault. Importantly, however, the exact timing and location of this assault was kept completely secret and unknown outside of the American army's top brass (Hanley, 2020)

On September 15th 1950, the successful UN counteroffensive spearheaded by the Incheon Landing led to huge casualties in the North Korean forces stationed at Incheon and the surrounding environs, enabling (i) fresh UN forces from Incheon to march on to Seoul (ii) embattled UN forces at Busan to break out from the Busan Perimeter and advance on to Seoul. Together, these two pincer actions formed a temporary offensive line connecting the northwest and southeast of the South Korean Peninsular. Importantly, the offensive line formed the basis from which UN forces began recapturing vital territory and pushing the depleted North Korean forces towards the West and North.

Figure 1 depicts the offensive line made by United Nations forces following the Incheon Landing. This process depended entirely on the consequences of Operation Chromite (official name of the military operation that initiated the Incheon Landing), which was highly

[7]Following the Incheon Landing, Allied forces nearly succeeded in reclaiming the Korean Peninsula. However, this progress was halted in November 1950 when China entered the war. The Chinese People's Volunteer Army launched a massive counteroffensive, driving UN forces into a rapid retreat. Chinese troops recaptured Pyongyang and pushed UN forces south of the 38th Parallel, ultimately retaking Seoul in January 1951.

unpredictable due to its extremely low perceived likelihood of success. Douglas MacArthur, the Commander-in-Chief Far East (CINCFE), estimated the chances of success at 1 in 5,000 due to unfavorable military and geographic conditions at the time (Strain, Patrick M and US Army Command and General Staff, 1993). General Edward Almond, who commanded the Incheon landing, later remarked that Incheon was the worst possible location for an amphibious assault (Heinl, 1967).

The Incheon Landing occurred at an extremely rapid pace. Operation Chromite began on September 15th 1950, with UN forces recapturing Seoul on September 28 and advancing into North Korean territory by early October. The lack of forecasting, deceptive operations, and swift deployment completely surprised North Korean forces, providing an ideal setting for identification which we discuss in detail in Section 3.2.

**Civilian Killings and the UN Offensive Line** The Korean War had a higher proportion of civilian deaths than other major modern conflicts, including World War II and the Vietnam War (Lewy, 1980). While most civilian deaths were unintended war casualties, both North and South Korean armed forces also engaged in deliberate killings and executions of civilians. In this section, we focus on the key characteristics of North Korean killings which explicitly targeted more educated or elite civilians and their families (Kim, 2002). In contrast, South Korean killings were mainly committed based on ideological grounds.[8]

The majority of civilian killings by North Korean forces occurred immediately after the Incheon Landing. Following Operation Chromite and the subsequent South Korean advance, North Korean forces retreated, while escalating violence against civilians. A South Korean government survey in 1952 estimated the total number of civilian killings by North Korean forces to be almost 60,000,[9] with 60% inflicted during this period.

Importantly, the civilian killings carried out after the Incheon Landing differed in two ways from those committed at the onset of the war. First, their spatial distribution was shaped by the UN offensive line, which determined the feasible retreat routes for North Korean forces. The rapid UN counteroffensive brought the towns connecting the northwest (Seoul) and the

[8]The civilian massacres carried out by South Korean authorities during the Korean War were often directed at unspecified groups, purportedly based on ideological grounds, occurring under the orders or tacit approval by the government. The Syngman Rhee administration issued directives targeting "Bodo League" members nationwide on charges of collaborating with enemy forces, resulting in the deaths of an estimated 60,000 to 200,000 civilians (Kim, 2004). However, many of those identified as Bodo League members lacked ideological ties, with some victims even not officially listed as members (Kim, 2002). In Section 4.3, we show that our results are robust to controlling for the intensity of killings perpetuated by South Korean authorities.

[9]Author's calculations from Statistics Division of Republic of Korea (1952), the main source of our baseline dataset on civilian killings by North Korean forces. We describe this data source in Section 3.

southeast (Busan) under UN and South Korean control. This corridor was the principal axis capable of supporting an organized withdrawal with heavy equipment, and its closure left units to the west without a viable overland route north (Appleman, 1961). These units abandoned their equipment and dispersed into the interior (Merrill, 1989). Moreover, the subsequent expansion of the UN offensive line over the following two weeks (Yoo, 2010) pushed North Korean forces farther outward, displacing the killings westward from the line.

Second, under mounting pressure from the UN operations, the North Korean authority issued a retreat order, instructing their troops to "*eliminate all elements that could support the UN forces upon landing* (Kim, 1984)." The retreating units operated under the assumption that the advancing enemy could use the civilians left behind to staff an occupation administration, mobilize the local population, or lend legitimacy to the new regime. This notion underpinned a doctrine of "preventive violence," the execution of non-combatants whose position or skills could serve UN forces (Kwon, 2020). Unlike earlier killings aimed at facilitating rapid advancement, the incidents after September 1950 therefore targeted individuals who might potentially aid the ROK and UN forces, such as local elites, highly educated individuals, or those fluent in foreign languages. Killings extended beyond these identified "elements," as many individuals were killed simply because they were family members or associates of suspected supporters of the allied forces (Kim, 2000; Yim, 2020).[10]

Reports from the Truth Commission (Truth and Reconciliation Commission of Republic of Korea, 2010) provide qualitative evidence for the targeted nature of civilian killings towards elites, educated individuals, and their families. Produced in response to requests from the families of victims and concerned parties, these reports examined individual cases of civilian killings, offering a clearer understanding of the circumstances under which these killings took place. One report identifies the primary reason as "the victims being suspected of collaborating with UN and South Korean forces or perceived as likely to do so." In another, an investigation into a mass killing in *Cheoggye-myeon* reveals that the victims were predominantly individuals with prior government service experience, such as township clerks and former police officers, as well as those who were affluent or had affiliations with right-wing organizations.

The reports further suggest that many individuals were killed simply due to their relationships with targeted elites and educated individuals. For example, victims were "persecuted

[10]Beyond the incentives, selective violence requires an apparatus to collect and verify local information (Kalyvas, 2006). To this end, North Korean troops established temporary internal affairs offices and stations (Jung, 2012), which identified targets by position, including landowners, police officers, civil servants, judicial officials, members of right-wing organizations, and church leaders (Truth and Reconciliation Commission of Republic of Korea, 2010).

because of their associations with village leaders and government officials." In one case, "· · · individuals were executed near their homes by partisans using bamboo spears solely because they were family members of a village leader." Similarly, the head of a regional financial cooperative and his wife were killed because a prosecutor was part of his wife's family. Another account describes "an individual who was taken by partisans and executed near a riverside because his eldest son was an employee at the township office." There was also a case in which an entire family was killed allegedly because the two sons had received college education and organized a welcome event for the Allied forces. A surviving relative reportedly remarked, "Educating the eldest son well ended up causing their death."

Quantitatively, Appendix E.1 documents a general pattern based on textual analysis of the universe of these reports: victims, or their family members, were disproportionately well-educated. Using occupation as a proxy for educational attainment, professional, technical, and clerical workers, who constituted only 2.7% of the rural workforce at the time, account for 33.4% of victims with known occupations, a figure that rises to 53.9% when immediate family members are included. These patterns are difficult to reconcile with indiscriminate violence, supporting the premise that civilian killings by retreating North Korean forces targeted the educated minority.

Given qualitative accounts of the targeted nature of civilian killings towards elites, educated individuals, and their families, we hypothesize that our findings on the persistent negative effects of killing intensity on local development likely reflect the consequences of a reduced demand for human capital in the post-war period. In Section 5, we provide suggestive evidence supporting this view.

# 3 Data and Empirical Strategy

This section discusses our data sources and identification strategy. Section 3.1 introduces the archival records of civilian killings that we collect and digitize. In Section 3.2, we propose an instrumental variable strategy, which exploits exogenous variation in the local intensity of killings due to unexpected changes in the course of the war.

## 3.1 Archival Records of Civilian Killings

The regional distribution of civilian casualties during the Korean War has not been well documented, except for a few qualitative studies (National Committee for Investigating Civilian Massacres Before and After the Korean War, 2005; Kim, 2010). For our analysis, we digitize

the "List of Noncombatants Killed by North Korean Forces" (Statistics Division of Republic of Korea, 1952), a comprehensive dataset on civilian killings that has not been previously used in the literature. Published by the South Korean Government's Office of Statistics in 1952, this report provides a comprehensive list of civilians killed by North Korean Forces.[11] Crucially, it records the location and date of death of victims, allowing us to capture both inter-temporal and spatial variation in civilian killings, which is key to our identification strategy.

The total number of victims recorded is 59,994.[12] The 1955 Statistical Yearbook of the Republic of Korea (Statistics Division of Republic of Korea, 1955) estimates the number of civilian killings by North Korean forces to be around 120,000. However, the Yearbook's estimate could have exaggerated the scale of killings. Since civilian killings were classified as war crimes, South Korean authorities had an incentive to inflate the number of victims to gain legitimacy from the international community and hold the North Korean government accountable (Kong, 2015). In this context, our dataset, based on detailed personal information for each victim, likely represents the most comprehensive record of civilian deaths during the Korean War.[13] Moreover, the spatial distribution of killing intensity from this dataset is strongly correlated with that from alternative data sources, compiled through independent processes but with smaller coverage, reinforcing the validity of the data.[14]

Figure 2 illustrates trends in the number of civilian killings from March 1950 to June 1951.

[11]The dataset includes demographic information on victims, such as name, gender, age, and hometown, identification of social status from these records are not available. While the origin of the surname once indicated the relative status of a family in pre-modern Korean dynasties, such information is not available in this dataset and holds little significance in modern society. The dataset also includes the occupations of victims, but the records are highly inconsistent and lack information regarding the occupations of family members. To assess the nature of targeted killings, Appendix E.1 presents text analysis results using a supplementary source that covers a smaller number of victims but provides detailed information on the occupational status of both victims and their family members.

[12]We present summary statistics on gender and age groups in Figure H.1. In our sample, 74.7% of the individuals are male, with 87.5% of them aged between 15 and 64. Among females, 71.2% fall within the 15–64 age range, while approximately one-quarter are under the age of 15.

[13]Our dataset covers civilian killings committed by North Korean forces only. Ideally, one would also control for killings committed by South Korean forces, given the possibility of a correlation between killings intensity by different perpetrators. However, comprehensive records of such killings do not exist. As the perpetrating authorities, South Korea or Allied forces had little incentive to compile systematic records of killings they carried out within their own territory. Nevertheless, Table B.13 tests the robustness to local intensity of killings by South Korean forces, calculated from a supplementary data source (Truth and Reconciliation Commission of Republic of Korea, 2010).

[14]We compare our baseline township-level measure of civilian killings with an alternative measure constructed from the TRC reports (Truth and Reconciliation Commission of Republic of Korea, 2010). Although the coverage of victims from TRC reports is much smaller as discussed above, they will still be useful for verifying the validity of our data by examining the correlation between the two independently generated datasets. Figure H.2 shows a positive association between the township-level intensity of civilian killings calculated from the two datasets, with a correlation coefficient of 0.50.

Each dot denotes the total number of civilians killed by North Korean forces each month.[15] Consistent with the discussion in Section 2, the graph shows that civilian killings by North Korean forces were concentrated in a short period between September and October 1950, immediately following the Incheon Landing.

Beyond inter-temporal variation, Figure 3 plots the township-level distribution of the number of civilian killings perpetuated by North Korean forces per 1,000 population (at the 1949 census township-level). The observed spatial distribution provides supporting evidence for the hasty and unplanned retreat of North Korean forces as a key determinant of the spatial distribution of killings.

Specifically, we distinguish between three regions demarcated by the UN offensive line: the West, East, and North. The map reveals two important patterns that inform our identification strategy (Section 3.2). First, killings were largely concentrated in the West and North (shaded in green and blue) with only a small number of killings occurring in the East (shaded in red). Second, killing intensity appears to increase with distance to the offensive line only for townships located in the West but not for townships located in the East and North. In the East (red), killings are largely concentrated along the offensive line, suggesting that these killings might have partially arisen from collateral damage during UN and North Korean frontline clashes. In the North (blue), there is no clear relationship between killing intensity and distance to the offensive line, with some townships located along the offensive line experiencing above-average killing intensity. This echoes the military literature which suggests that North Korean killings in the West were largely driven by the hasty retreat of North Korean forces in response to the advance of UN forces (Kim, 2003). In contrast, killings in the East were minimal as these areas were largely under UN control after the Incheon Landing. Similarly, killings in the North had a weaker relationship with distance to the offensive line given that North Korean forces could easily retreat up North into North Korean territory.

It should be noted that we exclude metropolitan cities from our estimation sample as township-level population data for these regions are not available before the 1970s. Nevertheless, this restriction is unlikely to affect the analysis. As a mechanism linking civilian killings to long-run local development, our findings point to the role of structural transformation, a channel more relevant to non-metropolitan areas than to large cities. Rather, there are two advantages to our focus on rural areas. First, the prevalence of rural towns in our sample provides insights into the processes of economic growth and development in initially less-developed areas during the period of rapid economic growth in Korea. Second,

[15]The Korean War continued until the Armistice Agreement in July 1953. However, civilian killings by North Korean forces were rare in later period of the war, which is thus omitted from the graph.

the exclusion of townships within major cities help mitigate concerns about the confounding effects of targeted governmental industrial policies in the 1970s, which were pivotal to the so called "Han River Miracle" (Kim et al., 2021; Lane, 2025). In Section 5.3, we check the robustness of our results to excluding regions exposed to industrial clusters established under these policies.

## 3.2 Instrumental Variable

**Identification Strategy** To identify the causal impact of civilian killings, we use an IV strategy that exploits exogenous variation in killing intensity based on proximity to the UN offensive line. Following the historical accounts in Section 2, we construct our IV as the interaction between distance to the offensive line and an indicator for whether a township was located in the West. The interaction reflects the historical fact that the North Korean retreat trajectory passed predominantly through the West, leading to a higher intensity of killings in western townships located further from the offensive line.[16]

Specifically, distance to the UN offensive line proxies for the likelihood that a township was exposed to retreating North Korean forces during the brief interval between the formation of the offensive line and UN liberation. As discussed in Section 2, the primary documented activity of these retreating units was the systematic execution of perceived enemies under direct orders to "eliminate all elements that could support the UN forces upon landing" (Kim, 1984). The probability of exposure to this directive likely increased with distance from the offensive line for two reasons. First, North Korean units operating farther from the front were more likely to remain operational and able to execute the directive. Second, townships farther from the line were more likely to lie along retreat trajectories pushed outward by the expanding UN advance.[17]

Figure 4 plots the township-level correlation between distance to the offensive line and post-

[16]The offensive line—formed by the Incheon Landing and the Busan Perimeter—includes the Gyeongbu Expressway, a highway constructed in 1970 that connects Seoul and Busan. Consequently, our IV strategy based on distance from the offensive line may raise concerns that proximity to the expressway could act as a confounding factor. However, our IV exploits the specific effect of distance to the offensive line in the western area, where the North Korean army's retreat was blocked, rather than relying solely on distance itself. This mitigates concerns about a direct effect of proximity to the expressway. Moreover, Table B.11 shows that our results are robust to controlling for the distance of each township to the Gyeongbu Expressway.

[17]This logic prevents us from using a spatial regression discontinuity design. The offensive line was a transient front that shifted the probability of exposure to retreating units *continuously* as distance to the line increased, rather than as a discrete jump across the line. Consistent with this, Table 1 shows that the coefficient on the *Retreat* indicator is small and statistically insignificant. We therefore use the interaction between distance and west-of-the-line as a continuous instrument to exploit this continuous gradient in retreat-phase exposure.

Incheon landing killing intensity, separately for townships in the West, North, and East. Consistent with our proposition, the scale of killings increases with distance from the offensive line in the West, where North Korean forces were trapped (Panel A), but not in the North or East, where retreat routes to North Korea remained open (Panel B). Panel C further confirms that killings in townships in the East (occupied by UN forces) were much lower.[18]

In addition to relevance, our identification strategy relies on the standard exclusion restriction: conditional on the controls and fixed effects, the interaction between distance to the offensive line and the West indicator affects long-run outcomes only through retreat-phase exposure to retreating North Korean forces. This assumption would be violated if exposure to retreating forces were correlated with other consequences that had independent effects on local economic development. Specifically, the probability that a township lay on a retreat trajectory, the occupation duration of retreating units, and the extent to which North Korean units remained operational might potentially vary with distance to the offensive line.

Although we are not able to identify the role of different aspects of retreat-related exposure, the short window of variation we exploit potentially limits this concern. UN forces regained South Korean territory within two to three weeks of the Incheon Landing (Appleman, 1961), so the instrument is unlikely to reflect the consequences of variation in long-term occupation duration, such as institutional disruption, economic extraction, or sustained coercive recruitment. We emphasize that the differential exposure our instrument captures is the short post-Incheon retreat window (days to two–three weeks), not the roughly two-month pre-Incheon occupation, which was common to townships on both sides of the offensive line. Institutional and extractive channels requiring sustained territorial control would have operated during this common occupation period and are likely differenced out by our design. In contrast, the differential retreat window was dominated by targeted killings. Moreover, the persistence of any attendant effects need not require a long exposure window if, for example, they operate through the intergenerational transmission of preferences and attitudes. This would be consistent with the persistence documented in other brief but intense episodes of violence (Blouin and Mukand, 2019; Ochsner and Roesel, 2024). To that end, our estimates are best read as the effects of exposure to retreat-phase violence on upper-tail human capital, for which targeted killings are the most proximate and best-documented component.

Nevertheless, physical and economic destruction remains a plausible alternative explanation,

[18]Table C.1 further conducts a placebo test in which (i) townships east of the line are retained as the control group, and townships north of the line are defined as the treated group; and (ii) we estimated reduced-form regressions of key long-run development outcomes on the instrument. Panel A (Panel B) presents results from the baseline sample (placebo test) and shows that our instrument does not appear to predict negative effects on contemporary outcomes in the placebo townships.

as it could occur even within a short period. If distance to the offensive line operated through channels such as the destruction of infrastructure, confiscation of assets, or disruption of trade routes, one would expect to observe direct effects on industrial or occupational structure in the immediate post-war period. Yet indirect evidence suggests this is unlikely. First, as shown in Table F.1, we find no effect on employment or occupational structure in 1960. Second, historical accounts suggests that capital destruction was rare in the context of retreat-phase civilian killings, which predominantly took the form of executions targeting individuals rather than scorched-earth operations against productive assets.

Beyond anecdotal evidence, Appendix E.2 classifies the methods of killings documented in the Truth and Reconciliation Commission (TRC) reports (Truth and Reconciliation Commission of Republic of Korea, 2010), which are based on government investigation records compiled from applications filed by the families or relatives of victims presumed to have been killed by the North Korean military. The overwhelming majority of civilian killings were carried out using small or non-firearm implements (e.g., clubs, bayonets, and bladed tools), with few instances of damage to physical infrastructure reported. This pattern reinforces the interpretation that the variation captured by our IV operates primarily through the loss of human capital rather than through the destruction of physical or economic assets.

**First Stage Estimation** We examine this statistical relationship formally by estimating the following equation:

$$\begin{aligned}\log(1+\text{Civilian Killings Per Capita}_k) = \alpha + \beta Retreat_k \times Dist_k + \gamma Dist_k + \lambda Retreat_k \\ + \mu_G' X_k^G + \mu_S' X_k^S + \delta_p + \epsilon_k\end{aligned} \tag{1}$$

$\log(1+\text{Civilian Killings Per Capita}_k)$ represents our preferred measure of killing intensity—the log of one plus the number of civilian killings per 1,000 population in town $k$—measuring the number of civilian killings that occurred after the Incheon Landing. While the baseline specifications includes all zero-killing townships, the results are robust to alternative functional forms and to restricting our estimation sample to townships with positive killings only (Section 4.3).

$Retreat_k$ is an indicator that takes the value of 1 if town $k$ is located in the West or North, outside of the UN offensive line where North Korean forces could retreat (i.e., occupied by North Korean forces), and $Dist_k$ denotes distance to the nearest point on the offensive

line.[19] The interaction term, $Retreat_k \times Dist_k$, is our variable of interest and captures the difference in killing intensity between townships located in the West and North, further from the offensive line, vis-a-vis townships located in the East.

$X_k^G$ is a vector of geoclimatic controls, including caloric suitability, terrain elevation, slope, ruggedness, distance to the coast, distance to rivers, distance to Seoul, and latitude.[20] $X_k^S$ consists of initial socioeconomic controls: average literacy rate, population density, the share of employment in agriculture and manufacturing before the Korean War, and the intensity of civilian killings that occurred before the Incheon Landing. Importantly, the inclusion of pre-Incheon Landing killings further alleviates concerns about local fixed characteristics associated with killing intensity. $\delta_p$ indicates province fixed effects, which account for time-invariant provincial factors that may affect long-run development outcomes. Standard errors are adjusted following Conley (1999), with a distance cutoff of 30km.[21]

**First Stage Results** Table 1 summarizes first stage estimates. Odd-numbered columns include township-level geoclimatic controls, while even-numbered columns additionally control for socioeconomic characteristics. The estimation sample varies across columns: Columns (1) and (2) restrict the sample to townships located in all three regions; Columns (3) and (4) restrict the sample to townships located in the West and East; and Columns (5) and (6) restrict the sample to townships located in the North and East.

Consistent with our argument, the interaction term $Retreat \times Dist$ in Columns (1) and (2) are positively associated with the intensity of civilian killings, supporting the idea that areas farther from the line were more exposed to North Korean killings. Moreover, the coefficient of *Retreat* is close to zero, highlighting the importance of proximity to the border rather than the binary distinction of being inside or outside the offensive line. Results change little when we restrict our estimates to only the West and East (Columns (3) and (4)). However, the coefficient of the interaction term becomes close to zero when we restrict our sample to the North and East (Columns (5) and (6)). This aligns with the fact that proximity to the offensive line had little effect on civilian killings in the North, where retreat routes to North Korea were open. Hence, moving forward, we present all baseline regressions restricting our

[19] For townships within the UN offensive line, the minimum distance to the nearer of the two borders is used.

[20] The caloric suitability index accounts for agricultural endowment of each town. Topographic characteristics are included to address their potential impact on the accessibility of North Korean forces (Doyle and Bennett, 2013). We also control for distance to the coast, rivers, and Seoul given the possibility of their correlation with the proximity to the offensive line. By controlling for latitude, we mitigate concerns about endogeneity related to the south-to-north location of towns.

[21] This corresponds to approximately one-tenth of the width of the Korean Peninsula. A 30 km radius includes an average of about 167 towns. Table B.17 reports robustness to using alternative clustering methods.

estimation sample to townships located in the West and East only. Nevertheless, results are robust to alternative sample compositions including all areas (Appendix Table B.4) and including the West only (Appendix Table B.5). In the baseline sample, the township-level average number of killings per 1,000 population is 1.73, with an average township population of 24,846.[22]

Despite the strong first stage, concerns about the exclusion restriction may remain. If the distance to the offensive line were correlated with unobserved factors predicting subsequent economic development, particularly in the West, our IV strategy would be invalid. For example, if townships further from the offensive line in the West were initially more rural and had lower growth potential, this could bias our IV estimates.

Table A.1 examines this concern by regressing a battery of predetermined geographic features and pre-Korean War socioeconomic characteristics on our instrument. While distance to the line is negatively correlated with elevation, slope, and ruggedness west of the offensive line, this reflects the concentration of Korea's mountain ranges in the northeast and its plains in the southwest, and we control for these variables throughout. Turning to the socioeconomic characteristics, we find balance across the variables most relevant to subsequent development—population density (1949), literacy (1930), and sectoral employment shares (1930)—which we interpret as evidence of balance in available pre-treatment levels. We emphasize that these variables are cross-sectional snapshots and do not permit a test of comparable pre-war trajectories for our outcomes: nightlights, industrial productivity, and the other long-run measures have no pre-war counterparts, and the 1930 and 1949 data do not allow us to observe changes in the same outcomes over time. The balance evidence is therefore reassuring about the comparability of treated and control townships on observables, but it should not be read as a formal test of parallel pre-trends.

One might also be concerned that townships located further West of the offensive line were occupied by North Korean forces for a longer duration. As noted above, the pre-Incheon occupation was common to both sides of the line (the only exception being a small number of townships in the Southeast within the "Busan perimeter," which remained under Allied control), so the relevant question is whether distance to the line captures variation in post-Incheon occupation. This is unlikely for two reasons. First, distance from the advance line is unlikely to capture meaningful variation in post-Incheon occupation within areas west of the line: after the Incheon landing, Allied forces liberated the entire South Korea within two-three weeks, with Hanley (2020) noting that a single regiment drove "seventy-three miles in

[22] 53% of townships in our estimation sample report zero killings. As mentioned above, our baseline specification uses log(1 + killings per 1,000 population) as the endogenous variable and retains these zero-killing townships.

one ten-hour span," breaking out from the Busan perimeter and taking Chonju, the capital of North Cholla Province. Second, Table A.2 shows that our baseline results are robust to restricting the sample to a narrow bandwidth around the offensive line, which excludes townships within the Busan perimeter. Taken together, these suggest that our instrument is unlikely to capture differences in occupation duration.

Last, a remaining concern is that our identifying variation might be largely driven by Jeolla province which lies entirely west of the offensive line. Jeolla is widely perceived to have experienced political marginalization, stigma and/or policy neglect during South Korea's post–Korean-War industrialization period. In Table A.2, Column (1) restricts the sample to townships within 20km of the offensive line where only a single Jeolla township remains (0.3% of the sample). The coefficient remains negative and statistically significant (albeit with a much smaller F-stat). We interpret this as suggestive evidence that our findings are unlikely to be driven by townships in Jeolla. Section 4.3 conducts an additional robustness test where we restrict our sample to townships west of line and show that our results are robust to leveraging largely *within-province* variation in killings.[23]

**Baseline Specification** Our main estimating equation is as follows,

$$Y_k = \alpha + \beta \log(1 + \text{Civilian Killings Per Capita}_k) + \mu_G' X_k^G + \mu_S' X_k^S + \delta_p + \epsilon_k \qquad (2)$$

where we instrument $\log(1 + \text{Civilian Killings Per Capita}_k)$ with $Retreat_k \times Dist_k$ based on the above discussion. All other notations are identical to those in Equation (1), except that $Retreat_k$ and $Dist_k$ are now included in $X_k^G$. Standard errors are adjusted following Conley (1999), with a distance cutoff of 30km. All variables are harmonized with the 1975 town boundaries.[24] To allay any potential concerns about instrument strength, we report weak-instrument robust, Anderson-Rubin p-values in all subsequent tables. In Table B.15, we present reduced form estimates for key outcome variables.

[23] Another possible concern is that distance to the advance line might also be correlated with the destruction of physical capital. Two sets of evidence addresses this possibility. First, Table A.3 shows that our IV is uncorrelated with the incidence of major battles nor US bombing incidents. Second, Appendix E.2 shows that the majority of retreat-phase civilian killings were carried out using small or non-firearm implements, with little destruction of physical infrastructure reported.

[24] For data from 1975 onward, we harmonize variables using area-based weights calculated by intersecting the 1975 town shapefiles with those of the target period. Since township-level shapefiles are not available before 1975, we create crosswalks between townships in the pre-1975 periods and those in 1975 by tracking records of administrative unit changes.

**Immediate demographic impacts** What was the immediate impact of civilian killings on the demography of affected towns? We test this by regressing the demographic outcomes of 1955 on the intensity of civilian killings following Equation (2). As discussed above, our baseline specification controls for 1949 population density, which allows to examine the immediate impact of killings conditional on pre-Korean War demographics.

Table 2 displays the results. As shown in Column (1), civilian killings had little effect on post-war population, which aligns with our interpretation that the small scale of these incidents was not sufficient to generate an immediate reduction in the population or supply-side effects on the local economy. Columns (2) and (3) further show negligible effects on the demographic composition, as reflected in few differences in the shares of youths and working-age individuals.

The minimal effect of targeted killings on the size and composition of the population suggests that any effects on economic development are unlikely to be due to direct losses in population or age-specific human capital. However, we emphasize that this null pertains to immediate demographic change in the early post-war period (1955) and (i) does not preclude the more gradual, selective compositional change over subsequent decades; (ii) neither does it rule out selective elite flight (We discuss these possibilities in Section 5.3). In the following sections, we examine how civilian killings influenced long-term local development outcomes and explore potential channels through which these effects persisted.

# 4 Civilian Killings and Long-Run Development

This section examines the long-run effects of civilian killings on contemporary economic development. We find that townships with greater exposure to civilian killings during the war exhibit lower nightlight intensity and reduced industrial productivity today, a relationship that survives a range of robustness checks.

## 4.1 Nighttime Luminosity

Nighttime luminosity has been widely adopted as a proxy for (sub-)national economic development (Henderson et al., 2012; Michalopoulos and Papaioannou, 2018; Gibson et al., 2021). Following this approach, we measure township-level economic prosperity using the harmonized global nighttime light dataset (Li et al., 2020), which combines two publicly available datasets: the United States Air Force Defense Meteorological Satellite Program (DMSP) from 1992 to 2013 and the Visible Infrared Imaging Radiometer Suite (VIIRS) from 2012 to

2018. The harmonized global nighttime light data is generated as annual composites from monthly observations, which mitigates concerns on cloud covers and solar illumination (Li et al., 2020). Hence, our main dependent variable is the natural logarithm of average nighttime luminosity at the township-level. We construct this by calculating the total number of stable nightlights per square kilometre within each grid-cell and taking the average across all grid-cells that lie within each township's 1975 administrative boundaries.

Table 3 shows the IV estimates using nighttime luminosity as the outcome variable in five-year intervals from 1995 to 2015. The estimates are negative and statistically significant. Considering that our explanatory variable is $log(1 + \text{killings per capita})$ and over half of the townships in our estimation sample (53%) have no recorded killings, these coefficient estimates should not be interpreted as an elasticity (Chen and Roth, 2024). Instead, to interpret the magnitude of our estimated effects, Table B.2 replicates the estimation restricting the sample to townships with positive killings and defining the endogenous variable as the number of killings per 1,000 population. According to this specification, a 10% increase in killing intensity (on a sample mean of 5.08) corresponds to a 4.4% decrease in nightlight intensity (Column (1) in Panel D). We interpret the economic significance of this number by relying on estimates of the elasticity of nightlights to GDP per capita reported in Henderson et al. (2012). A 10% increase in killing intensity corresponds to a 1.5% decrease in GDP per capita.[25] We note that this figure should be read as the upper end of a plausible range. The more conservative within-West estimates (Table B.5) imply effects roughly 40% as large, so the corresponding GDP-per-capita effect would be closer to 0.6%. We regard the true effect as bracketed by these two designs.

While not strictly comparable, a 1.5% decrease in GDP per capita is about one-fifth the estimate in Riaño and Valencia Caicedo (2024), where a one standard deviation increase

[25] We base this magnitude on the levels specification estimated among townships with positive recorded killings (Panel D of Table B.2), rather than on our baseline. The reason is that our baseline endogenous variable is log(1+ killings per capita) and 53% of townships record no killings, so its coefficient cannot be read as an approximate percentage effect (Chen and Roth, 2024). Restricting to townships with recorded killings yields an intensive-margin estimand—the effect of greater killing intensity where killings occurred—which is the quantity a percentage magnitude interpretation requires. Two specifications are available on that sample: Panel B, which uses log(killings per capita) and implies a 19.4% decline in nightlight intensity for a 10% increase in killing intensity, and Panel D, which uses killings per capita in levels and implies 5.5%. We report the latter because its first stage is considerably stronger (Kleibergen–Paap $F$ of 13.1 against 4.3) (It is also the more conservative of the two). Concretely, we re-estimate the Panel D specification using the raw level of nightlights per township as the outcome. The coefficient on killings per 1,000 population is $-2.46$, on a sample mean of 23.3 among townships with positive killings. Applying the estimated elasticity of GDP to lights of 0.277 of Henderson et al. (2012), the implied decrease in GDP per capita per additional killing per 1,000 is $(-2.46/23.3) \times 0.277 = -0.029$, or approximately 2.9%. For a 10% increase in killing intensity at the sample mean of all townships with positive killings (+0.508 killings per 1,000), this corresponds to $(-2.46 \times 0.508/23.3) \times 0.277 = -0.015$, or approximately 1.5%.

in unexploded ordinances (UXO) in Laos is associated with a 7.1% decrease in GDP per capita.[26] Appendix Table H.1 further situates our estimates within the broader literature on the long-run economic effects of violence and forced migration stemming from the loss of human capital. Taken together, our effect sizes are largely comparable to those of existing studies.

Overall, given that all regressions control for pre-war local economic characteristics, such as literacy rates, population density, and employment shares in agriculture and manufacturing, our IV estimates likely reflect the long-term negative effects of civilian killings that emerged following the end of the Korean War.

## 4.2 Sectoral Composition and Productivity

Our findings based on nighttime luminosity are echoed by patterns in local industrial characteristics. These measures are obtained from the 2020 Census of Establishments, which provides various information on establishments across sectors, including their township-level locations. It should be noted that township-level socioeconomic data for periods after the 1960s is not publicly available. Consequently, we use restricted-access data that require approval from Statistics Korea. Due to data confidentiality policies, the number of townships slightly differs by dependent variable.

Panel A of Table 4 shows the long-term impacts of civilian killings on sectoral composition and productivity. As proxies for industrial productivity, we calculate the manufacturing value added per worker and the average salary of manufacturing workers, representing marginal and average labor productivity in the industrial sector, respectively. While Column (1) suggests minimal impact on the manufacturing employment share, the effects on productivity measures are negative and statistically significant. The absence of effects on the manufacturing employment share, combined with significant declines in value added and wages per worker, suggests that civilian killings have had persistent negative effects on industrial productivity within the manufacturing sector.

Consistent with this, Panel B we documents heterogeneous effects of civilian killings on manufacturing employment by industry-level human capital intensity. Following Ciccone and Papaioannou (2009), we construct an index of industry-level human capital intensity using the share of college-educated workers in each industry in 2005.[27] Based on this index, industries

[26] The larger effects in Riaño and Valencia Caicedo (2024) are possibly due to large-scale contamination of agricultural land by UXOs that further deterred long-term investments and rural-urban migration. We test for selective migration in our context in Section 5.3.

[27] We use data from the 2005 census 20% sample—the latest census for which we have industry-level

are grouped into quartiles based on the employment share distribution across the full sample, and the effects of local killing intensity on employment share in each quartile are estimated separately.[28] Since industry-specific employment share is not available at the township-level, the analysis relies on county-level data, with the instrumental variable constructed following the same procedure as in Section 3.2.

The results show a clear pattern: as shown in Columns (3) and (4), the effects on employment shares in the top two quartiles are negative and statistically significant, whereas the effects for the bottom two quartiles are positive, though statistically insignificant. This indicates that civilian killings disproportionately reduced employment in human capital-intensive industries, consistent with the interpretation that wartime killings constrained local industrial development primarily by depressing the supply of skilled labor.

## 4.3 Robustness Checks

We conduct a battery of robustness checks. First, Table B.1 and B.2 shows that our estimates are qualitatively robust to alternative functional forms and restricting our sample to townships with positive killings. Table B.3 reports the Poisson (PPML) first stage (Chen and Roth, 2024), together with its sensitivity to extreme values.[29] Second, we test if our findings are sensitive to alternative sample compositions. Table B.4 shows that our results are robust to extending our baseline sample to all townships in South Korea, with those in the East and North serving as controls. In Table B.5, we restrict the sample to townships west of the offensive line and instrument killing intensity with distance to the line alone (i.e. $Dist$ in place of $Retreat \times Dist$ in Equation 2). The estimates remain negative and statistically significant across all outcomes, but their magnitudes are notably smaller than in the full-sample baseline, roughly 39% for log nightlights (-0.37 versus -0.95), 41% for log value added per worker (-0.77 versus -1.90), and 25% for log average salary (-0.10 versus -0.40). We interpret this at-

employment information at a disaggregated enough level.

[28] Among the 23 industries in manufacturing, the classification into quartile groups is as follows: (i) $\leq 25th$: Food and Beverages; Textiles; Wearing Apparel and Fur Articles; Leather, Bags, and Footwear; Wood and Wood Products; Fabricated Metal Products (ii) 25-50$th$: Pulp, Paper, and Paper Products; Rubber and Plastic Products; Non-Metallic Mineral Products; Motor Vehicles and Trailers; Furniture and Other Manufactured Products; Recycled Raw Materials (iii) 50-75$th$: Basic Metals; Other Electrical Machinery and Apparatus; Electronic Components, Audio, Video, and Communication Equipment (iv) $\geq 75th$: Tobacco Products; Publishing, Printing, and Reproduction of Recorded Media; Coke, Refined Petroleum Products, and Nuclear Fuel; Chemicals and Chemical Products; Other Machinery and Equipment; Computers and Office Machinery; Medical, Precision, and Optical Instruments, Watches, and Clocks; Other Transport Equipment

[29] The PPML coefficient is insignificant in the full sample but positive and significant once the upper tail of the killings distribution is trimmed, while the OLS $\log(1+Y)$ first stage is significant throughout.

tenuation as informative rather than as evidence against our result. Because the west-of-line first stage remains strong (Kleibergen–Paap F-stat $\approx$ 48), the smaller coefficients are not driven by weak instruments or reduced power; they reflect a genuinely more demanding identifying contrast. The full-sample estimate draws on both the within-West intensity gradient and the coarser comparison between western and eastern townships, while the latter captures some residual cross-regional variation in development potential that survives our province fixed effects and pre-war controls. The west-of-line specification removes this cross-regional component and identifies the effect purely from within-region, and largely within-province, variation in exposure. We therefore view it as our most conservative estimate and read it as an approximate lower bound. Even under this stringent comparison, higher killing intensity is associated with economically meaningful and statistically significant declines in long-run development.[30] Table B.6 further shows that the results are robust to excluding townships with the top-decile of killings, indicating that our findings are not driven by a small number of townships with an exceptionally high number of killings.

Next, we examine the possibility that the destruction of physical capital or infrastructure might confound our results. Building on Table A.3, which shows that our instrument is not associated with the prevalence of major battles or bombing incidents, Table B.8 reports that our findings are robust to controlling for an indicator for whether a major battle occurred and excluding entirely all townships that experienced a US bombing.

We also test robustness to additional controls. To address concerns about proximity to key infrastructure as potential confounders, we show that our findings are robust to excluding townships close to Seoul (Table B.9) and Busan (Table B.10); and controlling for proximity to the Gyeongbu Expressway (Tables B.11) and the nearest port (Table B.12).[31] In Table B.13, we control for the intensity of civilian killings by South Korean authorities.[32] Although the

[30] A commonly-held perception is that *Jeolla* province was discriminated against in terms of unfavorable industrial policy during the post-Korean War period (Kim, 2012). This concern is potentially reinforced by Lane (2025), who documents that the Heavy-Chemical Industry Drive of the 1970s had transformative effects on manufacturing in targeted regions, which were disproportionately located in the East of our sample. We directly address this in two ways. First, Section 5.4 shows that our results are robust to controlling for the location of industrial complexes established under this policy (Table F.4). Second, Appendix Table B.7 reports estimates that exclude the *Jeolla* provinces and, separately, that restrict the sample to *Jeolla* alone.

[31] Completed in 1970, the Gyeongbu Expressway was the first major highway linking South Korea's main cities and is considered to have contributed to the overall effectiveness of government-led industrial policies during that period.

[32] We digitize township-level data on killings by South Korean forces from case reports investigated by Truth and Reconciliation Commission of Republic of Korea (2010). However, reports are limited to cases formally submitted for investigation, some of which remain ongoing. Thus, this data does not necessarily provide a complete record of South Korean killings. Nonetheless, the correlation between the number of victims attributed to North Korean forces in these reports and our baseline data is relatively strong (0.5, Figure H.2), suggesting that this data on South Korean killings is likely a reasonable approximation.

killings by South Korea were not targeted in nature and occurred at different times (Section 2), any potential correlation between the killings by South Korean authorities and those by North Korean forces may raise concerns about interpretation. However, the results show minimal change.

Last, as documented in Table A.1, townships west of and within the offensive line show different topographic characteristics, which raise concerns about their confounding effects. To address this, Table B.14 replicates the estimation by excluding townships with the highest elevation values. In this trimmed sample, geographic characteristics are largely balanced, and the estimated effects of killing intensity remain negative and statistically significant. The coefficients of the geographic variables are also statistically insignificant, further alleviating concerns about confounding effects. Lastly, we show that our findings remain similar across reduced-form and OLS specifications (Table B.15 and B.16).

# 5 Potential Mechanisms

Conflicts may influence long-term economic development through a number of channels including trust (Cassar et al., 2013), institutions (Dell and Querubin, 2018), and the loss of a parent on children's future outcomes (Dupraz and Ferrara, 2023).

In our context, where killings targeted educated individuals and those connected to them, human capital emerges as a likely channel. We classify potential mechanisms into two broad categories. Demand-side channels capture the possibility that killings reduced incentives for human capital accumulation through discouragement, reduced returns, income effects, or the loss of educated networks. Supply-side constraints capture the possibility that killings directly disrupted educational infrastructure. Given that the killings disproportionately targeted elites and their families, together with the small scale of incidents, we hypothesize that demand-side channels are more likely to drive our findings.

We first provide evidence for our demand-side hypothesis before ruling out supply-side constraints and showing downstream effects on structural transformation. Lastly, we consider alternative explanations including differential exposure to govenrment policies, selective migration, and trust.

## 5.1 Discouragement of Human Capital Accumulation

### 5.1.1 Targeted Killings and Demand for Human Capital

The targeted nature of killings toward elites, intellectuals, and their families suggests that changes in the demand for higher education may be the key explanation for observed negative long-run outcomes. As illustrated in Section 2, the majority of victims had relatively high levels of education or close ties to educated individuals. We hypothesize that targeted killings reduced demand for human capital accumulation through several interconnected pathways.

Primarily, targeted killings may have created a discouragement effect toward education. Exposure to violent killings associated with higher educational attainment could have reduced incentives to invest in human capital by reducing the perceived returns to education. If higher educational attainment or stronger ties to educated networks were perceived as increasing the probability of violent persecution, the expected risks of education would rise. Together with a possible reduction in positive externalities from the loss of educated individuals and their networks (Borjas, 1995), targeted killings might decrease the likelihood of individuals pursuing higher education.

This hypothesis is consistent with human capital models that emphasize the role of both expectations and preferences as key determinants of educational investment (Patnaik et al., 2022). Psychological mechanisms may further reinforce this effect: fear and trauma from past persecution can lead to an aversion to activities perceived as risky (Young, 2019; Rockmore and Barrett, 2022). These dynamics may be amplified by the enduring political and military tensions on the Korean peninsula, as well as through intergenerational transmission of cultural attitudes within families and communities, allowing the legacy of violence to persist across generations (Nunn and Wantchekon, 2011; Dohmen et al., 2012).

### 5.1.2 Cross-Sectional Analysis

If reduced demand for human capital (rather than a supply-side mechanism) operated as the primary channel, we should see a short-run impact of civilian killings on *investments* in human capital, with little to no effects on the existing stock of human capital. Using educational outcomes from the 1960 and 1966 censuses (the earliest post-Korean War census waves), Table 5 shows results supporting this prediction. Column (1) considers the school enrollment rate of children aged 13 or younger from the 1960 census. Due to data availability, the ratio is calculated using the total number of children aged 13 and younger, rather than specifically for school-aged children. This introduces measurement error, as the denominator

includes children below school age, likely leading to attenuation bias. Despite this limitation, the negative coefficient (weak-IV-robust p-value $\approx 0.05$) is consistent with lower investment in children's education in the post-war period.

Columns (2) and (3) use the share of adults aged 25 and over who completed secondary and college education as outcome variables. The results indicate that adult educational attainment was not significantly associated with the local intensity of civilian killings. This suggests that the effects of civilian killings on human capital are unlikely to reflect the direct destruction of existing human capital stock and its continued effect; rather, they appear to be a consequence of reduced investment in education in subsequent periods.

Since the 1960 census only reports school enrollment rate of children under 13, Columns (4) and (5) augment this with data on school enrollment rates by different levels of education from the 1966 census.[33] The point estimate for primary schooling is close to zero (Column (4)), plausibly reflecting near-universal enrollment following the 1950 compulsory primary education laws,[34] while the secondary-enrollment estimate is negative and marginally significant (Column (5)). We are cautious in interpreting the primary-versus-secondary contrast, since both estimates are subject to attenuation from the misaligned age bands (5–14 and 15–19 rather than actual schooling ages). Thus, we read them as broadly consistent with a response concentrated at the secondary-school margin rather than as a precise localization of the effect.

These results are suggestive of a negative demand-side effect on human capital accumulation among those directly or indirectly exposed to civilian killings. Korean students typically attend secondary school between the ages of 13 to 15. Secondary school enrollment rates in 1966 thus reflects enrollment decisions of parents whose children were born at the height of the Korean War from 1951 to 1953. In the following section, we further investigate the timing of the effects of the killings intensity on educational attainment.

### 5.1.3 Cohort-Level Analysis

To examine the timing of the effects, this section employs a cohort analysis that leverages variation in exposure to civilian killings across birthyear cohorts within townships, as well as variation in the intensity of civilian killings across townships within birthyear cohorts. This

[33]Enrollment rates from the 1966 census are also subject to measurement errors. The census reports population data only for age groups (5-14 and 15-19) that do not correspond to the typical primary (5-12) and secondary (13-18) schooling ages. As we use these mismatched age groups in the denominator, measurement errors may introduce attenuation bias.

[34]In 1959, the primary school enrollment rate for school-aged children exceeded 96.4% (Ministry of Education, 1998).

approach allows us to examine whether the negative effects on education persisted over time and across different generations.[35]

We use information on individual-level educational attainment from the 2010 population census and employ a difference-in-differences approach:

$$\text{Education}_{kc} = \alpha + \beta_c \text{ Cohort}_c \times \log(1 + \text{Civilian Killings Per Capita}_k) + \gamma_c + \tau_k + \epsilon_{kc} \quad (3)$$

For birthyear cohort $c$ and township $k$, $\text{Education}_{kc}$ denotes the two outcome variables: the share of high school graduates or higher and the share of the population with an associate degree or higher. $\text{Cohort}_c$ is an indicator for five-year birthyear cohorts, ranging from 1930-1934 to 1975-1979. $\gamma_k$ and $\tau_c$ denote township and birthyear-cohort fixed effects, respectively. Standard errors are clustered at the township level. As in our baseline IV specification, we include all townships with zero reported killings.

This analysis should be interpreted with caution in two respects. First, since the data do not provide information on birthplace, we proxy township of birth using current town of residence. Thus, our findings may reflect migration patterns that may or may not be associated with human capital or local development. Second, we are unable to test for pre-trends, as the oldest cohorts still alive in 2010 would have been at least partially exposed to the Korean War. Accordingly, we do not interpret our parameter of interest, $\beta_c$, as the causal effect of civilian killings on educational attainment. Instead, we view it as a suggestive correlation indicating whether individuals in townships with more intense civilian killings were more or less likely to have higher education.

Figure 5 displays estimates using the cohort-level share of high school graduates (left panel) and college graduates, defined as individuals with an associate degree or higher (right panel), as outcome variables. Starting from the left panel, townships that experienced a higher killing intensity have a lower share of high school graduates in later cohorts relative to cohorts born in 1930-1934. Given that the later cohorts were either exposed to the Korean War at an earlier age or born after the legacy of civilian killings had emerged, this declining trend suggests that the negative relationship between civilian killings and educational attainment is not merely a reflection of existing local characteristics. Negative effects peak for cohorts

[35]The cross-sectional IV specifications are not applicable to the analysis of historical educational outcomes, mainly due to the lack of data. While the 1960 and 1966 censuses include information on township-level enrollment, the dependent variables cannot be well-defined. For instance, the 1960 census includes the number of children enrolled in school aged 13 or younger, which does not correspond to a standard school-age population. Similarly, the 1966 census reports population data only for specific age groups (5-14 and 15-19) that do not correspond to typical primary (5-12) and secondary (13-18) schooling ages, which hinders the exact calculation of enrollment rates. Furthermore, there is no township-level data on educational outcomes for the 1970-1990s.

born in 1960-1964, but this trend reverses afterward. This reversion can be attributed to the expansion of the education system in South Korea. The high school enrollment rate in South Korea began to rise rapidly in the 1970s and exceeded 95% by the 1990s (Korea Educational Development Institute, 2024), which suggest significant regional convergence in high school education across regions, townships with low enrollment were catching up with their counterparts.

Moving to the right panel, we find similarly negative albeit smaller effects on the share of college graduates. Coefficient estimates are negative and statistically significant throughout (except for the coefficient on the 1950-1954 cohort), attesting to the possibly long-lasting effects of targeted killings on human capital accumulation. Importantly, in contrast to results on the share of high school graduates, a negative effect on the share of college graduates persists for cohorts born well after the Korean War (1975-1979), suggesting a possibly persistent effect of targeted killings on right-tail human capital accumulation. This stands in contrast to Riaño and Valencia Caicedo (2024) suggesting that average levels of human capital accumulation eventually recover in the decades following the first bombings in Laos.

The timing and magnitude of these decreases in human capital accumulation are consistent with the possibility that targeted killings led to a persistent discouragement effects on the demand for higher education. We next present suggestive, survey-based evidence related to long-run effects on individual attitudes toward education, which is largely consistent with our preferred interpretation.

### 5.1.4 Survey-Based Evidence

Our preferred interpretation is that killings reduced educational attainment by lowering the perceived value of education, through potential channels such as reduced expected returns, the loss of role models, and psychological discouragement. Ideally, we would evaluate these explanations directly, but no such data exist.

Alternatively, this section examines the long-term effects of killing intensity on attitudes toward education using the Korean General Social Survey, a nationally representative dataset.[36] Because the survey was conducted from 2003 to 2019, our analysis is limited in its assessment of contemporaneous effects. Nevertheless, a long-term relationship between killing intensity and attitudes toward education would provide informative evidence for understanding the mechanisms behind our findings.

[36]More information on the KGSS data can be found at https://kgss.skku.edu/kgss/index.do.

$$Y_{ik} = \alpha + \beta \log(1 + \text{Civilian Killings Per Capita}_k) + \mu'_G X_k^G + \mu'_S X_k^S + \mu'_I X_{ik} + \delta_p + \delta_w + \epsilon_{ik} \quad (4)$$

The estimation follows Equation (4). $Y_{ik}$ is an outcome of interest for respondent $i$ residing in township $k$. We control for predetermined township-level characteristics ($X_k^G$ and $X_k^S$) as in Equation (2). We also control for individual-level characteristics ($X_{ik}$) including age, age squared, and indicators for gender and urban residence. $\delta_p$ and $\delta_w$ each denote province and survey-wave fixed effects, respectively. Standard errors are adjusted following Conley (1999) using a distance cutoff of 30km. Following our baseline IV specification, we include all townships with zero reported killings.

We use two sets of questions associated with the subjective valuation of education. Panel A of Table 6 reports results using individual perceptions of desirable wage determinants as dependent variables. For interpretation, survey responses are adjusted to a 0–1 scale (1 = strongly agree, 0 = strongly disagree). As shown in Column (1), individuals in areas with higher intensity of killings tend to place a lower weight on education in the labor market, consistent with a possible decline in the perceived value of education. A potential concern is that this pattern may reflect a consequence rather than a mechanism: the local history of killings may have affected educational outcomes through alternative channels, with further implications for attitudes toward education. To address this possibility, Column (2) controls for fixed effects for educational attainment. The results remain unchanged, suggesting that our findings are not second-order effects of killings on educational attainment given that they hold even among individuals with the same level of education.

Columns (3)–(6) replicate the estimation using perceptions of alternative wage determinants. We find no significant effects for years of employment, a proxy for seniority that is an important factor in determining remuneration in the Korean context (OECD, 2024), or for job performance. Taken together, these results are consistent with the interpretation that targeted killings reduced the perceived value of education specifically, rather than altering attitudes toward wage determinants more broadly.

Panel B of Table 6 tests whether killings reduced individuals' likelihood of relying on highly educated people for important discussions. We use survey questions asking respondents to identify individuals with whom they discuss important matters and to report that person's characteristics, including educational background. The dependent variable is an indicator for whether the first person a respondent recalled was a college graduate. According to sociological studies, people tend to mobilize social ties based on the resources they deem

most valuable (Lin, 2002; Bourdieu, 2018). If individuals are less likely to seek out highly educated individuals when seeking advice on important matters, this may be reflective of a lower subjective valuation of education.

Column (1) shows that in townships with a higher intensity of killings, those with whom people discuss important matters are less likely to hold a college degree. Moreover, Column (2) shows that this relationship remains robust to the inclusion of fixed effects for respondents' own educational attainment, suggesting that the observed pattern is not likely to be driven by differences in the average educational levels of one's social circle (as proxied by an individual's own educational attainment).

#### 5.1.5 Quantifying the Contribution of the Education Channel

Next, we provide a back-of-the-envelope assessment of how much of the long-run effect of killings on GDP per capita can be attributed to the education channel. This calculation is built entirely on the 2010 human-capital estimates (Column 4, Panel D of Table B.2); it does not draw on the 1960 and 1966 enrollment coefficients discussed in Section 5.1.2. The logic proceeds in three steps.

First, we run a cross-sectional IV specification where we (i) restrict our estimation sample to townships with positive killings; (ii) define the endogenous variable as the number of killings per 1,000 population. Column 4, Panel D of Table B.2 shows that a 10% increase in killing intensity is associated with an approximately 0.9 percentage point decrease in the share of individuals with some college education or above ($-0.017 \times 0.508$). Second, using estimates from the literature on Mincerian returns, we adopt a return of approximately 15.5% per additional year of schooling for South Korea (Psacharopoulos and Patrinos, 2004). The transition from high school to college represents approximately 4 additional years of schooling, implying an earnings premium of approximately 62%. Third, under the assumption that local GDP per capita is proportional to average labor productivity, the implied effect on GDP per capita of reducing the college share by 2 percentage points is approximately:

$$\Delta \ln(\text{GDP per capita}) \approx \Delta(\text{college share}) \times \text{college premium} = 0.009 \times 0.62 \approx 0.56\%$$

Our main IV estimate implies that a 10% increase in killing intensity reduces long-run GDP per capita by approximately 1.5%. The back-of-the-envelope calculation suggests that the education channel alone can account for approximately 37% of this total effect. This suggests that the education channel is quantitatively important but likely not the sole mechanism, consistent with our evidence that delayed structural transformation and other factors may

have also played complementary roles. We note that this calculation likely represents a lower bound on the contribution of education, as it abstracts from general equilibrium forces, such as productivity spillovers from educated workers to the broader local economy and complementarities between human capital and physical capital accumulation, that would amplify the aggregate impact of reduced educational attainment.

#### 5.1.6 Alternative Channels

**Other Demand-Side Drivers** Targeted killings might have led to immediate post-war declines in economic development, which in turn could have reduced the demand for skills. However, Table F.1 shows that the intensity of civilian killings is not significantly linked to sectoral composition (Columns 1–2) or the distribution of high- and low-skill occupations (Columns 3–5) in 1960. This suggests that short-run effects on local development are unlikely to explain the persistent legacy of civilian killings on human capital investments and development outcomes.

**Supply-Side Constraints** An alternative explanation is that areas with higher targeted killings experienced greater losses in the supply of education, such as a reduction in teachers or destruction of school infrastructure. Table 7 examines this possibility using county-level data on the number of teachers in the immediate post-War period (1955), given the absence of corresponding township-level data. Column (1) shows that counties with more civilian killings had lower school enrollment rates in the immediate post-war period, consistent with the township-level estimates in Table 5. Column (2) reports that the decline in enrollment was accompanied by a decline in the number of teachers. However, the reduction in teachers is unlikely to be the operative channel for the decline in educational investment. A back-of-the-envelope calculation suggests the teacher-supply effect accounts for at most 0.4% of the enrollment effect (Appendix G), which is too modest to explain the substantial drop in enrollment. As shown in Column (3), the long-term effect of killing intensity on teacher supply is negligible.

In addition, there appears to have been few recorded instances of widespread destruction of schooling infrastructure or long-term school closures (Appleman, 1961). Quantitatively, we present two pieces of supporting evidence. First, Appendix E.2 suggests that killings by retreating North Korean forces were not accompanied by widespread physical destruction. Second, as discussed in Section 4.3, Table B.8 shows that our results are robust to controlling for major battle occurrences and excluding bombed townships. These patterns suggest that supply-side constraints were not likely the primary driver of the decline in educational

investment.

## 5.2 Declines in Structural Transformation

Having established the effects of civilian killings on human capital accumulation, we now investigate how these effects might have affected long-term development outcomes through structural transformation.

Human capital accumulation has been shown to play a critical role in structural transformation (Caselli and Coleman II, 2001; Porzio et al., 2022). In South Korea, this process was particularly pronounced. The country transformed from one of the world's poorest agrarian economies into a leading industrial economy within a few decades. This rapid shift was driven not only by government policies but also by significant investments in human capital, with families prioritizing education to ensure their children could acquire the skills needed for higher-paying jobs in emerging industries (Kim and Topel, 1995).

In this context, the negative effects of civilian killings on human capital investment could have slowed the reallocation of labor from agriculture to higher-productivity sectors. Importantly, if reduced human capital investment were the primary channel, we would expect to observe a time lag between killings and their effect on structural transformation, as affected cohorts reached working age and entered the labor force. We examine this using the 1970 and 1980 Agricultural Censuses, the only township-level data containing information on agricultural prevalence, covering over 75% of towns in our estimation sample.[37]

Panel A in Table 8 examines the impact of civilian killings on the size of the agricultural sector, as measured by the share of full-time agricultural farm households in 1970 and 1980 (i.e. households solely dependent on farm income). The 1970 estimate is negative and only marginally significant (Column (1)), while the 1980 estimate is strongly positive (Column (2)). Although we do not have a clean interpretation of the negative 1970 level effect, the relevant test for our mechanism is whether the two periods differ. In Column (3), we use the change in the agricultural-household share between 1970 and 1980 as the dependent variable. The coefficient is positive and significant (0.384; weak-IV-robust $p < 0.001$), indicating that the effect of killings on agricultural prevalence changed significantly over the 1970s.[38]

[37] The dataset, officially titled Censuses of Agriculture, Forestry, and Fisheries of the Ministry of Agriculture and Forestry Statistics of Korea, is not publicly available. We are grateful to Hong et al. (2023) for generously sharing their data with us.

[38] We obtain the same conclusion from a stacked specification pooling both census years and interacting killing intensity with a $\text{Post}_{1980}$ indicator instrumented by $\text{Retreat}_k \times \text{Dist}_k \times \text{Post}_{1980}$, with all controls fully interacted with $\text{Post}_{1980}$. The interaction is 0.404 ($p < 0.01$), close to the first-difference estimate. We do not report this specification in full for brevity.

This significant change over the decade has two implications. First, it is consistent with lower early educational investment (Table 5) acting as a proximate factor behind the later slowdown in structural transformation. In high-killing townships, cohorts of secondary-school age in the 1960s depressed structural transformation only once they reached working and household-formation age. Second, the fact that the effect strengthens over the decade, rather than appearing immediately after the war, partially alleviates concerns about the role of other immediate post-Korean War shocks: it is not obvious why alternative war-related factors would produce a sectoral-reallocation effect that grows between 1970 and 1980. While our findings do not decisively establish specific channels, their timing is consistent with interpretations based on the downstream effects of human capital discouragement.

One possible concern is that a slowdown in structural transformation might not necessarily lead to lower development if accompanied by improvements in agricultural productivity. In this view, Panel B in Table 8 investigates two outcomes potentially associated with 1980 farm productivity: average farm size and the number of cultivators per acre. The coefficients for all these measures are close to zero, providing no evidence that the persistence of agricultural households in higher-killing areas was accompanied by offsetting gains in farm productivity.

## 5.3 Alternative Explanations

While the evidence supports our primary mechanism of reduced human capital accumulation, alternative explanations could potentially confound our findings. We examine three leading possibilities: differential exposure to national development policies, selective migration, and declines in trust. As we show, none of these alternatives appear to fully account for our findings.

**National Policies** The negative effect on structural transformation emerges during the 1970s, a period that coincides with two major national policy initiatives: the agricultural New Village Movement (NVM) and the Heavy-Chemical Industry Drive Policy (HCIDP). The NVM's main focus was on increasing agricultural productivity through infrastructure development and modern farming techniques (Moore, 1984). The HCIDP provided support to targeted industries in various forms, including tax reductions, subsidized loans, and the construction of industrial complexes in nine locations across the country (Kim et al., 2021), which had substantial effects on manufacturing development in targeted regions (Lane, 2025). If the killings reduced exposure to these policies, for example through the loss of local elites with better access to centrally directed programs, this would offer an alternative explanation

for our findings. To assess this possibility, we examine whether the allocation and local execution of these policies vary with killing intensity.

We begin with the top-down allocation of NVM resources. If policy implementation was associated with elite killings, we would expect to see a negative association between killing intensity and the allocation of these policies across townships. Using data from Hong et al. (2023), Table F.2 examines this possibility by investigating the township-level relationship between civilian killings and the amount of per capita NVM transfers received. The results indicate little variation in relation to killings.

Local execution of these programs may nonetheless have differed. Table F.3 further examines the effects of civilian killings on two county-level measures of local responses to NVM projects.[39] Column (1) uses the share of total project costs funded by local residents as a proxy for the extractive capacity of the local government. Column (2) reports the ratio of implemented to planned projects, a proxy for implementation efficiency. We find no significant effects on either outcome, suggesting that townships with higher killing intensity neither received systematically fewer resources nor executed them less effectively.

We cannot apply the same approach to the HCIDP due to the absence of data on HCIDP budget expenditures. Instead, Table F.4 tests if the key effects of civilian killings on long-term development outcomes are robust to controlling for the presence of HCIDP industrial complexes at the county-level (as identified in Kim et al. (2021)). The estimates show minimal differences from the baseline specifications, suggesting that the long-run effects of civilian killings are not driven by heterogeneity in the implementation of government-led industrial policies.

Taken together, the results suggest that the 1970s national policy interventions are not a confounding channel. We note that this exercise addresses the narrower question of whether differential exposure to these programs accounts for our findings, while leaving open a broader role for local state capacity. As discussed in Appendix E.1, a significant share of the victims likely held high-status occupations, suggesting that the killings may have weakened local state capacity alongside the loss of educated elites. We view this as a potential component of the broader upper-tail human capital channel rather than a competing explanation: the removal of locally prominent individuals may have reduced both the human capital available within communities and the capacity of the institutions that relied on it.

**Displacement and selective migration** We interpret our estimate as the downstream

[39] As data are not available at the township level, we collect and digitize the statistical yearbooks for each county in the National Archives of Korea. Because statistical yearbook coverage varies by county, the dependent variable is calculated by aggregating all available values for the 1970s.

effects of retreat-phase violence on upper-tail human capital, of which killing and (elite) flight-under-threat are two related facets. Let us discuss effects on immediate displacement.

We find little evidence that the killings triggered immediate displacement on a scale capable of driving our results solely through compositional change. Table 2 shows that killing intensity did not have any significant effect on post-war population-levels (Column 1); the share of youths (Column 2); or prime-age population ratio (Column 3). Moreover, two features of the counter-offensive suggest that selective elite flight is unlikely to be a main driver: (i) Any flight prompted by a two-to-three-week retreat episode was likely temporary: these areas were quickly and permanently secured as South Korean territory, leaving comparatively weaker reasons for permanent relocation; (ii) Given North Korean units lacked clear retreat routes and largely dispersed into the interior (Section 2), (elite) civilians might not have known where or how to flee safely.

Another possibility is that affected areas gradually lost their most productive residents through selective out-migration. Although the above discussion suggests that demographic change is unlikely to account for contemporaneous effects on human capital, selective migration could still reinforce the long-term impact of civilian killings. Unfortunately, data limitations preclude observation of individual migration status, making it difficult to cleanly decompose incumbent from compositional effects.

To that end, as a proxy for selective out-migration, we examine the prime-age population share (individuals aged 25–54) in each census year. Rural-to-urban migration in Korea has shown strong age-related patterns, with working-age individuals being more likely to move to urban areas (Kim, 1982; Lee and Kim, 2020). Table F.5 reports IV estimates for each year: the effect of killings on the prime-age share is close to zero through 1985, turns negative but statistically insignificant in 1995 ($-0.040$), and is negative and significant only in 2005 ($-0.113$, $p < 0.05$). We are cautious in interpreting these coefficients: we do not treat the insignificant earlier estimates together with the significant 2005 estimate as establishing a dated onset of migration. The data do not permit us to formally pin down when compositional change began.

The timing of evidence, read alongside the rest of our results, is nonetheless informative. Negative effects on educational attainment are already visible for cohorts born in the 1950s (Figure 5) and on structural transformation by 1980 (Table 8), whereas the prime-age population share shows no detectable response until 2005. This pattern is therefore more consistent with human-capital responses preceding selective migration than with migration driving the initial divergence. However, because our data do not allow us to observe individual migration status, we cannot cleanly separate effects on the incumbent population from later composi-

tional change. The results therefore should not be interpreted as ruling out the possibility or effects of selective migration. Rather, we read the timing as broadly indicating that any role of selective migration in our findings likely reinforced the persistence rather than initiated the initial divergence in local development. As a further test of this interpretation, Table F.6 examines heterogeneous effects by access to the pre-Korean War colonial road network. Under explanations based on selective out-migration, if road access primarily lowered the cost of leaving affected townships, we would expect negative effects to be larger where such access was available. The results suggest no differential effects.

**Trust** A substantial literature documents the negative effects of conflict and killings on trust (Nunn and Wantchekon, 2011; De Juan and Pierskalla, 2016; Barceló, 2021). Given the potential link between social capital and economic development (Knack and Keefer, 1997), negative effects on trust might also explain observed differences in long-run development. Given this, we examine the long-term relationship between civilian killings and trust using the Korean General Social Survey (described in Section 5.1). The earliest KGSS wave was conducted in 2003, precluding analysis of short-term effects on trust as a potential mechanism. Nonetheless, we view this exercise as informative about the potential role of trust in driving our observed results.

Table F.7 shows results using different questions related to trust. We find a small, statistically significant negative effect on trust in the armed forces (Column 1), but the magnitude is too small to plausibly explain observed differences in long-run development. Moreover, the relationship with generalized trust (Columns 2 and 3) is statistically insignificant.

These null effects contrast sharply with previous studies that have documented a persistently negative effect of mass violence on social trust (e.g., Blouin and Mukand, 2019). One possibility is that targeted killings in our context were perpetrated by North Korean forces, who were entirely separated from the affected communities thereafter. In contrast, negative effects on trust might be more salient when killings are perpetrated by individuals who continue to interact with their victims such as in the Rwandan case.

# 6 Conclusion

This study provides novel evidence on the effects of civilian wartime killings on local development. We construct a comprehensive dataset on civilian killings during the Korean War and provide quantitative evidence that these events contributed to persistent regional disparities in development across South Korea. These effects have persisted for over 70 years, despite

the relatively small scale of civilian deaths and Korea's rapid post-war economic growth.

Given the historical context of civilian killings targeting educated elites, we hypothesize that the persistent effects emerged from reduced human capital accumulation. Our evidence supports this proposition: greater exposure to civilian killings appears to have led to lower investments in education which, in turn, led to an overall decline in structural transformation. These findings suggest a new perspective on the enduring impact of conflict. Beyond large-scale capital destruction or mass casualties, even localized, small-scale violence can leave a lasting mark on economic development by hindering human capital accumulation. In addition, our findings shed light on how historical conflicts can create persistent within-country disparities in economic development even within a rapidly industrializing economy. During South Korea's growth miracle, the accumulation of upper-tail human capital was an important input into local development, alongside its role in the country's postwar transformation (Kim and Topel, 1995). This study provides quasi-experimental evidence supporting this view, showing that an initial negative shock to the upper tail of human capital generated persistent divergence in local development. More broadly, they suggest that policies aimed at reshaping preferences for human-capital investment may be crucial for escaping low-development equilibria in conflict-ridden settings.

A key question is why exposure to short-lived violence generated such persistent effects. We note that such persistence is not uncommon across other historical contexts, including the Rwandan genocide (roughly 100 days) and the days-long Turkish siege of Vienna, with latent memories of the latter manifesting as Islamophobia nearly 500 years later (Blouin and Mukand, 2019; Ochsner and Roesel, 2024). In our setting, targeted killings, while limited in scale, may have had an outsized effect on the intergenerational transmission of attitudes toward education, particularly given the historically tight-knit nature of Korean villages and communities. This possibility is consistent with a broader body of evidence on the role of culture in shaping long-run development (Giuliano and Nunn, 2021).

The Korean War has created a complex legacy beyond the economic effects documented in this paper. The stigmatization of victims' families, the contested politics of memory and reconciliation, and the interaction between wartime violence and subsequent industrial policy all represent important dimensions of the war's long-run impact that warrant systematic analysis. Similarly, examining the political economy consequences of targeted killings, including effects on voting behavior and democratic participation, remains a promising direction for future research.

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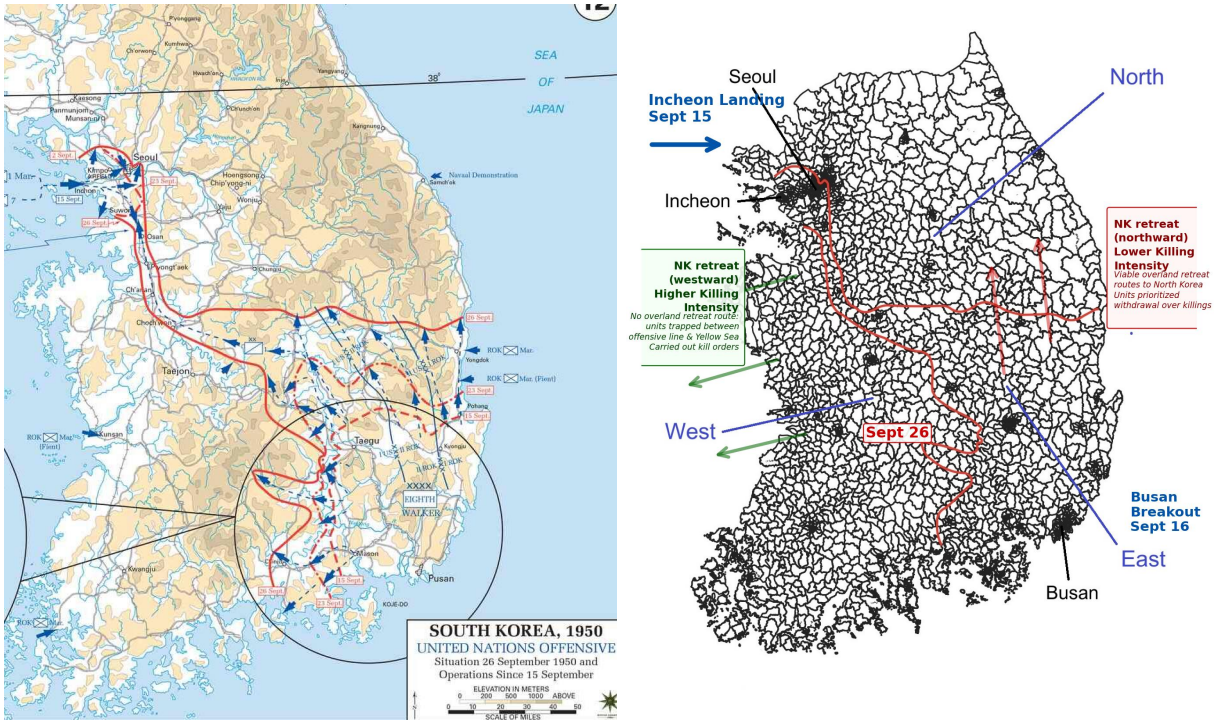
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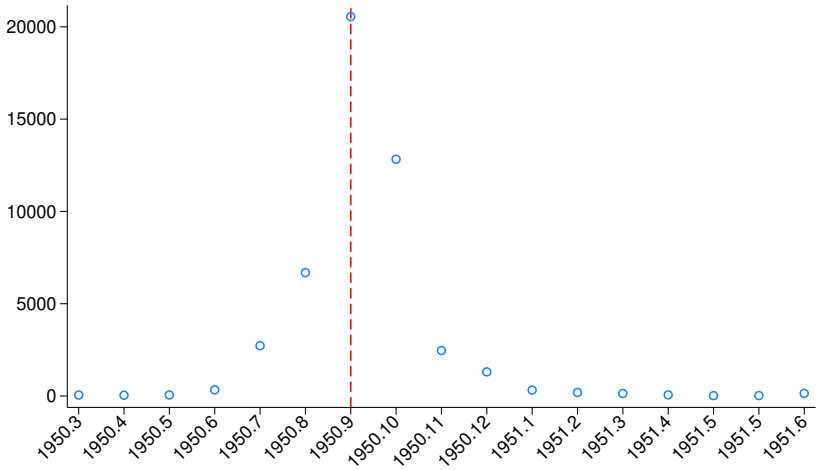
**Figure 1:** Map of United Nations offensive line following Incheon Landing




(a) Original Map (b) Digitized Map

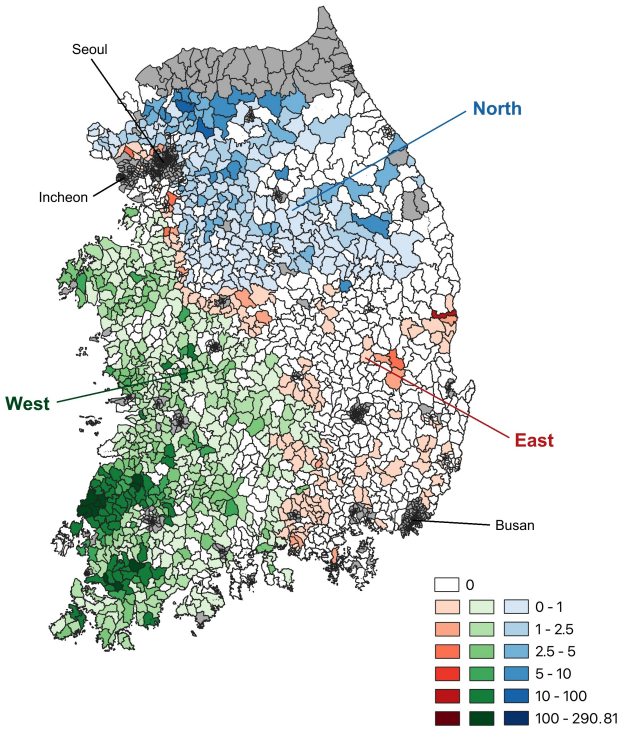
Notes: Panel (a) shows the original military map of the United Nations offensive following the Incheon Landing and operations between September 15 and September 26, 1950 (Source: United States Military Academy, Department of History). Panel (b) shows the digitized offensive line overlaid on township boundaries. On September 15, UN forces launched an amphibious landing at Incheon and broke out from the Busan Perimeter (September 16), forming a temporary offensive line connecting the northwest (Seoul) and southeast (Busan) by September 26. The offensive line divided South Korea into three regions: West, East, and North. North Korean units west of the line were trapped between the line and the Yellow Sea with no viable overland retreat route to North Korea. Green arrows indicate the westward retreat direction of these trapped North Korean units. They were thus more likely to carry out targeted killings under orders to "eliminate all elements that could support the UN forces". Killing intensity increased with westward distance from the line as these areas took longer for advancing UN forces to reach. In contrast, North Korean units east of the line received the same orders but had viable overland retreat routes. Red arrows indicate viable northward retreat routes for units east of the line. These units thus largely prioritized withdrawal, generating no such distance-from-line gradient in killings.

**Figure 2:** Trends in civilian killings during the Korean War



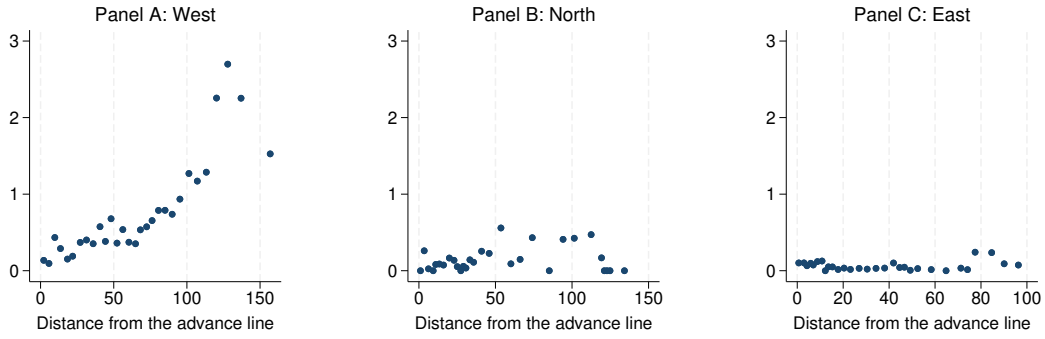

Notes: This figure shows the number of civilian killings by North Korean forces at the monthly level. The red line indicates when the Incheon Landing was conducted (September 1950). Source: "List of Noncombatants Killed by North Korean Forces" (Statistics Division of Republic of Korea, 1952)

**Figure 3:** Spatial distribution of civilian killings



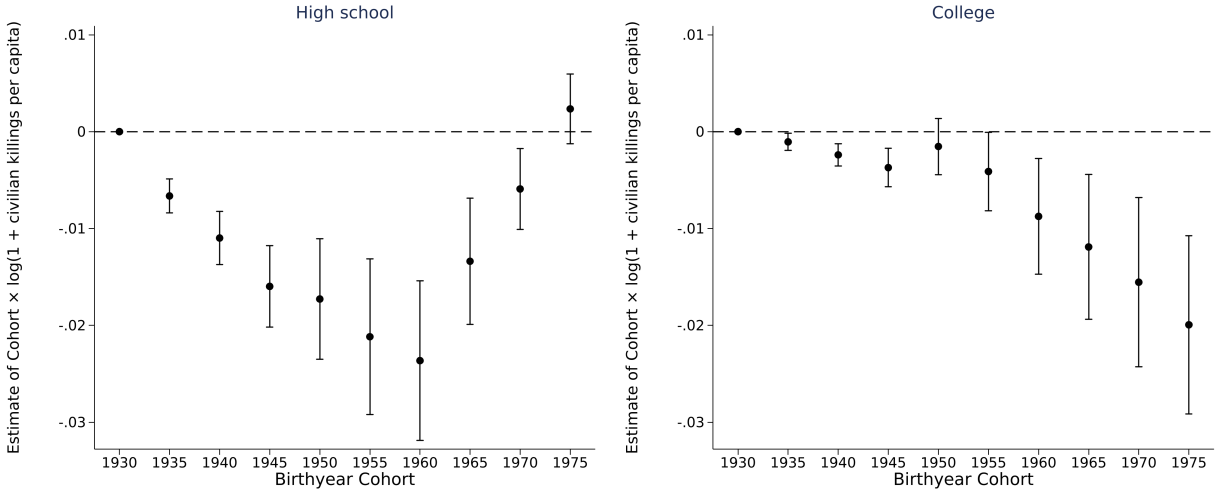

Notes: This figure shows the number of civilian killings per 1,000 population (Statistics Division of Republic of Korea, 1952). The mean number of killings in our entire dataset is approximately 1.7 per 1,000 population (17.5 killings per town). Areas North and West of the offensive line are designated in blue and green, respectively. The area East of and within the two offensive lines that connect Incheon and Busan are highlighted in Red.

**Figure 4:** Distance from Offensive Line and Intensity of Civilian Killings




Notes: These figures plot log(1+ civilian killings per 1,000 population) against the distance between township centroids to the offensive line in kilometers. Source: "List of Noncombatants Killed by North Korean Forces" (Statistics Division of Republic of Korea, 1952)

**Figure 5:** Civilian Killings and Education: Cohort-Township Level Estimates




Notes: This figure presents coefficient estimates of the interaction between birthyear cohort indicators and log(1 + civilian killings per 1,000 population) on high school (left) and college graduation (right), respectively. Birthyear cohorts are in five-year intervals (1930-1934, 1935-1939, ...). The specification includes township and cohort fixed effects. The reference group is the 1930-1934 cohort. Standard errors are clustered at the township level. Vertical bars indicate 95% confidence intervals. Source: Population Census 2010.

**Table 1:** First Stage Regression

| Dependent Variable: | Log(1 + Civilian Killings per 1,000 Population) | | | | | |
|---|---|---|---|---|---|---|
| | All | | West+East | | East+North | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Retreat × Distance | 0.009*** | 0.006*** | 0.011*** | 0.016*** | -0.008** | -0.003 |
| | (0.003) | (0.002) | (0.002) | (0.002) | (0.003) | (0.003) |
| Retreat | -0.026 | -0.033 | -0.182** | -0.127 | 0.116 | 0.024 |
| | (0.117) | (0.088) | (0.077) | (0.090) | (0.082) | (0.061) |
| Distance | 0.005** | 0.005*** | 0.005* | -0.003* | 0.004** | 0.001 |
| | (0.002) | (0.001) | (0.003) | (0.002) | (0.002) | (0.002) |
| Observations | 1784 | 1615 | 1343 | 1182 | 1147 | 1066 |
| R-Squared | 0.14 | 0.41 | 0.19 | 0.51 | 0.05 | 0.24 |
| Geoclimatic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Socioeconomic Controls | | Yes | | Yes | | Yes |
| Province Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: The dependent variable in Columns (1) - (6) is log(1+civilian killings per 1,000 population). Columns (1) - (2) include all towns. Columns (3) and (4) include towns located within (East) and below (West) the offensive line, and Columns (5) and (6) include towns located within (East) and above (North) the offensive line. Geoclimatic Controls include caloric suitability, terrain elevation, slope, ruggedness, distance to the coast, distance to rivers, distance to Seoul, and latitude. Socioeconomic Controls include share of employment in agriculture, average literacy rate, and population density. All columns account for the number of civilian killings that occurred before the formation of the offensive line, a dummy for being located within the offensive line (*Retreat*), and distance to the offensive line. Conley standard errors with a distance cutoff of 30km are shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Table 2:** Civilian Killings and Demographic Change (1955)

| Dependent Variables: | Log Total Population | Ratio of 14 and under | Prime–Age Ratio |
|---|---|---|---|
| | (1) | (2) | (3) |
| log(1 + killings per capita) | -0.002 | 0.002 | 0.004 |
| | (0.129) | (0.012) | (0.006) |
| Observations | 1064 | 1064 | 1064 |
| Mean | - | 0.47 | 0.30 |
| Kleibergen–Paap F–Stat | 50.56 | 50.56 | 50.56 |
| Weak-instrument-robust p-value | 0.991 | 0.896 | 0.508 |
| Geoclimatic Controls | Yes | Yes | Yes |
| Socioeconomic Controls | Yes | Yes | Yes |
| Province Fixed Effects | Yes | Yes | Yes |

Notes: The dependent variable in Column (1) is the log population by town calculated from the 1955 census. The dependent variable in Column (2) is the ratio between the number of individuals aged 14 and below, to the total population in each town. The dependent variable in Column (3) is the prime-age population ratio by town in the same year, defined as the share of individuals aged 25-54. Conley standard errors with a distance cutoff of 30km are shown in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

**Table 3:** Civilian Killings and Night Light Intensity

| Dependent Variable: | Log Average Night Light Intensity | | | | |
|---|---|---|---|---|---|
| | 1995 | 2000 | 2005 | 2010 | 2015 |
| | (1) | (2) | (3) | (4) | (5) |
| log(1 + killings per capita) | -0.690** | -0.797** | -0.966*** | -0.905** | -0.937** |
| | (0.339) | (0.340) | (0.373) | (0.387) | (0.406) |
| Observations | 1182 | 1182 | 1182 | 1182 | 1182 |
| Kleibergen–Paap F–Stat | 51.83 | 51.83 | 51.83 | 51.83 | 51.83 |
| Weak-instrument-robust p-value | 0.046 | 0.019 | 0.007 | 0.013 | 0.018 |
| Geoclimatic Controls | Yes | Yes | Yes | Yes | Yes |
| Socioeconomic Controls | Yes | Yes | Yes | Yes | Yes |
| Province Fixed Effects | Yes | Yes | Yes | Yes | Yes |

Notes: The dependent variable in Columns (1) – (5) is the log average night light intensity by township by year (Li et al., 2020). Conley standard errors with a distance cutoff of 30km are shown in parentheses. *$p <$ 0.1, **$p < 0.05$, ***$p < 0.01$

**Table 4:** Civilian Killings and Industrial Development

| *Panel A: Manufacturing Productivity (Township-Level)* | | | | |
|---|---|---|---|---|
| Dependent Variables: | Manufacturing Employment Share | Log Value Added per Worker | Log Average Salary per Worker | |
| | (1) | (2) | (3) | |
| log(1 + killings per capita) | -0.028 | -1.897*** | -0.399*** | |
| | (0.120) | (0.638) | (0.138) | |
| Observations | 882 | 1176 | 1173 | |
| Mean | 0.21 | - | - | |
| Kleibergen–Paap F–Stat | 21.81 | 51.52 | 50.69 | |
| Weak-instrument-robust p-value | 0.814 | 0.001 | 0.001 | |
| *Panel B: Industrial Development (County-Level, 2005)* | | | | |
| Dependent Variables: | Industry employment share by college-graduate quartile | | | |
| | ≤25th | 25-50th | 50-75th | ≥75th |
| | (1) | (2) | (3) | (4) |
| log(1 + killings per capita) | 0.185 | 0.107 | -0.263*** | -0.216*** |
| | (0.147) | (0.130) | (0.059) | (0.058) |
| Observations | 116 | 116 | 116 | 116 |
| Kleibergen–Paap F–Stat | 26.14 | 26.14 | 26.14 | 26.14 |
| Weak-instrument-robust p-value | 0.103 | 0.357 | 0.000 | 0.000 |
| Geoclimatic Controls | Yes | Yes | Yes | Yes |
| Socioeconomic Controls | Yes | Yes | Yes | Yes |
| Province Fixed Effects | Yes | Yes | Yes | Yes |

Notes: Panel A reports township-level IV estimates with Conley SEs (30km cutoff). The dependent variable in Column (1) is the manufacturing employment share (2020 census). Columns (2) and (3) are log value added and log average salary per worker. Panel B reports county-level IV estimates (N=116) with Conley SEs (100km cutoff). Dependent variables are industry employment shares by college-graduate quartile from the 2005 census. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

**Table 5:** Civilian Killings and Educational Outcomes (1960–1966)

| Dependent Variable: | Enrollment Rate Under 13 | Proportion of Graduates | | Enrollment Rate | |
|---|---|---|---|---|---|
| | | Secondary (1960) | College | Primary (1966) | Secondary |
| | (1) | (2) | (3) | (4) | (5) |
| log(1 + killings per capita) | -0.036* | -0.018 | 0.006 | 0.006 | -0.107* |
| | (0.021) | (0.029) | (0.009) | (0.007) | (0.059) |
| Observations | 1031 | 1143 | 1143 | 1143 | 1143 |
| Mean | 0.30 | 0.13 | 0.02 | 0.62 | 0.36 |
| Kleibergen–Paap F–Stat | 44.89 | 53.41 | 53.41 | 53.41 | 53.41 |
| Weak-instrument-robust p-value | 0.052 | 0.524 | 0.470 | 0.373 | 0.053 |
| Geoclimatic Controls | Yes | Yes | Yes | Yes | Yes |
| Socioeconomic Controls | Yes | Yes | Yes | Yes | Yes |
| Province Fixed Effects | Yes | Yes | Yes | Yes | Yes |

Notes: The dependent variable in Column (1) is the percentage enrolled of all children aged under 13 by township in 1960. The dependent variable in Columns (2)-(3) is the proportion of secondary school and college graduates, respectively, for all persons aged 25 and over in 1960. The dependent variables in Columns (4)–(5) are primary and secondary school enrollment rates for their respective age cohorts in 1966. Conley standard errors with a distance cutoff of 30km are shown in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

**Table 6:** Civilian Killings and Educational Attitudes

*Panel A: Attitudes Toward Wage Determinants*

| Dependent Variable: | Do you agree that wage should be determined by the following factors? | | | | | |
|---|---|---|---|---|---|---|
| | Education | | Years of employment | | Job performance | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| log(1 + killings per capita) | -0.097*** | -0.102*** | 0.011 | 0.014 | -0.062 | -0.054 |
| | (0.020) | (0.003) | (0.054) | (0.061) | (0.046) | (0.041) |
| Observations | 1428 | 1427 | 1427 | 1426 | 1428 | 1427 |
| Mean | 0.61 | 0.61 | 0.77 | 0.77 | 0.86 | 0.86 |
| Standard Deviation | 0.26 | 0.26 | 0.21 | 0.21 | 0.16 | 0.16 |
| Kleibergen–Paap F–Stat | 11.09 | 10.53 | 11.85 | 11.22 | 11.44 | 10.81 |
| Weak-instrument-robust p-value | 0.058 | 0.036 | 0.838 | 0.828 | 0.219 | 0.203 |
| Educational attainment | No | Yes | No | Yes | No | Yes |
| Geoclimatic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Socioeconomic Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Province Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |

*Panel B: Education of Discussion Partners*

| Dependent Variable: | Discussion partner is a college graduate | |
|---|---|---|
| | (1) | (2) |
| log(1 + killings per capita) | -0.230*** | -0.128*** |
| | ( 0.072) | ( 0.043) |
| Observations | 669 | 669 |
| Mean | 0.41 | 0.41 |
| Standard Deviation | 0.49 | 0.49 |
| Kleibergen–Paap F–Stat | 79.67 | 96.48 |
| Weak-instrument-robust p-value | 0.000 | 0.000 |
| Educational attainment | No | Yes |
| Geoclimatic Controls | Yes | Yes |
| Socioeconomic Controls | Yes | Yes |
| Province Fixed Effects | Yes | Yes |

Notes: The dependent variables are based on responses to KGSS surveys conducted between 2003 and 2019. Panel A: responses are rescaled to 0–1 (1 = strongly agree, 0 = strongly disagree). Panel B: we construct an indicator based on survey responses to the question: "During the past year, who did you talk to when you had some important matters ... try to answer the following questions for each of them... What is the highest level of school this person has attended?" We use an indicator that equals 1 if the first person that a respondent recalled graduated from college. Observations are weighted by individual survey weights. Even-numbered columns control for indicators of educational attainment categories. Conley standard errors with a distance cutoff of 30km are shown in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

**Table 7:** Civilian Killings and Teacher Supply: County-Level Estimates

| Dependent Variables: | School Enrollment | Log number of teachers per population aged 6–25 | |
|---|---|---|---|
| | 1955 | 1955 | 2010 |
| | (1) | (2) | (3) |
| log(1 + killings per capita) | -0.166*** | -0.005** | 0.006 |
| | (0.036) | (0.002) | (0.006) |
| Observations | 133 | 133 | 133 |
| Mean | 0.49 | - | - |
| Kleibergen–Paap F–Stat | 40.27 | 40.27 | 40.27 |
| Weak-instrument-robust p-value | 0.002 | 0.030 | 0.392 |
| Geoclimatic Controls | Yes | Yes | Yes |
| Socioeconomic Controls | Yes | Yes | Yes |
| Province Fixed Effects | Yes | Yes | Yes |

Notes: Conley standard errors with a distance cutoff of 100km are shown in parentheses. The dependent variables in Columns 1 and 2 are calculated using the 1955 census data; Column 3 uses the 2010 census data. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

**Table 8:** Civilian Killings, Agricultural Households, and Agricultural Productivity

| *Panel A: Ratio of Agricultural Households (1970–1980)* | | | |
|---|---|---|---|
| Dependent Variable: | Ratio of Agricultural Households | | Δ Ag HH Share |
| | 1970 | 1980 | (1980−1970) |
| | (1) | (2) | (3) |
| log(1 + killings per capita) | -0.154 | 0.231*** | 0.384*** |
| | (0.107) | (0.083) | (0.122) |
| Observations | 924 | 922 | 921 |
| Mean | 0.57 | 0.62 | 0.05 |
| Kleibergen–Paap F–Stat | 34.65 | 33.85 | 33.79 |
| Weak-instrument-robust p-value | 0.092 | 0.008 | 0.000 |
| Geoclimatic Controls | Yes | Yes | Yes |
| Socioeconomic Controls | Yes | Yes | Yes |
| Province Fixed Effects | Yes | Yes | Yes |
| *Panel B: Farm Characteristics (1980)* | | | |
| Dependent Variables: | Log Average Farm Size | Log Number of Cultivators Per Acre | |
| | (1) | (2) | |
| log(1 + killings per capita) | 0.043 | 0.041 | |
| | (0.122) | (0.045) | |
| Observations | 922 | 922 | |
| Kleibergen–Paap F–Stat | 33.85 | 33.85 | |
| Weak-instrument-robust p-value | 0.731 | 0.369 | |
| Geoclimatic Controls | Yes | Yes | |
| Socioeconomic Controls | Yes | Yes | |
| Province Fixed Effects | Yes | Yes | |

Notes: Panel A: The dependent variable is the ratio of full-time agricultural households to all households. Column (1) uses the 1970 agricultural census; Column (2) uses 1980. Column (3) uses the change in agricultural household share between 1980 and 1970. Panel B: The dependent variable in Column (1) is the log average farm size (hectares). Column (2) is the log number of cultivators per acre. All Panel B variables are from the 1980 agricultural census. Conley standard errors with a distance cutoff of 30km are shown in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

# Appendices

**October 19, 2026**

## A Balance Check

**Table A.1:** Balance Check Results

| Dependent Variables: | Caloric Suitability | Elevation | Slope | Ruggedness | Latitude | Distance to Coast |
|---|---|---|---|---|---|---|
| Retreat × Dist | 0.30 | -0.97*** | -27.87** | -905.94** | -0.00* | 182.00 |
| | (0.27) | (0.36) | (11.71) | (399.45) | (0.00) | (185.16) |
| Retreat | 3.01 | 64.84*** | 1969.75*** | 63044.59*** | 0.00 | 2678.45 |
| | (9.34) | (21.18) | (596.47) | (19590.15) | (0.09) | (7621.00) |
| Dist | -0.75*** | -0.39* | -17.35** | -592.84** | 0.00 | -545.11*** |
| | (0.26) | (0.23) | (7.12) | (252.25) | (0.00) | (149.47) |
| Observations | 1347 | 1347 | 1347 | 1347 | 1347 | 1347 |
| R–Squared | 0.28 | 0.21 | 0.17 | 0.17 | 0.07 | 0.36 |
| Dependent Variables: | Distance to River | Distance to Seoul | 1949 Population Density | 1930 Literacy | 1930 Share of Agriculture | 1930 Share of Manufacturing |
| Retreat × Dist | 20.28 | -0.22 | -0.00 | 0.00 | 0.00 | 0.00 |
| | (43.53) | (0.21) | (0.00) | (0.00) | (0.00) | (0.00) |
| Retreat | -1718.65 | 5.07 | -0.00** | -0.03*** | 0.02 | 0.02 |
| | (1861.59) | (10.42) | (0.00) | (0.01) | (0.03) | (0.02) |
| Dist | 7.01 | 0.56*** | 0.00 | 0.00 | -0.00** | -0.00 |
| | (36.44) | (0.17) | (0.00) | (0.00) | (0.00) | (0.00) |
| Observations | 1347 | 1347 | 1347 | 1315 | 1207 | 1207 |
| R–Squared | 0.01 | 0.19 | 0.01 | 0.05 | 0.03 | 0.02 |

Notes: Conley standard errors with a distance cutoff of 30km are shown in parentheses. The unit of observation is the township. 1949 population density; 1930 literacy and sectoral shares are obtained from the 1949 and 1930 South Korean population censuses. Geo-climatic controls are constructed by overlaying township boundaries with gridded raster data and aggregating cell-level values to the township level using area-weighted means. Sources: FAO/IIASA Global Agro-Ecological Zones for caloric suitability; global digital elevation data for elevation, slope and ruggedness. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

**Table A.2:** Townships within Narrow Bandwidth of the Advance Line

| Dependent Variable: | Log Mean Night Light Intensity (2010–2018) | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Bandwidth | 20km | 30km | 50km |
| log(1 + killings per capita) | -2.871** | -1.758* | -2.339* |
| | (1.134) | (0.931) | (1.244) |
| Observations | 309 | 446 | 687 |
| Kleibergen–Paap F–Stat | 6.55 | 22.27 | 10.49 |
| Anderson–Rubin p–value | 0.000 | 0.029 | 0.016 |
| tF 95% CI Lower | -8.177 | -4.108 | -6.501 |
| tF 95% CI Upper | 2.434 | 0.593 | 1.822 |
| tF 90% CI Lower | -5.802 | -3.396 | -4.979 |
| tF 90% CI Upper | 0.059 | -0.120 | 0.300 |
| Geoclimatic Controls | Yes | Yes | Yes |
| Socioeconomic Controls | Yes | Yes | Yes |
| Province Fixed Effects | Yes | Yes | Yes |

Notes: The dependent variable is the log of average night light intensity (2010-2018). Columns (1)–(3) show the results using townships located within 20km, 30km, and 50km from the advance line, respectively. Conley standard errors with a distance cutoff of 30km are shown in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

**Table A.3:** Placebo First Stage: IV and Incidence of Major Battles and US Bombings

| Dependent Variable: | Battle in Township | Bombing Incident |
|---|---|---|
| | (1) | (2) |
| Retreat × Distance | 0.001 | 0.001 |
| | (0.001) | (0.001) |
| Observations | 1182 | 1182 |
| Dep. Var. Mean | 0.039 | 0.047 |
| Dep. Var. SD | 0.194 | 0.211 |
| Geoclimatic Controls | Yes | Yes |
| Socioeconomic Controls | Yes | Yes |

Notes: Column (1) uses an indicator equal to one if a major battle occurred within the township boundary (228 battles from Ministry of National Defense records). Column (2) uses an indicator equal to one if the township experienced at least one US bombing or strafing incident documented by the Truth and Reconciliation Commission (106 incidents across 89 townships). All specifications include geoclimatic and socioeconomic controls. Conley (1999) spatial HAC standard errors with 30km cutoff in parentheses. $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

# B Robustness Checks

**Table B.1:** First Stage: Alternative Functional Forms for Civilian Killings

| Dependent Variable: | $\log(1+Y)$ Full Sample | $\mathbf{1}(Y>0)$ Full Sample | $\log(Y)$ Non-Zero Only | $Y$ (levels) Full Sample | $Y$ (levels) Non-Zero Only |
|---|---|---|---|---|---|
| | Baseline | Panel A | Panel B | Panel C | Panel D |
| | (1) | (2) | (3) | (4) | (5) |
| Retreat × Distance | 0.016*** | 0.010*** | 0.014** | 0.137*** | 0.349*** |
| | (0.002) | (0.003) | (0.007) | (0.051) | (0.093) |
| Observations | 1182 | 1182 | 552 | 1182 | 552 |
| R-Squared | 0.51 | 0.13 | 0.40 | 0.38 | 0.42 |
| Dep. Var. Mean | 0.412 | 0.470 | -0.166 | 2.392 | 5.075 |
| Dep. Var. SD | 0.799 | 0.499 | 1.624 | 13.117 | 18.754 |
| Sample | Full | Full | Non-zero | Full | Non-zero |

Notes: Each column reports the first-stage regression of a different transformation of civilian killings per 1,000 population on Retreat × Distance, with geoclimatic controls, socioeconomic controls, and province fixed effects. Column (1) is the baseline specification using $\log(1+Y)$ on the full sample. Column (2) uses a binary indicator for any killing. Column (3) restricts to townships with positive killings and uses $\log(Y)$ without the +1 transformation. Column (4) uses killings in levels on the full sample. Column (5) uses killings in levels restricted to non-zero townships. Conley standard errors with a distance cutoff of 30km are shown in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

**Table B.2:** Alternative Functional Forms for Civilian Killings

| *Panel A: Any Killing Indicator (=1) — Full Estimation Sample* | | | | | |
|---|---|---|---|---|---|
| Dependent Variables: | Log Night Light Intensity (1) | Manuf. Empl. Share (2) | Log Value Added per Worker (3) | Log Avg. Salary per Worker (4) | Share Some College (5) |
| Any Killing (=1) | -1.581** | -0.032 | -3.216** | -0.678*** | -0.317* |
| | (0.751) | (0.134) | (1.273) | (0.224) | (0.164) |
| Observations | 1182 | 882 | 1176 | 1173 | 1182 |
| KP F-Stat | 13.13 | 7.49 | 13.84 | 13.23 | 13.13 |
| *Panel B:* log*(Killings per 1,000 population) — Restricted Sample: Townships with Positive Killings* | | | | | |
| Log(Killings per 1,000) | -2.161* | -0.115 | -3.360* | -0.585 | -0.423** |
| | (1.119) | (0.148) | (1.794) | (0.473) | (0.199) |
| Observations | 552 | 365 | 550 | 550 | 552 |
| KP F-Stat | 4.27 | 1.86 | 4.43 | 4.43 | 4.27 |
| *Panel C: Killings per 1,000 (Levels) — Full Estimation Sample* | | | | | |
| Killings per 1,000 pop. | -0.111* | -0.009 | -0.216** | -0.045** | -0.022* |
| | (0.058) | (0.038) | (0.096) | (0.022) | (0.012) |
| Observations | 1182 | 882 | 1176 | 1173 | 1182 |
| KP F-Stat | 6.86 | 1.59 | 7.24 | 7.52 | 6.86 |
| *Panel D: Killings per 1,000 (Levels) — Restricted Sample: Townships with Positive Killings* | | | | | |
| Killings per 1,000 pop. | -0.088** | -0.019 | -0.139** | -0.024 | -0.017*** |
| | (0.036) | (0.021) | (0.069) | (0.020) | (0.007) |
| Observations | 552 | 365 | 550 | 550 | 552 |
| KP F-Stat | 13.09 | 1.80 | 13.30 | 13.30 | 13.09 |
| Geoclimatic Controls | Yes | Yes | Yes | Yes | Yes |
| Socioeconomic Controls | Yes | Yes | Yes | Yes | Yes |
| Province Fixed Effects | Yes | Yes | Yes | Yes | Yes |

Notes: This table reports IV estimates under four alternative functional forms for the endogenous variable, all instrumented by Retreat × Distance. Panel A uses a binary indicator (= 1 if killings per 1,000 population $> 0$) on the full sample. Panel B uses log(killings per 1,000 population), restricted to townships with positive killings. Panel C uses killings per 1,000 population in levels on the full baseline sample. Panel D uses killings per 1,000 in levels, restricted to townships with positive killings. The dependent variable in Column (1) is the log average night light intensity in 2010–2018. The dependent variables in Columns (2)–(4) are the percentage employed in manufacturing, the log value added per worker, and log average salary per worker in the year 2020. Column (5) is the share of the population with some college education from the 2010 census. Conley standard errors with a distance cutoff of 30km are shown in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

**Table B.3:** PPML vs. OLS First Stage under Progressive Trimming of Extreme Values of Killings

| | Full Sample (1) | Trim Top 5% (2) | Trim Top 10% (3) |
|---|---|---|---|
| *Panel A: Poisson (PPML)* | | | |
| Retreat × Distance | -0.0077 | 0.0325*** | 0.0334*** |
| | (0.0168) | (0.0125) | (0.0122) |
| Proportional Effect (%) | -0.8 | 3.3 | 3.4 |
| *Panel B: OLS with* $\log(1+Y)$ | | | |
| Retreat × Distance | 0.0162*** | 0.0100*** | 0.0071*** |
| | (0.0038) | (0.0015) | (0.0008) |
| Observations (PPML) | 1,161 | 1,107 | 1,051 |
| Observations (OLS) | 1,180 | 1,126 | 1,070 |
| Obs. Dropped by Trimming | 0 | 68 | 135 |
| Cutoff (killings per 1,000) | – | 7.28 | 2.43 |
| Max Killings per 1,000 | 232.4 | 7.2 | 2.4 |
| Skewness | 10.7 | 3.2 | 2.2 |

*Notes:* Panel A reports Poisson QMLE (`fixest::fepois()`) and Panel B reports OLS with $\log(1+Y)$ on the left-hand side, in each case regressing civilian killings per 1,000 population on *Retreat* × *Distance*. Columns (2) and (3) progressively trim townships with extreme killings per capita; percentiles are computed over the full estimation sample and the resulting cutoffs are reported in the table. The proportional effect is $\exp(\hat{\beta})-1$. All specifications include geoclimatic controls, socioeconomic controls, and province fixed effects. The PPML and OLS samples differ because one province records zero killings in every township, so the province fixed effect predicts the outcome perfectly and those observations do not enter the Poisson estimation. Conley (30km) spatial HAC standard errors are shown in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

**Table B.4:** Extended Sample Composition: All Areas

| Dependent Variables: | Log Night Light Intensity (1) | Manufacturing Employment Share (2) | Log Value Added per Worker (3) | Log Average Salary per Worker (4) |
|---|---|---|---|---|
| log(1 + killings per capita) | -2.130*** | -0.682 | -3.966*** | -0.924*** |
| | (0.642) | (0.483) | (1.029) | (0.343) |
| Observations | 1615 | 1261 | 1606 | 1603 |
| Mean | - | 0.21 | - | - |
| Kleibergen–Paap F–Stat | 11.45 | 2.45 | 11.31 | 11.43 |
| Geoclimatic Controls | Yes | Yes | Yes | Yes |
| Socioeconomic Controls | Yes | Yes | Yes | Yes |
| Province Fixed Effects | Yes | Yes | Yes | Yes |

Notes: The dependent variable in Column (1) is the log average night light intensity in 2010-2018. The dependent variables in Columns (2) – (4) are the percentage employed in manufacturing, the log value added per worker, and log average salary per worker in the year 2020, respectively. We include areas both north and south of the offensive line. Conley standard errors with a distance cutoff of 30km are shown in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

**Table B.5:** Effect of Killings: West-of-Line (predominantly within *Jeolla* Province)

| Dependent Variables: | Log Night Light Intensity | Manufacturing Employment Share | Log Value Added per Worker | Log Average Salary per Worker |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| log(1 + killings per capita) | -0.370** | -0.094** | -0.771*** | -0.099* |
| | (0.160) | (0.037) | (0.212) | (0.054) |
| Observations | 551 | 334 | 549 | 549 |
| Mean | - | 0.24 | - | - |
| Kleibergen–Paap F–Stat | 48.28 | 58.28 | 47.63 | 47.63 |
| Geoclimatic Controls | Yes | Yes | Yes | Yes |
| Socioeconomic Controls | Yes | Yes | Yes | Yes |
| Province Fixed Effects | Yes | Yes | Yes | Yes |

Notes: The dependent variable in Column (1) is the log average night light intensity in 2010-2018. The dependent variables in Column (2) – (4) are the percentage employed in manufacturing, the log value added per worker, and log average salary per worker in the year 2020, respectively. We include areas only south of the offensive line. The geoclimatic controls do not include dummies for being outside the offensive line, and distance to the offensive line. Conley standard errors with a distance cutoff of 30km are shown in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

**Table B.6:** Excluding Extreme Values

| Dependent Variables: | Log Night Light Intensity | Manufacturing Employment Share | Log Value Added per Worker | Log Average Salary per Worker |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| log(1 + killings per capita) | -2.989* | -0.207 | -6.443** | -1.315** |
| | (1.571) | (0.252) | (2.672) | (0.517) |
| Observations | 1045 | 823 | 1040 | 1037 |
| Mean | - | 0.21 | - | - |
| Kleibergen–Paap F–Stat | 23.12 | 9.88 | 24.02 | 24.28 |
| Geoclimatic Controls | Yes | Yes | Yes | Yes |
| Socioeconomic Controls | Yes | Yes | Yes | Yes |
| Province Fixed Effects | Yes | Yes | Yes | Yes |

Notes: The dependent variable in Column (1) is the log average night light intensity in 2010-2018. The dependent variables in Column (2) – (4) are the percentage employed in manufacturing, the log value added per worker, and log average salary per worker in the year 2020, respectively. Observations exclude towns at the highest 10 percent of civilian killings. Conley standard errors with a distance cutoff of 30km are shown in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

**Table B.7:** Robustness to the *Jeolla* Confound

| Dependent Variables: | Log Night Light Intensity (1) | Manufacturing Employment Share (2) | Log Value Added per Worker (3) | Log Average Salary per Worker (4) |
|---|---|---|---|---|
| *Panel A: Excluding Jeolla* | | | | |
| log(1 + killings per capita) | -5.045 | -0.886 | -10.715* | -2.286* |
| | (3.129) | (0.812) | (6.329) | (1.228) |
| Observations | 872 | 697 | 868 | 865 |
| Kleibergen–Paap F–Stat | 3.59 | 2.22 | 3.58 | 3.62 |
| Anderson–Rubin p–value | 0.001 | 0.047 | 0.000 | 0.000 |
| tF 95% CI Lower | . | . | . | . |
| tF 95% CI Upper | . | . | . | . |
| Geoclimatic Controls | Yes | Yes | Yes | Yes |
| Socioeconomic Controls | Yes | Yes | Yes | Yes |
| Province Fixed Effects | Yes | Yes | Yes | Yes |
| *Panel B: Jeolla Only* | | | | |
| log(1 + killings per capita) | -0.168 | 0.017 | -0.394 | -0.119* |
| | (0.200) | (0.040) | (0.240) | (0.063) |
| Observations | 310 | 185 | 308 | 308 |
| Kleibergen–Paap F–Stat | 18.57 | 24.30 | 18.97 | 18.97 |
| Anderson–Rubin p–value | 0.350 | 0.649 | 0.035 | 0.008 |
| tF 95% CI Lower | -0.699 | -0.081 | -1.029 | -0.286 |
| tF 95% CI Upper | 0.363 | 0.115 | 0.240 | 0.049 |
| Geoclimatic Controls | Yes | Yes | Yes | Yes |
| Socioeconomic Controls | Yes | Yes | Yes | Yes |
| Province Fixed Effects | Yes | Yes | Yes | Yes |

*Notes:* This table reports IV estimates of the effect of civilian killings on long-run development, testing whether our results are driven by the *Jeolla* provinces. The endogenous variable is log(1+killings per capita) throughout. Panel A excludes North and South *Jeolla* and retains our baseline specification, instrumented by *Retreat* × *Distance*. Panel B restricts the sample to North and South *Jeolla*, instrumented by *Distance* alone and following the control structure of the west-of-line specification. Both panels include geoclimatic controls, socioeconomic controls, and province fixed effects. The dependent variable in Column (1) is the log average night light intensity in 2010–2018. The dependent variables in Columns (2)–(4) are the percentage employed in manufacturing, the log value added per worker, and log average salary per worker in the year 2020. Anderson–Rubin $p$–values are robust to weak instruments; $tF$ confidence intervals follow Lee, McCrary, Moreira, and Porter (2022) and are unbounded in Panel A, where the first stage is weak. Conley standard errors with a distance cutoff of 30km are shown in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

**Table B.8:** IV Estimates Controlling for Major Battle Occurrence and Excluding Bombed Townships

| Dependent Variable: | Log Night Light Intensity | Manuf. Empl. Share | Log Value Added | Log Avg. Salary |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Panel A: Controlling for battle occurrence in township* | | | | |
| log(1 + killings per capita) | -0.950** | -0.024 | -1.899*** | -0.397*** |
| | (0.396) | (0.120) | (0.637) | (0.136) |
| Observations | 1182 | 882 | 1176 | 1173 |
| KP F-Stat | 53.19 | 22.54 | 52.88 | 52.10 |
| *Panel B: Excluding townships with bombing incidents* | | | | |
| log(1 + killings per capita) | -0.877** | -0.017 | -1.794*** | -0.349*** |
| | (0.402) | (0.127) | (0.668) | (0.116) |
| Observations | 1123 | 840 | 1117 | 1114 |
| KP F-Stat | 54.62 | 18.32 | 62.06 | 61.62 |
| Geoclimatic Controls | Yes | Yes | Yes | Yes |
| Socioeconomic Controls | Yes | Yes | Yes | Yes |

Notes: The dependent variable in Column (1) is the log average night light intensity in 2010-2018; (2)-(4) are measured in 2020. Panel A controls for whether a major battle occurred within the township boundary (228 battles from Ministry of National Defense records). Panel B excludes townships that experienced at least one US bombing incident documented by the Truth and Reconciliation Commission (2010). All specifications include geoclimatic and socioeconomic controls. Conley (1999) spatial HAC standard errors with 30km cutoff in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

**Table B.9:** Excluding Townships Distant from Seoul

| Dependent Variables: | Log Night Light Intensity | Manufacturing Employment Share | Log Value Added per Worker | Log Average Salary per Worker |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| log(1 + killings per capita) | -0.802** | 0.059 | -1.530** | -0.386*** |
| | (0.405) | (0.124) | (0.636) | (0.135) |
| Observations | 1000 | 702 | 994 | 991 |
| Mean | - | 0.21 | - | - |
| Kleibergen–Paap F–Stat | 40.04 | 34.26 | 41.16 | 40.40 |
| Geoclimatic Controls | Yes | Yes | Yes | Yes |
| Socioeconomic Controls | Yes | Yes | Yes | Yes |
| Province Fixed Effects | Yes | Yes | Yes | Yes |

Notes: The dependent variable in Column (1) is the log average night light intensity in 2010-2018. The dependent variables in Column (2) – (4) are the percentage employed in manufacturing, the log value added per worker, and log average salary per worker in the year 2020, respectively. We exclude towns within a 100km radius of Seoul. Conley standard errors with a distance cutoff of 30km are shown in parentheses. *$p <$ 0.1, **$p < 0.05$, ***$p < 0.01$

**Table B.10:** Excluding Townships Near Busan

| Dependent Variable: | Log Night Light Intensity (1) | Manuf. Empl. Share 2020 (2) | Log Value Added per Worker (3) | Log Avg Salary per Worker (4) |
|---|---|---|---|---|
| **Panel A: Baseline (Full Sample)** | | | | |
| log(1 + killings per capita) | -0.952** | -0.028 | -1.897*** | -0.399*** |
| | (0.400) | (0.120) | (0.638) | (0.138) |
| Observations | 1182 | 882 | 1176 | 1173 |
| Kleibergen–Paap F–Stat | 51.83 | 21.81 | 51.52 | 50.69 |
| **Panel B: Excluding < 10 km from Busan (dropping 8 townships)** | | | | |
| log(1 + killings per capita) | -0.972** | -0.013 | -1.859*** | -0.398*** |
| | (0.406) | (0.117) | (0.633) | (0.139) |
| Observations | 1174 | 874 | 1168 | 1165 |
| Kleibergen–Paap F–Stat | 50.37 | 21.49 | 50.18 | 49.45 |
| **Panel C: Excluding < 20 km from Busan (dropping 22 townships)** | | | | |
| log(1 + killings per capita) | -0.977** | -0.003 | -1.952*** | -0.411*** |
| | (0.412) | (0.119) | (0.691) | (0.143) |
| Observations | 1162 | 862 | 1156 | 1153 |
| Kleibergen–Paap F–Stat | 48.15 | 21.42 | 48.09 | 47.52 |
| **Panel D: Excluding < 30 km from Busan (dropping 44 townships)** | | | | |
| log(1 + killings per capita) | -0.828** | 0.028 | -1.631*** | -0.384*** |
| | (0.367) | (0.112) | (0.596) | (0.138) |
| Observations | 1143 | 844 | 1137 | 1134 |
| Kleibergen–Paap F–Stat | 45.38 | 22.41 | 44.89 | 45.14 |
| **Panel E: Excluding < 40 km from Busan (dropping 75 townships)** | | | | |
| log(1 + killings per capita) | -0.730** | 0.058 | -1.546** | -0.429*** |
| | (0.341) | (0.117) | (0.645) | (0.164) |
| Observations | 1119 | 821 | 1113 | 1110 |
| Kleibergen–Paap F–Stat | 40.25 | 20.37 | 40.81 | 41.62 |
| **Panel F: Excluding < 50 km from Busan (dropping 95 townships)** | | | | |
| log(1 + killings per capita) | -0.661* | 0.081 | -1.256** | -0.373** |
| | (0.339) | (0.123) | (0.581) | (0.160) |
| Observations | 1101 | 804 | 1095 | 1092 |
| Kleibergen–Paap F–Stat | 37.92 | 19.29 | 38.57 | 38.43 |
| **Panel G: Excluding < 60 km from Busan (dropping 147 townships)** | | | | |
| log(1 + killings per capita) | -0.608** | 0.176 | -0.770** | -0.203 |
| | (0.289) | (0.123) | (0.326) | (0.138) |
| Observations | 1058 | 765 | 1052 | 1049 |
| Kleibergen–Paap F–Stat | 31.28 | 15.81 | 31.22 | 31.13 |
| **Panel H: Excluding < 70 km from Busan (dropping 204 townships)** | | | | |
| log(1 + killings per capita) | -0.419 | 0.106 | -0.383 | -0.135 |
| | (0.319) | (0.113) | (0.343) | (0.117) |
| Observations | 1012 | 727 | 1009 | 1009 |
| Kleibergen–Paap F–Stat | 29.91 | 11.80 | 29.55 | 29.55 |

Notes: Civilian killings are instrumented by Retreat × Distance to the September 26 line. Panel A reproduces the baseline estimates from the full regression sample. Panels B–H progressively exclude townships whose centroid falls within the stated radius of Busan. Column (1) uses the log of mean nightlight intensity (averaged over 2010–2018). All regressions include the full set of geoclimatic controls (treated, distance to border, caloric suitability, elevation, slope, terrain ruggedness, distance to coast, distance to river, latitude, distance to Seoul), socioeconomic controls (population density 1949, literacy rate 1930, agricultural employment share 1930, manufacturing employment share 1930, log pre-war killings per capita), and province fixed effects. Conley (1999) spatial HAC standard errors with a 30km distance cutoff are reported in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

**Table B.11:** Controlling for Distance to Gyeongbu Expressway

| Dependent Variables: | Log Night Light Intensity | Manufacturing Employment Share | Log Value Added per Worker | Log Average Salary per Worker |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| log(1 + killings per capita) | -0.573* | 0.023 | -1.278*** | -0.285** |
| | (0.302) | (0.102) | (0.451) | (0.124) |
| Observations | 1182 | 882 | 1176 | 1173 |
| Mean | - | 0.21 | - | - |
| R-Squared | 0.46 | 0.18 | -0.07 | -0.06 |
| Geoclimatic Controls | Yes | Yes | Yes | Yes |
| Socioeconomic Controls | Yes | Yes | Yes | Yes |
| Province Fixed Effects | Yes | Yes | Yes | Yes |

Notes: This table shows the results after controlling for the nearest distance from each township to Gyeongbu Expressway. The dependent variable in Column (1) is the log average night light intensity in 2010-2018. The dependent variables in Column (2)–(4) are the percentage employed in manufacturing, the log value added per worker, and log average salary per worker in the year 2020, respectively. Conley standard errors with a distance cutoff of 30km are shown in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

**Table B.12:** Robustness: Controlling for Distance to Port

| Dependent Variable: | Log Night Light Intensity | Manuf. Empl. Share | Log Value Added per Worker | Log Avg Salary per Worker |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| log(1 + killings per capita) | -1.058** | -0.076 | -1.978*** | -0.429*** |
| | (0.418) | (0.132) | (0.652) | (0.151) |
| Observations | 1181 | 881 | 1175 | 1172 |
| Dep. Var. Mean | 3.155 | 0.214 | 10.861 | 3.126 |
| Kleibergen–Paap F–Stat | 48.31 | 16.27 | 47.94 | 47.54 |
| Geoclimatic Controls | Yes | Yes | Yes | Yes |
| Socioeconomic Controls | Yes | Yes | Yes | Yes |
| Distance to Port | Yes | Yes | Yes | Yes |
| Province Fixed Effects | Yes | Yes | Yes | Yes |

Notes: The dependent variable in Column (1) is the log average night light intensity in 2010-2018. The dependent variables in Column (2)–(4) are the percentage employed in manufacturing, the log value added per worker, and log average salary per worker in the year 2020, respectively. All regressions include the baseline geoclimatic and socioeconomic controls and distance to nearest port. Conley standard errors with 30km cutoff in parentheses. All regressions include province fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Table B.13:** Inclusion of South Korean Killings

| Dependent Variables: | Log Night Light Intensity | Manufacturing Employment Share | Log Value Added per Worker | Log Average Salary per Worker |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| log(1 + killings per capita) | -0.963** | -0.031 | -1.934*** | -0.407*** |
| | (0.400) | (0.117) | (0.626) | (0.132) |
| Observations | 1182 | 882 | 1176 | 1173 |
| Mean | - | 0.21 | - | - |
| Kleibergen–Paap F–Stat | 64.25 | 22.49 | 64.26 | 64.01 |
| Geoclimatic Controls | Yes | Yes | Yes | Yes |
| Socioeconomic Controls | Yes | Yes | Yes | Yes |
| Province Fixed Effects | Yes | Yes | Yes | Yes |

Notes: The dependent variable in Column (1) is the log average night light intensity in 2010-2018. The dependent variables in Column (2) – (4) are the percentage employed in manufacturing, the log value added per worker, and log average salary per worker in the year 2020, respectively. We include log(1+ killings by South Korean forces per 1,000) in our controls. Conley standard errors with a distance cutoff of 30km are shown in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

**Table B.14:** Sensitivity to Elevation Trimming

| | Full Sample (1) | ≤90th pctile (2) | ≤85th pctile (3) | ≤80th pctile (4) |
|---|---|---|---|---|
| *Panel A: Balance (Treated × Distance on pre-determined variables)* | | | | |
| Elevation | −0.975*** | −0.643** | −0.498** | −0.383* |
| | (0.355) | (0.260) | (0.224) | (0.220) |
| Slope | −27.87** | −19.66* | −14.52 | −10.71 |
| | (11.71) | (11.89) | (10.48) | (10.13) |
| Ruggedness | −905.9** | −630.0 | −457.6 | −329.2 |
| | (399.5) | (405.9) | (361.5) | (352.5) |
| Literacy 1930 | 0.000 | 0.000 | 0.000 | 0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| Shr Agriculture 1930 | 0.001 | 0.001 | 0.001 | 0.001 |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| Shr Manufacturing 1930 | 0.000 | 0.000 | 0.000 | 0.000 |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| Pop Density 1949 | −0.000 | −0.000 | −0.000 | −0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| $N$ (balance) | 1,347 | 1,213 | 1,144 | 1,078 |
| *Panel B: First Stage* | | | | |
| Retreat × Distance | 0.016*** | 0.016*** | 0.015*** | 0.014*** |
| | (0.002) | (0.002) | (0.002) | (0.003) |
| $N$ | 1,182 | 1,049 | 981 | 916 |
| *Panel C: IV Estimates* | | | | |
| Log Mean Night Light Intensity | −0.952** | −0.999*** | −1.184*** | −1.269*** |
| | (0.400) | (0.382) | (0.399) | (0.435) |
| Manuf. Employment Share | −0.028 | −0.053 | −0.062 | −0.106 |
| | (0.120) | (0.120) | (0.130) | (0.143) |
| Log Value Added per Worker | −1.897*** | −1.988*** | −2.229*** | −2.329*** |
| | (0.638) | (0.640) | (0.706) | (0.732) |
| Log Avg Salary per Worker | −0.399*** | −0.418*** | −0.469*** | −0.518*** |
| | (0.138) | (0.132) | (0.141) | (0.170) |
| Elevation Cutoff (m) | — | 225.8 | 202.2 | 184.3 |
| Observations | 1,182 | 1,049 | 981 | 916 |

*Notes:* Columns progressively trim townships above the indicated elevation percentile. Panel A reports coefficients from regressing each pre-determined variable on Treated × Distance, Treated, Distance, and province fixed effects. Panel B reports the first-stage coefficient on Retreat × Distance. Panel C reports IV estimates from the baseline specification; the dependent variable in Row (1) is the log average night light density, and the dependent variables in Rows (2)–(4) are the percentage employed in manufacturing, the log value added per worker, and log average salary per worker in the year 2020, respectively. All specifications include geoclimatic controls, socioeconomic controls, and province fixed effects. Conley standard errors with a 30km distance cutoff in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

**Table B.15:** Reduced Form Estimates

| Dependent Variables: | Log Night Light Intensity | Manufacturing Employment Share | Log Value Added per Worker | Log Average Salary per Worker |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Treated × Dist | -0.015** | -0.000 | -0.031*** | -0.007*** |
| | (0.006) | (0.001) | (0.010) | (0.002) |
| Observations | 1182 | 882 | 1176 | 1173 |
| Mean | - | 0.21 | - | - |
| R-Squared | 0.58 | 0.16 | 0.20 | 0.08 |
| Geoclimatic Controls | Yes | Yes | Yes | Yes |
| Socioeconomic Controls | Yes | Yes | Yes | Yes |
| Province Fixed Effects | Yes | Yes | Yes | Yes |

Notes: This table shows the reduced form estimates using the IV as an explanatory variable. The dependent variable in Column (1) is the log average night light intensity in 2010-2018. The dependent variables in Column (2)–(4) are the percentage employed in manufacturing, the log value added per worker, and log average salary per worker in the year 2020, respectively. Conley standard errors with a distance cutoff of 30km are shown in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

**Table B.16:** OLS Estimates

| Dependent Variables: | Log Night Light Intensity | Manufacturing Employment Share | Log Value Added per Worker | Log Average Salary per Worker |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| log(1 + killings per capita) | -0.086*** | 0.001 | -0.160*** | -0.022 |
| | (0.030) | (0.012) | (0.057) | (0.019) |
| Observations | 1182 | 882 | 1176 | 1173 |
| Mean | - | 0.21 | - | - |
| R-Squared | 0.51 | 0.15 | 0.15 | 0.06 |
| Geoclimatic Controls | Yes | Yes | Yes | Yes |
| Socioeconomic Controls | Yes | Yes | Yes | Yes |
| Province Fixed Effects | Yes | Yes | Yes | Yes |

Notes: This table shows the OLS estimates using the intensity of civilian killings as the explanatory variable. The dependent variable in Column (1) is the log average night light intensity in 2010-2018. The dependent variables in Column (2)–(4) are the percentage employed in manufacturing, the log value added per worker, and log average salary per worker in the year 2020, respectively. Conley standard errors with a distance cutoff of 30km are shown in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

**Table B.17:** Robustness to Alternative Clustering Methods

| Dependent Variables: | Log Night Light Intensity (2010–2018) (1) | Manufacturing Employment Share (2) | Log Value Added per Worker (3) | Log Average Salary per Worker (4) |
|---|---|---|---|---|
| Panel A | | Conley Distance cutoff: 30 km | | |
| log(1 + killings per capita) | -0.952** | -0.028 | -1.897*** | -0.399*** |
| | (0.400) | (0.120) | (0.638) | (0.138) |
| Kleibergen–Paap F–Stat | 51.83 | 21.81 | 51.52 | 50.69 |
| Panel B | | Conley Distance cutoff: 50 km | | |
| log(1 + killings per capita) | -0.952** | -0.028 | -1.897*** | -0.399*** |
| | (0.412) | (0.115) | (0.612) | (0.141) |
| Kleibergen–Paap F–Stat | 62.52 | 31.59 | 60.33 | 58.80 |
| Panel C | | Conley Distance cutoff: 100 km | | |
| log(1 + killings per capita) | -0.952*** | -0.028 | -1.897*** | -0.399*** |
| | (0.140) | (0.053) | (0.592) | (0.133) |
| Kleibergen–Paap F–Stat | 24.77 | 17.67 | 24.18 | 23.12 |
| Panel D | | County-level clustering | | |
| log(1 + killings per capita) | -0.952** | -0.028 | -1.897*** | -0.399** |
| | (0.418) | (0.136) | (0.664) | (0.183) |
| Kleibergen–Paap F–Stat | 22.23 | 14.23 | 22.32 | 22.45 |
| Observations | 1182 | 882 | 1176 | 1173 |
| Number of Clusters | 126 | 114 | 126 | 126 |
| Mean | - | 0.21 | - | - |
| Geoclimatic Controls | Yes | Yes | Yes | Yes |
| Socioeconomic Controls | Yes | Yes | Yes | Yes |
| Province Fixed Effects | Yes | Yes | Yes | Yes |

Notes: The dependent variable in Column (1) is the log average night light density in 2010-2018. The dependent variables in Columns (2)–(4) are the manufacturing employment share, the log value added per worker, and the log average salary per worker in 2020, respectively. Panels A–C report Conley standard errors with distance cutoffs of 30 km, 50 km, and 100 km, respectively. Panel D reports standard errors clustered at the county level. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

# C Placebo test

**Table C.1:** Reduced Form Estimates: Baseline and Placebo Treatment

| Dependent Variables: | Log Night Light Intensity (2010-2018) (1) | Manufacturing Employment Share (2) | Log Value Added per Worker (3) | Log Average Salary per Worker (4) |
|---|---|---|---|---|
| | | *Panel A. Baseline Sample* | | |
| Retreat × Distance | -0.008*** | -0.001* | -0.015*** | -0.003*** |
| | (0.002) | (0.001) | (0.003) | (0.001) |
| Observations | 1180 | 880 | 1174 | 1171 |
| Mean | - | 0.20 | - | - |
| R-squared | | | | |
| | | *Panel B. Placebo Sample* | | |
| Retreat × Distance | -0.004 | 0.001* | -0.000 | -0.002 |
| | (0.003) | (0.001) | (0.007) | (0.001) |
| Observations | 1013 | 881 | 1009 | 1006 |
| Mean | - | 0.20 | - | - |
| R-squared | | | | |
| Geoclimatic Controls | Yes | Yes | Yes | Yes |
| Socioeconomic Controls | Yes | Yes | Yes | Yes |
| Province Fixed Effects | Yes | Yes | Yes | Yes |

Notes: This table shows reduced form estimates on four key variables of long-run development using our IV as the explanatory variable. Panel A compares the baseline treated group (townships west of the offensive line) with our baseline control group (townships east of the offensive line). Panel B compares the placebo treated group (townships north of the offensive line), excluding townships within 25km of the MDL (ceasefire line from the 1953 armistice that divides North and South Korea) or in border area counties (Paju, Yeoncheon, Pocheon, Chuncheon) designated under the Border Area Support Act, which are subject to military facility protection zone development restrictions, to our baseline control group (townships east of the offensive line). The dependent variable in Column (1) is the log average night light intensity in the 2010s. The dependent variables in Column (2) – (4) are the percentage employed in manufacturing, the log value added per worker, and log average salary per worker in the year 2020. Conley standard errors with a distance cutoff of 30km are shown in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

# D Civilian Killings Dataset

**Source** The civilian killings data are digitized from *6.25 Sabyun Pisalja Myeongbu* [List of Victims of the 6.25 War], compiled by the Statistics Division of the Public Information Office of the Republic of Korea and published in 1952 (Statistics Division of Republic of Korea, 1952). The volume was reprinted in 2002 by *Wolgan Chosun* (Monthly Chosun), a Korean-language monthly magazine published by the Chosun Ilbo Media Group. The digitization underlying this paper is based on the 2002 reprint, which preserves

the content and layout of the 1952 original. The volume records 59,994 individuals identified by the South Korean government as civilians killed by North Korean forces between March 1950 and June 1951. For each victim, the volume reports name, gender, age, address of residence, location of death, and date of death.

**Digitization and Coding Protocol** The 2002 reprint was processed through optical character recognition (OCR), and the OCR output was manually corrected against the source pages on a record-by-record basis. A record was retained in the working dataset when the victim's address could be identified at the township (*eup, myeon, or dong* in Korean) level. Records without township identification—typically due to entries containing only a province or county name—were excluded. Given the importance of the timing of incidence in our analysis, records for victims without a date of death were also dropped. Missing values in other fields (e.g., name, age, gender, location of death) were not used as exclusion criteria.

The regional measure of killing intensity is calculated based on the victim's recorded address rather than the recorded location of death. Two considerations motivate this choice. First, locations of death are frequently reported without precise geographic detail, often as generic references to parks, churches, or schools that cannot be linked to a township in a consistent way. Second, larger-scale killings were in some cases carried out after victims had been gathered from a wider surrounding area, so that the location of death reflects an ad hoc assembly point rather than the community that bore the loss. Because the empirical analysis concerns local economic outcomes, the relevant population would be the one that lost community members, which is best measured by the victim's address of residence.

**Geocoding and Crosswalk** The victims' addresses in the original records are first matched to 1949 administrative townships, the latest available prior to the Korean War. We then map these 1949 townships to 1975 townships, the geographic unit of observation in the analysis. If recorded addresses could not be matched to the 1949 township list, these observations were excluded from the dataset. Since shapefiles for 1949 township boundaries are not available, we construct a crosswalk between 1949 addresses and 1975 townships.

Specifically, we manually track records of boundary revisions, mergers, and renaming using official administrative records and local historical accounts. If a 1949 township was split or absorbed in later reorganizations, the mapping was assigned to the dominant successor unit based on the relevant resources.

**Measurement Error** Two sources of measurement error in the killing intensity are worth noting. First, the dataset records fewer victims than the figure of approximately 120,000 reported in the 1955 Statistical Yearbook (Statistics Division of Republic of Korea, 1955). As discussed in the main text, the Yearbook estimate was produced in a context in which South Korean authorities had incentives to report a high count, while the 1952 volume is based on individual records and therefore provides a more conservative measure. Second, the exclusion of records without a township-level address removes a portion of the 59,994 source entries from the working dataset. These exclusions are driven by the level of geographic detail recorded by the compilers rather than by the location or circumstances of the killing, and there is no a priori reason to expect them to be correlated with township-level outcomes conditional on the controls used in the analysis.

# E Patterns of Civilian Killings

## E.1 Occupational Background

A central premise of our analysis is that civilian killings carried out by retreating North Korean forces were concentrated on highly educated individuals and people related to them. A direct evaluation of proposition is constrained by the absence of individual-level educational records for victims.

To address this concern, we draw on detailed information documented in the Truth and Reconciliation Commission (TRC) investigation reports—the government investigation records on killings presumed to have been committed by the North Korean military during the Korean War (Truth and Reconciliation Commission of Republic of Korea, 2010). As discussed in Section 3, the reports do not cover the full range of incidents, but they would be sufficient for characterizing the overall patterns of killings during this period. The TRC reports do not record educational attainment. However, they record the

occupations of victims in many cases, as well as the occupations of family members or other relatives in some instances. We use occupation as an observable proxy for educational status, given that the occupation–education gradient was extraordinarily steep in mid-twentieth-century Korea.

The 1960 Population Census shows that at the national level, only 15.7% of employed persons had completed more than six years of schooling, and just 8.5% had completed ten years or more.[40] Against this baseline, two occupational categories stand apart. Among professional and technical workers, 87.5% had at least seven years of schooling and 76.1% had at least ten years; among clerical workers, the corresponding figures were 77.9% and 57.7%. No other category approaches these magnitudes. Managerial workers (48.4% and 34.5%) come closest, but the spread within this group is likely to be wide.

More relevant to this study, where the sample townships are mostly drawn from rural areas (Section 3), the 1960 census reports corresponding figures for rural townships.[41] Average educational attainment is markedly lower: only 10.4% of employed persons had completed six or more years of schooling, and 4.7% had completed ten or more. Importantly, however, the earlier-observed occupation–education gradient persists. Among rural professional and technical workers, 84.8% had at least seven years of schooling; among rural clerical workers, 61.6%. Furthermore, these two occupational categories accounted for only 2.7% of the rural workforce. Taken together, within the rural communities in which the vast majority of targeted killings took place, individuals working in professional, technical, and clerical occupations formed a small and readily identifiable educated minority. Motivated by these patterns, and the possible sparse-ness of victim narratives given the exigency of these killings, we use a simple binary classification. We classify professional, technical, and clerical workers as "high-education" occupations and all other occupations as "low-education".

Next, we take this approach to the data by extracting victim-level occupational information from the TRC reports through textual analysis. Occupational information in the TRC reports does not follow a standardized classification. Entries vary substantially in granularity and form: some describe the victim's work only at the level of a broad sector (e.g., "farmer," "merchant,"

[40] 1960 is the earliest post-War census year reporting schooling cross-tabulated by occupational category.

[41] The Census provides statistics only for the overall rural population and does not provide township-specific statistics.

"teacher"), others record narrowly defined tasks, job titles, or the name of an employing firms and institutions. To render this information comparable across victims and consistent with the 1960 census occupational categories, we train and use a frontier generative AI model (Claude Opus 4.6) to extract open-ended occupational descriptions and map each entry to one of the high- and low-education occupations described above.

This procedure yields 3,603 victim narratives with addresses identifiable at the township level, of which 3,222 contain detailed occupational information for why a victim and/or family member was targeted and killed. The reason being that a large proportion of victims had missing narratives. This was expected given the exigency of killings and that, in many of the summary tables and preambles to each report, the Truth and Reconciliation Commission of Republic of Korea (2010) themselves noted a substantial number of cases where (i) a family had reported a victim as missing and presumed dead but no body was ever found; and (ii) mass killings where bodies were disposed of in unmarked, mass graves.

Even though these digitized narratives represents a fraction of the 49,027 victims in our baseline regression sample, we believe that this is the best-available data source we have from which to quantitatively validate that killings were disproportionately targeted against elites and the educated. Descriptive evidence suggests that these digitized, occupational killings are roughly representative of our total baseline killings. Figure E.1 displays a scatterplot of the two measures, where we find a correlation of 0.47 between digitized occupation-level victim data and our baseline measure of killings. Furthermore, within the retreat zone—where the vast majority of killings occurred—240 of 648 townships (37%) have digitized data, spanning all 7 retreat-zone (West-of-line) provinces, with similar mean latitude (35.68 vs. 35.69) and distance to the offensive line (67 vs. 61 km) as all retreat-zone townships in our baseline sample. Within these 240 townships, the digitized narratives cover approximately 19% of baseline killings per capita (mean 1.01 vs. 5.40 per 1,000 population).

We find strong support for the targeted nature of killings. Among victims for whom occupation information is available, 33.4% are classified as high-education occupations. Compared to the rural high-education employment share of around 2.7%, this figure indicates a substantial over-representation in

the population from which the victims were drawn. When victims are further classified based on the occupation of family members, the high-education share rises to 53.9%, suggesting that a majority of those killed either had high-education occupations themselves and/or had immediate family ties to individuals with high-education occupations.

This pattern should be interpreted with caution, since the victims recorded in the TRC reports are not a random sample. This selection may bias the observed composition in either direction. On the one hand, if targeted killings left fewer surviving family members or relatives, the likelihood of a highly educated victim being recorded would decrease. Conversely, highly educated victims may have been more likely to be reported if their relatives or descendants were better positioned to pursue a claim through greater literacy, familiarity with official procedures, or better documentary evidence of the incident, all of which would bias the observed share in the opposite direction.

While the latter possibility raises concerns about the premise of targeted killing, the key question is whether documentation bias alone could generate the observed pattern under the null hypothesis that killings were in fact indiscriminate. To assess this, let $\theta$ and $s$ be the true and observed shares of the high-educated among all victims, and let $r = p_H/p_L$ denote the ratio of documentation probabilities for high- and low-education victims. Then, $s/(1-s) = r \cdot \theta/(1-\theta)$.[42] Under the most conservative null (that killings were entirely indiscriminate), let $\theta$ equal 2.7% (the national rural high-education employment share). If so, reconciling this with the observed share of 33.4% would require $r = 18.1$. Even using the observed share of 53.9% (which includes the occupations of family members) would yield $r = 42.1$.

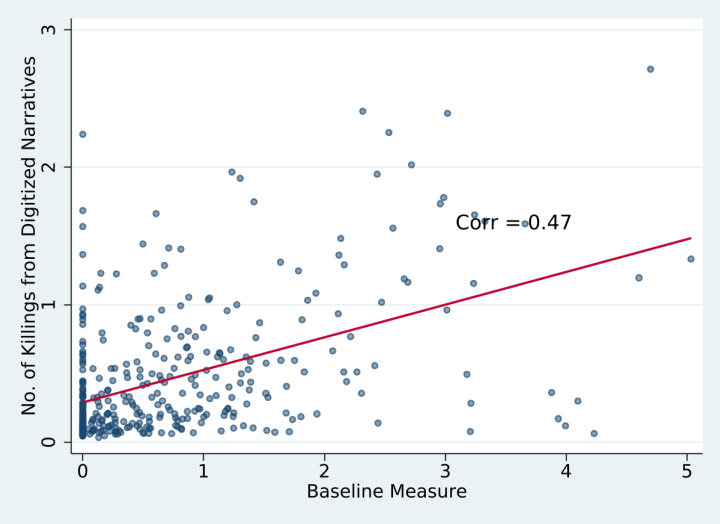
This implies that highly educated victims would need to be 18 to 42 times more likely to be documented than other victims, for selection bias alone to account for the observed pattern. Even at $\theta = 10\%$ (roughly four times the rural high-education employment share) $r$ would yield a $r$ of between 4.5 to 10.5. We note that higher values of $\theta$ would by definition imply substantial targeting of elites, which is precisely the premise we seek to establish. This exercise therefore rules out the most relevant alternative—that the overrepresentation of elites is an artifact of differential documentation rather than

[42] Let $p_H$ and $p_L$ be the probabilities that high- and low-education victims are documented in the TRC reports. The observed share is then $s = p_H\theta/[p_H\theta + p_L(1-\theta)]$, which leads to $s/(1-s) = (p_H/p_L) \cdot \theta/(1-\theta) = r \cdot \theta/(1-\theta)$.

reflecting the actual composition of victims.

Taken together, the occupational composition of victims identified in the TRC reports is difficult to reconcile with indiscriminate violence. The over-representation of professional, technical, and clerical workers, both among victims and their immediate family members, is significantly pronounced relative to the occupational distribution in rural communities at that time. This pattern aligns with our premise that civilian killings by North Korean forces largely targeted the educated stratum of rural society and their kin.

**Figure E.1:** Data Validity: Correlation between Victim Killings with Digitized Occupational Narratives and Baseline Killings




Notes: This figure plots our baseline township-level measure of log(1 + killings per capita) (x-axis)—used throughout all baseline regressions—against our occupational-based victim killings data from digitized case narratives (Truth and Reconciliation Commission of Republic of Korea, 2010) (y-axis) (also in log(1 + killings per capita)). Of the 1,347 townships in our baseline regression sample, 310 (23%) have digitized occupation data and only these townships are shown. Within the retreat zone—where the vast majority of killings occurred—240 of 648 townships (37%) have digitized data. Both axes are in log(1 + killings per capita) for ease of readability.

## E.2 Physical Destruction

If civilian killings were accompanied by destruction of local physical capital or community infrastructure, they could have shaped the post-war development path and human capital formation through channels independent of our preferred interpretation based on attitudes toward education.

Similar to Appendix E.1, we conduct a keyword analysis of TRC reports

to examine patterns of civilian killings. For each TRC report, we extract frequency counts of 31 keywords spanning six conceptual categories: primitive weapons, firearms, heavy weaponry, destruction and arson, killing methods, and references to local infrastructure. The results suggest that damage to local infrastructure was limited and that the destruction of fixed assets was unlikely to have been a dominant feature of these events.

Table E.1 summarizes the key results in three aspects. First, the weaponry recorded in the reports is overwhelmingly inconsistent with operations capable of inflicting large-scale damage on the built environment. As shown in Panel A, within the weapon-related lexicon, firearm references (small arms and execution shootings) and primitive implements (bamboo spears, knives, and clubs) account for 82% and 96% of weapon-related mentions, respectively. Overall, approximately 98.5% of all weapon-related mentions involve either primitive implements or small firearms, while explosive weapons account for only the remaining 1.5%. This pattern indicates that the killings were carried out almost exclusively with handheld weapons, agricultural tools, and improvised implements, which were incapable of destroying large-scale capital, assets, or infrastructure.

Second, descriptions of physical destruction are scarce. The TRC reports document the circumstances and details of each incident comprehensively; where large-scale physical destruction of facilities occurred, relevant descriptions would therefore be expected. In this view, we examine the prevalence of four destruction-related keywords: arson, burning, incineration, and total combustion across reports. As shown in Panel B, approximately half of the reports contain at least one of these terms. However, their occurrence is highly skewed: the top three reports account for roughly 46 %, and the top five for approximately 59 %, of all destruction-related mentions in the corpus.

Furthermore, classifying the target of each destruction-keyword mention suggests that productive economic infrastructure was rarely the primary target. As Panel C shows, approximately one-third of destruction-related mentions refer to the residences of victims or associated individuals burned as an act of retribution. A further 39% refer to police and security facilities used as killing sites. The remaining cases include administrative offices, churches, and "village-general" references, each accounting for 4%. In contrast, productive economic infrastructure does not appear as the target of any destruction-

related mention. Taken together, even where destruction did occur, it was concentrated on personal residences and security facilities, mitigating concerns about the potential destruction of economic infrastructure and its effects on local development.

The TRC reports were assembled to identify victims, not to document property damage. The absence of destruction-related language might therefore partially reflect the documentary scope rather than an actual absence of damage. Nevertheless, given the wide range of details covered by the reports, the three patterns discussed above—(i) the near-complete absence of heavy weaponry, (ii) a small and highly concentrated destruction-related lexicon, and (iii) the absence of productive economic infrastructure as a target—jointly suggest a minimal effect of local killing intensity on the destruction of physical capital and infrastructure.

**Table E.1:** Keyword Analysis Based on TRC Reports

| | Extensive margin (% of reports) | Intensive margin (% of total mentions) |
|---|---|---|
| Panel A. Weapon-related lexicon | | |
| Primitive weapons (bamboo spear, knife, club, . . . ) | 82% | 7.5% |
| Firearms (execution-style shooting, rifle, M1, . . . ) | 96% | 12.9% |
| Panel B. Arson and burning lexicon | | |
| Aggregate (arson, burning, incineration, total combustion) | 50% | 1.2% |
| Panel C. Targets of arson and burning | | |
| Private residence | – | 34% |
| Police and security facilities | – | 39% |
| Administrative office | – | 4% |
| Village (general reference) | – | 4% |
| Church | – | 2% |
| Unclear reference | – | 14% |

*Notes:* For each row in Panels A and B, the first column reports the share of reports containing at least one mention of the category, and the second column reports the share of within-panel mentions accounted for by the category. Panel C reports the distribution of destruction-related keyword contexts across target types.

# F Additional Mechanisms

**Table F.1:** Civilian Killings and Labor Market Outcomes (1960)

| Dependent Variables: | Share of Employment by | | | | |
|---|---|---|---|---|---|
| | Sector | | Occupation | | |
| | Agric. | Manuf. | Professional | White-collar | Laborer |
| | (1) | (2) | (3) | (4) | (5) |
| log(1 + killings per capita) | -0.053 | 0.035 | -0.018 | 0.017 | 0.020 |
| | (0.050) | (0.024) | (0.024) | (0.013) | (0.054) |
| Observations | 1031 | 1031 | 1031 | 1031 | 1031 |
| Mean | 0.79 | 0.05 | 0.11 | 0.07 | 0.28 |
| Kleibergen–Paap F–Stat | 44.89 | 44.89 | 44.89 | 44.89 | 44.89 |
| Weak-instrument-robust p-value | 0.253 | 0.095 | 0.434 | 0.189 | 0.696 |
| Geoclimatic Controls | Yes | Yes | Yes | Yes | Yes |
| Socioeconomic Controls | Yes | Yes | Yes | Yes | Yes |
| Province Fixed Effects | Yes | Yes | Yes | Yes | Yes |

Notes: The dependent variables are the share of employment in the agricultural and manufacturing sectors (Columns (1) and (2)), and the share of employment in each occupational category, calculated among non-agricultural workers (Columns (3) to (5))—all from the 1960 Census. Conley standard errors with a distance cutoff of 30km are shown in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

**Table F.2:** Civilian Killings and New Village Movement Transfers Per Person

| Dependent Variable: | Log NVM Transfer Amount Per Person | | | |
|---|---|---|---|---|
| | 1974 | 1975 | 1977 | 1978 |
| | (1) | (2) | (3) | (4) |
| log(1 + killings per capita) | 0.005 | -0.018 | -0.020 | -0.016 |
| | (0.021) | (0.035) | (0.041) | (0.030) |
| Observations | 924 | 925 | 925 | 925 |
| Kleibergen–Paap F–Stat | 34.65 | 34.73 | 34.73 | 34.73 |
| Geoclimatic Controls | Yes | Yes | Yes | Yes |
| Socioeconomic Controls | Yes | Yes | Yes | Yes |
| Province Fixed Effects | Yes | Yes | Yes | Yes |

Notes: The dependent variable is the natural logarithm of cash transfers per person, disbursed by the New Village Movement program. Conley standard errors with a distance cutoff of 30km are shown in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

**Table F.3:** Civilian Killings and New Village Movement Outcomes: County-Level Estimates

| Dependent Variables: | Share of local funding (1) | Actual-to-planned performance (2) |
|---|---|---|
| log(1 + killings per capita) | 0.032 | 0.621 |
| | (0.052) | (0.551) |
| Observations | 90 | 86 |
| Mean | 0.82 | 0.00 |
| Kleibergen–Paap F–Stat | 51.74 | 51.12 |
| Geoclimatic Controls | Yes | Yes |
| Socioeconomic Controls | Yes | Yes |
| Province Fixed Effects | Yes | Yes |

Notes: Conley standard errors with a distance cutoff of 100km are shown in parentheses. Share of local funding indicates the proportion of total project costs funded by local residents. Actual-to-planned performance refers to the ratio of implemented projects to the number planned, z-standardized for ease of interpretation. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

**Table F.4:** Controlling For Location of Government-Built Industrial Clusters

| Dependent Variables: | Log Night Light Intensity (1) | Manufacturing Employment Share (2) | Log Value Added per Worker (3) | Log Average Salary per Worker (4) |
|---|---|---|---|---|
| log(1 + killings per capita) | -0.853** | -0.027 | -1.571** | -0.294** |
| | (0.422) | (0.130) | (0.689) | (0.142) |
| Observations | 1182 | 882 | 1176 | 1173 |
| Mean | - | 0.21 | - | - |
| Kleibergen–Paap F–Stat | 44.92 | 19.98 | 44.21 | 43.30 |
| Geoclimatic Controls | Yes | Yes | Yes | Yes |
| Socioeconomic Controls | Yes | Yes | Yes | Yes |
| Province Fixed Effects | Yes | Yes | Yes | Yes |

Notes: The dependent variable in Column (1) is the log average night light intensity in 2010-2018. The dependent variables in Column (2) – (4) are the percentage employed in manufacturing, the log value added per worker, and log average salary per worker in the year 2020, respectively. We include a dummy for nine heavy engineering complexes at the county–level to account for industrial policy during the 1970s. Conley standard errors with a distance cutoff of 30km are shown in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

**Table F.5:** Civilian Killings and Demographic Composition Over Time

| Dependent Variables: | Prime–Age Population Ratio | | | | |
|---|---|---|---|---|---|
| | 1966 | 1975 | 1985 | 1995 | 2005 |
| | (1) | (2) | (3) | (4) | (5) |
| log(1 + killings per capita) | 0.007 | 0.010 | 0.002 | -0.040 | -0.113** |
| | (0.006) | (0.010) | (0.015) | (0.039) | (0.050) |
| Observations | 1072 | 1072 | 1072 | 1072 | 1072 |
| Mean | 0.30 | 0.31 | 0.35 | 0.37 | 0.38 |
| Kleibergen–Paap F–Stat | 51.95 | 51.95 | 51.95 | 51.95 | 51.95 |
| Geoclimatic Controls | Yes | Yes | Yes | Yes | Yes |
| Socioeconomic Controls | Yes | Yes | Yes | Yes | Yes |
| Province Fixed Effects | Yes | Yes | Yes | Yes | Yes |

Notes: The dependent variables are the share of the prime-age population for each year. Prime-Age individuals are defined as those aged 25-54. Conley standard errors with a distance cutoff of 30km are shown in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

**Table F.6:** Heterogeneity by Distance to Road

| Dependent Variable: | Log Night Light Intensity | Manuf. Empl. Share | Log Value Added per Worker | Log Avg Salary per Worker |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| **Panel A: Off-Road (Distance to Road > 0)** | | | | |
| log(1 + killings per capita) | -1.019* | 0.119 | -1.789** | -0.389 |
| | (0.572) | (0.181) | (0.745) | (0.262) |
| Observations | 282 | 205 | 279 | 278 |
| Dep. Var. Mean | 2.943 | 0.214 | 10.805 | 3.137 |
| Kleibergen–Paap F–Stat | 20.86 | 13.97 | 22.36 | 22.19 |
| Geoclimatic Controls | Yes | Yes | Yes | Yes |
| Socioeconomic Controls | Yes | Yes | Yes | Yes |
| Province Fixed Effects | Yes | Yes | Yes | Yes |
| **Panel B: On-Road (Distance to Road = 0)** | | | | |
| log(1 + killings per capita) | -0.800** | -0.058 | -1.747*** | -0.373*** |
| | (0.356) | (0.136) | (0.612) | (0.138) |
| Observations | 899 | 676 | 896 | 894 |
| Dep. Var. Mean | 3.225 | 0.214 | 10.878 | 3.123 |
| Kleibergen–Paap F–Stat | 45.60 | 14.11 | 50.59 | 52.16 |
| Geoclimatic Controls | Yes | Yes | Yes | Yes |
| Socioeconomic Controls | Yes | Yes | Yes | Yes |
| Province Fixed Effects | Yes | Yes | Yes | Yes |

Notes: The dependent variable in Column (1) is the log average night light intensity in 2010-2018. The dependent variables in Column (2) – (4) are the percentage employed in manufacturing, the log value added per worker, and log average salary per worker in the year 2020, respectively. Panel A restricts to townships with positive distance to nearest road (off-road). Panel B restricts to townships with zero distance to nearest road (on-road). Conley standard errors with 30km cutoff in parentheses. All regressions include province fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Table F.7:** Civilian Killings and Trust

| Dependent Variables: | Trust in | | |
|---|---|---|---|
| | Army (1) | Society (2) | People (3) |
| log(1 + killings per capita) | -0.048* | -0.046 | -0.191 |
| | (0.027) | (0.032) | (0.123) |
| Observations | 8766 | 7239 | 3932 |
| Mean | 0.48 | 0.53 | 0.47 |
| Standard Deviation | 0.34 | 0.19 | 0.50 |
| Kleibergen–Paap F–Stat | 29.00 | 24.58 | 23.08 |
| Geoclimatic Controls | Yes | Yes | Yes |
| Individual Controls | Yes | Yes | Yes |
| Province Fixed Effects | Yes | Yes | Yes |
| Survey–wave Fixed Effects | Yes | Yes | Yes |

Notes: The dependent variables are based on responses to KGSS surveys conducted between 2003 and 2019. The questions we use are: (1) "The armed forces is a core social institution. How much do you trust the people that lead these institutions?", (2) "How trustworthy is our society in your opinion?", and (3) "In general, do you think people can be trusted, or should you exercise caution?" Answers range from 0 to 10. For ease of interpretation, outcome variables are normalized to range from 0 to 1. Observations are weighted by individual survey weights. Conley standard errors with a distance cutoff of 30km are shown in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

# G Enrollment and Teacher Supply

A possible concern with the estimates in Table 7 is that, although the elasticity of teacher supply with respect to civilian killings is small, it could have had significant influence on school enrollment given the initially supply of teachers at the time. From this perspective, we conduct a back-of-the-envelope calculation that combines our reduced-form estimates with benchmark estimates from the literature on the effect of school inputs on educational outcomes.

In our 1955 county-level sample, the mean number of teachers per child aged 6–25 is 0.0102 and the mean enrollment rate for this age group is 0.49. Dividing the latter by the former yields a pupil-teacher ratio (PTR)—defined as enrolled pupils per teacher—of 48. Although this ratio is moderate by modern standards, the fact that fewer than half of school-age children were enrolled in 1955 implies that one teacher was available per approximately 100 school-age children, which confirms that the initial supply of teachers was thin. From Column (2) of Table 7, a one unit increase in log(1 + killings per 1,000 population) reduces log(teachers per child aged 6–25) by 0.005, equivalent to a 0.005 increase in log(PTR). At the mean baseline, this corre-

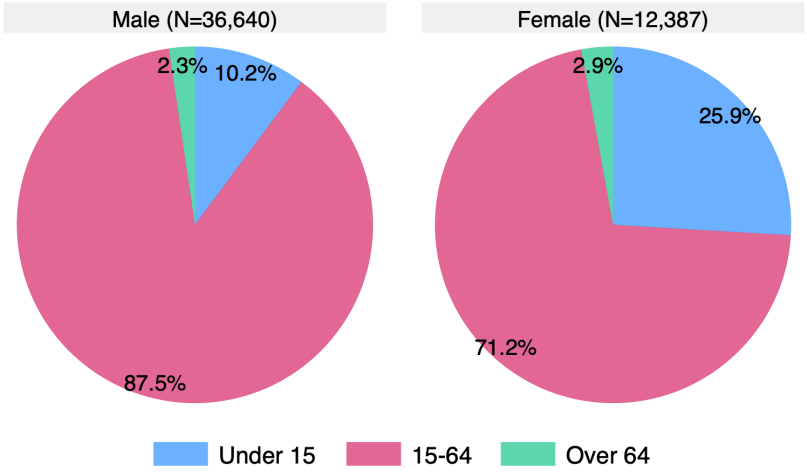
sponds to an increase of approximately 0.34 pupils per teacher, or a reduction of 0.071 teachers per $1,000$ children.

We translate this change in teacher supply into an implied enrollment effect using two benchmarks from the literature, which are based on educational conditions likely similar to those in Korea immediately after the war. First, following Case and Deaton (1999) on the early 1990s South Africa, we assume that a one-unit increase in the PTR reduces the probability of individual shcool enrollment by 0.2 percentage points. The implied reduction in enrollment is $0.002 \times 0.34 = 0.00068$, which corresponds to approximately 0.4% of the estimated coefficient $-0.166$ in Column (1). Second, following Duflo (2001) on the 1970s INPRES school construction program in Indonesia, we use the estimate that one additional primary school per 1,000 children increases schooling attainment by 0.12 to 0.19 years. Assuming three teachers per school, consistent with the INPRES staffing standard, the implied reduction in teacher supply corresponds to 0.024 fewer schools per 1,000 children, which translates into a reduction of 0.003 to 0.005 years of schooling. To make this magnitude comparable to the Column (1) estimate, we convert the $-0.166$ enrollment effect into years of schooling using the 13-year window from ages 6 to 18 ($-0.166 \times 13 = -2.16$ years), under which the teacher supply channel accounts for approximately 0.13% to 0.21% of the total schooling loss.

Taken together, the two benchmarks indicate that the teacher supply channel may account for at most approximately 0.4% of the observed decline in school enrollment. This suggests that the negative effects of civilian killings on educational outcomes cannot be explained primarily through reductions in teacher supply, consistent with our preferred interpretation that the documented effects operate through demand-side mechanisms rather than supply-side constraints on educational infrastructure.

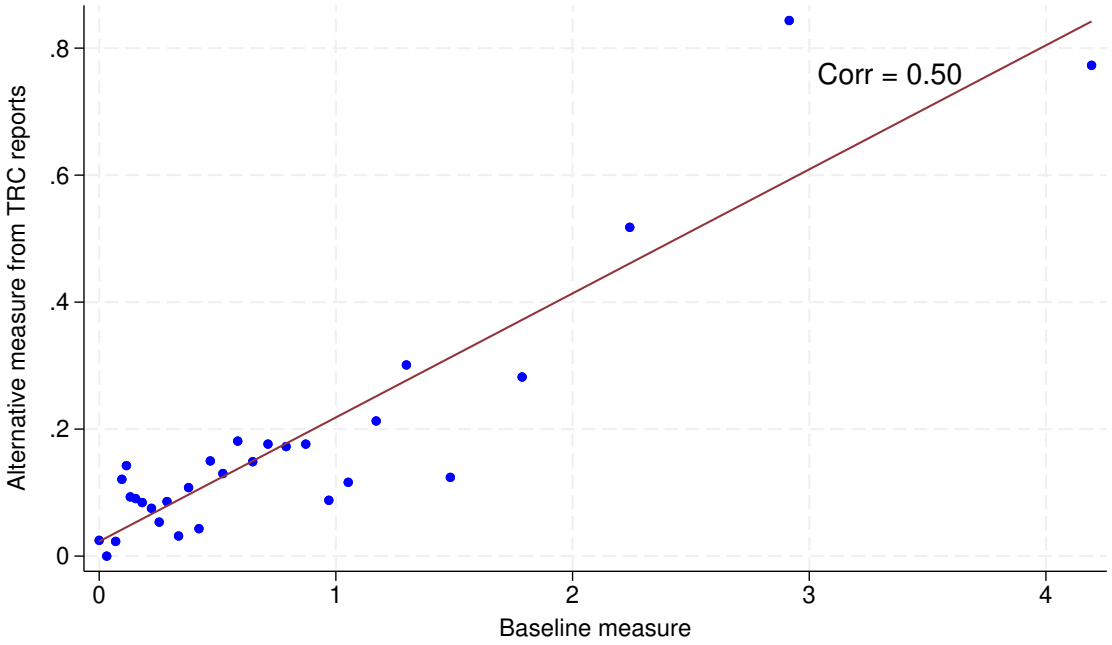
# H Additional Tables and Figures

**Figure H.1:** Gender and Age group of Civilian Victims




Notes: This figure plots the gender and age of civilians that were intentionally targeted and killed by North Korean military forces, using individual level information on age and gender digitized from the "List of Noncombatants Killed by North Korean Forces" (Statistics Division of Republic of Korea, 1952).

**Figure H.2:** Data Validity Check: Correlation with TRC reports




Notes: This figure plots a binned scatterplot comparing our baseline township-level measure of civilian killings (x-axis) with an alternative measure derived from the Truth and Reconciliation Commission of Republic of Korea (2010) reports (y-axis). Each point represents the average values within one of 60 equally sized bins.

**Table H.1:** Benchmarking of GDP per capita effects against comparable papers

| Study | Setting | GDP p.c. equivalent | Conversion |
|---|---|---|---|
| **This paper** | Korean War targeted killings (10% ↑ killing intensity) | −1.5% | NL-GDP elasticity (Henderson et al., 2012) |
| Riaño and Valencia Caicedo (2024) | Laos UXO (1 SD ↑ contamination) | −7.1% | Nightlights |
| Acemoglu et al. (2011) | Russian Holocaust (occupied oblasts with high Jewish population) | −16% | Descriptive: GDP p.c. $4,555 vs. $5,846 |
| Huber et al. (2021) | Nazi Aryanizations (expulsion of Jewish managers) | −1.8% of GNP | Stock market value |
| Becker et al. (2020) | Post-WWII forced expulsions to Poland | +2.0% | +0.82 yrs schooling × 8.8% return × 28% pop. share (Psacharopoulos and Patrinos, 2004) |
| Grasse (2023) | Cambodia Khmer Rouge (binary treatment, RD) | −0.6 SD of NL | Our study: −0.13 SD of NL per 10% killing increase (continuous treatment) |