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. As a key mechanism, we find that civilian killings led to a relative decline in structural transformation, potentially due to reduced investments in human capital.

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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, have 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 targeted killings of individuals based on their education or social status.[2] We know much less about the long-term effects of such episodes despite the importance of human capital in long-run growth, development, and structural transformation (Porzio et al., 2022).[3]

To that end, this paper asks: What are the long-run effects of targeted civilian killings on economic development? We hypothesize that targeted killings of local elites and intellectuals might dis-incentivize investments in human capital, leading to a long-term adverse effect on structural change and development. To answer this question, we examine a critical juncture in contemporary Korean history: the UN counter-offensive (the *Incheon Landing*) that turned the tides of the Korean War (1950-1953) and led to the victory of Allied forces over North Korea. Identification-wise, we leverage distance to the resulting, initial offensive line (Figure 1) formed by clashes between UN and North Korean forces as a plausibly exogenous shock to the spatial distribution of killing intensity over a brief 2-month period. In terms of data, we digitize a hitherto unused dataset on the location of individual-level civilian killings (Statistics Division of Republic of Korea, 1952), which allows us to construct a disaggregated township level dataset on the spatial distribution of killings, overcoming a key data limitation in the literature thus far.

We focus on the effects of targeted civilian killings perpetrated by North Korean forces.

[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.

[3]An important literature in economic history further highlights the outsized role of local knowledge, elites, or the upper tail of human capital for industrialization (Mokyr, 2005; Squicciarini and Voigtländer, 2015). We are unable to differentiate between the effects of killings on a (direct) loss in the upper tail of human capital vis-a-vis average human capital as (i) victim-level occupational data is largely too coarse and (ii) we do not have individual-level data on the origins of inventors in South Korea. Nevertheless, (i) we acknowledge the possibility of this channel and consider it potentially part of any observed effects of killings on reducing the incentives for broader human capital accumulation (ii) we study the effects of killings on the geographical distribution of government policy allocations in Section 6.3, a plausible proxy for the direct effects of elite deaths.

Three key characteristics of these killings make them well-suited for answering our question. First, the majority of casualties occurred over a short period due to sudden shifts in the course of the war. In our empirical analysis, we leverage the abrupt and unplanned retreat of North Korean forces as a plausibly exogenous shock to killings. 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 isolate the effects of targeted killings from other war-related consequences.[4] 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 this historical account, we collect and digitize the most comprehensive set of individual-level historical records on killings that occurred during the Korean War, which contain geographic information of every reported civilian killed by North Korean forces. We link this dataset to a wide range of development outcomes, including nightlight luminosity, sectoral composition, industrial productivity, human capital, and demographics across time.

Even with this disaggregated dataset of killings, estimating the causal impact of targeted killings is challenging because the intensity and location of killings could be endogenous. To address endogeneity concerns, we employ an instrumental variable (IV) that potentially influenced the spatial distribution of killings independently of initial socioeconomic conditions. Specifically, our IV strategy exploits two key regional differences shaped by the *Incheon Landing*, which determined the location and intensity 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 and North Korean defensive forces (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 Pusan to break out from the Pusan Perimeter and advance on to Seoul. These two pincer actions formed a temporary offensive line connecting the northwest (Seoul) and southeast (Pusan) as UN and South Korean forces took control over key towns along this line.

Second, killings by North Korean forces predominantly occurred West of the offensive line. As UN forces advanced and expanded the offensive line, North Korean troops retreated in response. Acting on orders from the North Korean authority to "eliminate all elements that

[4] South Korean authorities were also responsible for perpetrating civilian killings within their own territory. In the Bodo League Massacre at the beginning of the North Korean invasion, South Korean President Syngman Rhee ordered the execution of alleged Communist sympathisers. However, despite their historical significance, the civilian massacres perpetrated by South Korean authorities occurred selectively over an extended period, making causal inference challenging.

could support the UN forces upon landing," retreating forces carried out targeted killings in the areas they occupied. The duration of their occupation played a critical role in the scale of these killings, with longer occupations leading to higher casualties. As a proxy for the duration of North Korean occupation, we use the distance of each township from the offensive line.

Specifically, our instrument is constructed as the interaction between distance to the offensive line and an indicator for whether a town was located West of the line (Figure 1). The validity of our identification strategy rests on two key conditions. First, that the formation and shape of the offensive line was unanticipated by North Korean forces. This assumption appears ex-ante valid given that the *Incheon Landing* was widely deemed improbable, and was executed with extreme urgency under utmost secrecy: both the Japan and Korea press corps knew that an amphibious attack was imminent but the time and location were unconfirmed (Hanley, 2020). Second, townships further West of the offensive line experienced more intense killings. Due to the longer time required for UN forces to arrive, these places experienced an extended duration of occupation by retreating North Korean forces, leading to greater exposure to violence. In contrast, distance to the offensive line should not predict the intensity of killings for townships to the East, as there was little occupation of this region by retreating North Korean forces. Our data supports this: as distance to the offensive line increases, townships located West of the line experienced a significantly higher intensity of targeted killings while no such pattern is observed for townships East of the offensive line.

A potential concern is that townships located further West of the offensive line may have experienced more civilian killings due to existing local characteristics associated with past and future economic development. For example, if such townships initially had more educated individuals, this could be linked to disproportionate targeting by retreating North Korean forces, as well as subsequent local development through a separate channel, which would undermine the validity of our IV strategy. To address these concerns, we show that our instrument does not correlate with a host of pre-war local characteristics that are likely to affect economic development, including literacy rates and the share of employment in agriculture and manufacturing. In addition, throughout all our regressions, we control for a broad set of geographic and locational characteristics, including elevation, slope, ruggedness, and distance to rivers. We also control for other characteristics relevant for this particular context, such as distance to Seoul and pre-war differences in literary rates and sectoral shares in agricultural and manufacturing.

We find that, despite the absence of any immediate post-war impact on demographic outcomes, civilian killings led to a persistently negative effect on long-run development. Towns exposed to higher civilian killings in the 1950s continue to exhibit lower nighttime light intensity, higher agricultural employment shares, and reduced industrial productivity today. In terms of magnitudes, we find that a 10% increase in killing intensity leads to a 2.2% decrease in GDP per capita.[5] Notably, we also find large and persistently negative effects on human capital accumulation. These findings are robust to alternative sample compositions, including the exclusion of metropolitan areas around Seoul. Taken together, our findings suggest that targeted civilian killings had a long-lasting negative impact on long-run development.

Given that North Korean forces disproportionately targeted elites, educated individuals, and their families, we hypothesize that a fall in demand for human capital accumulation was possibly a key driver of the long-term negative impacts of killings on development. In support of this, we find an immediate negative impact of killings on educational investment: children of primary-school age in towns that experienced higher civilian killings were less likely to be enrolled in school in 1960. Similarly, a negative and larger effect is observed for secondary schooling in the 1966 census data. Importantly, we show that these results cannot be attributed to differences in contemporaneous economic development. In addition, we conduct a difference-in-differences type analysis to show that the negative effects on human capital accumulation started from as early as the first cohort that grew up during the thick of the Korean War and have persisted till today.

Next, we propose that the reduction in human capital accumulation led to a decline in structural transformation. If human capital was the primary driver of the slowdown in structural change, we would expect this relationship to manifest with a time lag, once cohorts with lower human capital investment enter the labor force. Our results support this view: negative effects of civilian killings on the prevalence of agriculture is observed only from the 1980 census, with no similar associations in earlier periods. Specifically, towns with higher civilian killings do not exhibit any significant differences in measures of agricultural employment in 1960 and 1970, but experience a relative increase in the share of agricultural households by 1980, suggesting a slowdown in structural transformation. This relative increase in agricultural prevalence was not accompanied by improvements in farm productivity. Collectively, our evidence suggests that the economic legacy of civilian killings operated through reduced human capital investment and its subsequent impediment to structural transformation.

Further evidence suggests that these downstream effects of targeted killings most plausibly originated from decreases in human capital accumulation. Specifically, we rule out alternative pathways related to the direct loss of local elites, state capacity, and trust. We do, however, find evidence of increases in selective out-migration but only beginning from 1995.

[5]While we are unaware of any studies that have studied the long-term impact of targeted killings on long-run development, our effect size is about a third smaller than the baseline estimate in (Riaño and Valencia Caicedo, 2024) where a one standard deviation increase in unexploded ordinances in Laos is associated with a 7.1% decrease in GDP per capita.

We interpret this as evidence that weaker economic development in the short to medium-run could have pushed prime-age individuals out of towns, reinforcing negative effects on long-run development.

Our findings extend the literature on the effects of violence—such as mass killings or forced relocations—on human capital and long-run development.[6] Such effects have been documented across diverse contexts, including Nazi violence (Acemoglu et al., 2011; Huber et al., 2021), the Boko Haram conflict in Nigeria (Bertoni et al., 2019), and La Violencia in Colombia (Fergusson et al., 2020). More closely related to our study, a growing literature highlights the salient impact of targeted violence against educated elites. For example, Grasse (2023) finds long-term negative effects of Khmer Rouge killings in Cambodia on average human capital and village development. 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). We innovate by showing how targeted killings of elites and their families at an early stage of the development growth path can inhibit long-run development and structural transformation. We provide evidence that this was possibly driven by a "terror" effect from targeted killings that led to a lower demand for human capital accumulation.

The lasting impacts of civilian killings on human capital accumulation further contribute to our understanding of the relationship between conflicts and development. While much of this literature focuses on the effects of large-scale bombings and physical capital destruction (Davis and Weinstein, 2002; Miguel and Roland, 2011; Feigenbaum et al., 2022), our study examines the long-term consequences of small-scale civilian killings, particularly those targeted at local elites and educated individuals. The paper closest to ours, Riaño and Valencia Caicedo (2024), found that unexploded ordinances have a persistently negative effect on economic development in Laos through hampering *average* human capital accumulation and inhibiting downstream structural transformation and rural-urban migration flows. Importantly, however, the authors find that effects on average human capital accumulation eventually fade after three to four decades. We find empirical support for some of these findings and provide novel evidence of how targeted killings can have a negative effect on *right-tail* human capital accumulation (lower share of college graduates) that continues to be visible nearly six decades later. This suggests that, while perhaps less visibly destructive, the targeted killings of elites and intellectuals can have an even more enduring effect on human capital accumulation, even in the absence of physically hazardous factors.

Lastly, this paper adds to the literature on historical conflicts in Asia (Miguel and Roland,

[6] A related strand of literature studies the effects of conflict and violence on local collective action (Bellows and Miguel, 2009), altruism and risk-seeking behavior (Voors et al., 2012), and schooling (Shemyakina, 2011).

2011; Dell et al., 2018; Dell and Querubin, 2018), which have been profoundly shaped by colonial rule and ideological confrontations between Western powers. The Korean War, rooted in the broader ideological struggle between the Soviet bloc and Western democracies, is an enduring but extremely understudied context in economic history. Most existing research has focused on the long-term effects of the Korean War on contemporary attitudinal and political outcomes (Hong and Kang, 2017; Joo, 2023). We contribute by providing the first quantitative evidence of the economic legacy of the Korean War, combining a comprehensive, newly digitized dataset on individual-level victimization with various detailed census datasets.

2 Historical Background

This section describes the historical background to the Korean War and the formation of the UN offensive line, with a focus on key elements that form the basis for our identification strategy in Section 4.2.

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 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

[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.

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 Pusan Perimeter. Shortly after the Korean War began, defeated UN forces were pushed back to the Pusan Perimeter, a temporary defensive line around Pusan (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 Pusan to break out from the Pusan 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 advance 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 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 Chomite 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 4.2.

Civilian Killings and the UN Offensive Line The Korean War had a higher proportion of civilian deaths than other major modern conflict, 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]

Killings perpetuated by the North Korean armed forces began after the onset of the war, as North Korean forces carried out civilian killings in the process of taking control of parts of South Korea. Following their invasion, North Korean forces quickly advanced along the southwestern route of the Korean Peninsula, killing civilians who obstructed their progress. However, the scale of these killings was relatively limited, as the primary objective of North Korea was to capture territory as quickly as possible (Razuvaev, 2001; Kim, 2012a).

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 (Statistics Division of Republic of Korea, 1952), with 60% inflicted during this period.

Importantly, the civilian killings carried out after the Incheon Landing differed from those perpetuated at the onset of the war in two ways. First, 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)." Consequently, unlike earlier killings aimed at facilitating rapid advancement, the incidents after September 1950 targeted individuals who might potentially aid the ROK and UN forces. These individuals included local elites, highly educated individuals, or those fluent in foreign languages. Killings extended beyond these identified "elements." Many individuals were killed just because they were family members or associates of suspected supporters of the allied forces (Kim, 2000; Yim, 2020).

Second, the spatial distribution of killings were shaped by the UN offensive line which determined the direction of feasible North Korean retreat routes. The rapid post-Incheon

[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 5.3, we show that our results are robust to controlling for the intensity of killings perpetuated by South Koreans.

Landing UN counteroffensive allowed UN forces and the South Koreans to take control over key towns connecting the northwest (Seoul) and the southeast (Pusan). This rapid advance directly isolated all North Korean troops that happened to be caught to the West and South of the offensive line. Moreover, the rapid, subsequent expansion of the UN offensive line in the following two weeks (Yoo, 2010), forced North Korean forces to retreat further outwards, leading to the concentration of targeted killings in these areas.

Reports from the Truth Commission (Truth and Reconciliation Commission of Republic of Korea, 2010) provide further evidence for the targeted nature of civilian killings towards elites and educated individuals. 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. Separately, another report describes a case in *Inja-myeon* in which right-wing figures, village leaders, and public officials were forcibly assembled in each village and executed by gunfire.

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 because of their associations with village leaders and government officials." In one case, "$\cdots$ 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."

3 Conceptual Framework

Civilian killings and conflict may influence long-term economic development through a number of channels (e.g., Besley and Persson, 2010; Cassar et al., 2013; Dell and Querubin, 2018; Dupraz and Ferrara, 2023). In this section, we elucidate key mechanisms in the South Korean context suggested by the broader socio-anthropological literature.

Targeted killings and the demand for human capital accumulation The targeted nature of killings towards (families of) elites and intellectuals suggest that changes in the demand for higher education may be a key explanation for observed negative long-run outcomes on economic development. As illustrated in Section 2, the majority of victims had relatively higher levels of education or close familial ties to these individuals. Hence, we hypothesize that targeted killings might have led to a "terror" effect on the greater populace towards the demand for education. While data limitations prevent us from completely ruling out alternative explanations, we discuss other possible explanations such as income shocks or a decrease in teachers and schools in Section 6.1.3.

Specifically, direct and indirect experiences of oppression associated with higher education can reduce incentives to invest in human capital through several pathways. The most immediate channel would be the effect of perceived threat. If education or ties to educated networks are viewed as increasing vulnerability to persecution, the perceived returns to education may decline, fostering a sense of education as a liability rather than an asset. Psychological mechanisms may further reinforce this effect, as fear and trauma from past persecution can lead to an aversion to activities perceived as risky (Young, 2019; Rockmore and Barrett, 2022). Changes in the local environment may also have strengthened conditions less conducive to human capital accumulation. For instance, individual-level effects may collectively contribute to the formation of negative social norms and attitudes toward human capital accumulation. Alternatively, the loss of educated individuals and their networks likely weakened intellectual communities, reducing positive externalities like knowledge diffusion and peer effects (Borjas, 1995). These, in turn, could perpetuate a cycle that further discourages investment in human capital.[9]

Effects on Structural Transformation After the Korean War, South Korea transformed from one of the world's poorest agrarian economies into a global industrial leader in just a few decades, exemplifying the process of structural transformation. This rapid shift was driven not only by government policies but also by significant investments in human capital. Families prioritized education, often making substantial sacrifices to ensure their children could acquire the skills needed for higher-paying jobs in emerging industries. These collective efforts, combined with government initiatives to expand access to education, created a highly skilled and adaptable workforce (Kim and Topel, 1995).

However, the pace of structural transformation varied significantly across regions within South Korea. While some areas transitioned quickly from an agrarian economy to manu-

[9]For instance, the literature on the Cultural Revolution in China suggests that persecution of educated and elite classes led to a decline in perceived value of education (Bai and Wu, 2023) and reduced emphasis on personal effort (Qian, 2024), reinforcing the long-term impact of the political violence.

facturing and services, others remained dependent on agriculture for a longer period. These uneven rates of transformation likely contributed to regional disparities in long-term development outcomes, with more industrialized regions achieving higher income levels and greater economic advancement over time. Moreover, human capital may be a critical factor in this process. For example, Caselli and Coleman II (2001) demonstrate that regional convergence in income levels in the United States is primarily explained by structural transformation, with human capital accumulation playing a key role in labor reallocation. Similarly, Porzio et al. (2022) extend this argument, showing that nearly half of the global decline in agricultural employment can be attributed to new cohorts entering the labor market, with human capital growth among these cohorts serving as a critical driver of this transition.

In this context, we propose structural transformation as a mechanism through which civilian killings during the Korean War had long-term economic consequences. As discussed above, towns that experienced greater exposure to civilian killings likely saw a reduction in household investment in human capital. This reduction in human capital accumluation would have slowed the reallocation of labor from agriculture to higher-productivity sectors, thereby decelerating structural transformation during South Korea's critical period of rapid economic growth.

Importantly, we hypothesize that the negative effects of civilian killings on structural transformation emerged with a time lag. If reduced human capital investment is the primary channel, the consequences would only become evident when the generations born or educated during the war period began entering the labor market. This suggests that the link between civilian killings and agricultural prevalence would not have been immediate but would instead have surfaced 15–20 years later, as the affected cohorts reached working age. To test this hypothesis, we analyze data on agricultural prevalence over time, examining whether regions exposed to higher levels of civilian killings experienced slower transitions out of agriculture as these cohorts entered the workforce.

Alternative Channels A decline in human capital investment may not be the only channel through which targeted killings might have affected long-run economic development outcomes. The direct loss of state capacity due to the deaths of local elites could be a key alternative explanation (Galor et al., 2009; Martinez-Bravo, 2017). Their loss, even in small numbers, might have reduced the provision of public goods in affected townships. This explanation could particularly be pertinent given extensive rural and industrial policies implemented by the Korean government during the 1970s (Ban et al., 2020; Kim et al., 2021; Lane, 2024).

Selective migration may also be a channel. Killings might have led to greater out-migration to urban areas, due to trauma or the destruction of productive economic capacity in home regions. In particular, rural-to-urban migration in Korea was highly age-selective, with working-age individuals more likely to move to urban areas (Kim, 1982; Lee and Kim, 2020), potentially correlated with regional variations in human capital investments. Lastly, declines in trust could play a role. This is plausible given the documented effects of conflicts on trust (Nunn and Wantchekon, 2011; Tur-Prats and Valencia Caicedo, 2020) and the significant link between social capital and economic development (Knack and Keefer, 1997).

In Section 6.3, we test the plausibility of the proposed alternative channels, finding that they are unlikely to serve as valid explanations. Although we cannot completely rule out the influence of other factors, the later discussion bolsters the credibility of our proposed mechanism.

4 Data and Empirical Strategy

This section discusses our data sources and identification strategy. Section 4.1 introduces the archival records of civilian killings that we collect and digitize. Based on this, Section 4.2 proposes the instrumental variable approach.

4.1 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 detailed demographic information on civilians killed by North Korean Forces, such as name, gender, age, and hometown.[10] Crucially, it records the location

[10] Unfortunately, the demographic information in the dataset does not allow for the identification of local elites. 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 inconsistencies in the recording of occupations complicate analysis. In urban areas, occupations are recorded with relative precision, such as "Interior Ministry personnel manager," "U.S. embassy employee," and "Jeonbuk Daily vice president." However, occupations in rural areas are categorized more broadly by industry, with terms like "Farming" or "Commercial" used instead of specific roles. While most rural victims are listed under "Farming," the dataset does not distinguish between landowners, farm managers, and tenant farmers, which are crucial for understanding socioeconomic status. Moreover, positions in non-profit socio-political organizations, such as "Daehan-Cheongnyeondan" or "Hanguk-Cheongnyeondan," are often recorded as occupations, further complicating interpretation.

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.[11] 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, this data. However, the Yearbook's estimate could have exaggerated the scale of killings. As 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.[12]

Figure 2 illustrates trends in the number of civilian killings from March 1950 to June 1951. Each dot denotes the total number of civilians killed by North Korean forces each month.[13] 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 4.2). 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 towns located in the West but not for towns 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

[11] We present summary statistics on gender and age groups in Figure A.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.

[12] It should be noted that our dataset only covers civilian killings committed by North Korea. While killings by South Korean and Allied forces also occurred during the war, there are no comprehensive records covering the entire South Korean region at the sub-national level, which prevents their inclusion in our analysis.

[13] 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.

clashes. In the North (blue), there is no clear relationship between killing intensity and distance to the offensive line, with some towns 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 population data for these regions are not available before the 1970s. However, this restriction is unlikely to affect our analysis. Our hypothesis focuses on structural transformation, where effects on large metropolitan areas would be less relevant. 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, the exclusion of towns 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, 2024). In Section 6.3, we conduct a robustness check, showing that our findings are not sensitive to regional exposure to industrial clusters established under these policies.

4.2 Instrumental Variable 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 town was located in the West. This IV is motivated by the fact that the North Korean retreat towards the West led to a higher intensity of killings in towns located further from the offensive line. That is, distance from the offensive line potentially captures the extent of township-level exposure to North Korean occupation by retreating North Korean forces until the point of UN liberation.

Figure 4 plots the township-level correlation between distance to the offensive line—the nearest distance from the centroid of each town to the closest point on the offensive line—and the intensity of civilian killings, as measured by the log number of victims per 1,000 population. We restrict killings to those that occurred after the Incheon Landing. We plot these correlations separately for towns located in the West, North, and East. Panel A shows that, consistent with our hypothesis, the scale of killings tends to increase with distance from the offensive line for towns in the West that were occupied by North Korean forces at the time of the Incheon Landing. In contrast, Panel B shows no such correlation for towns in the North and the overall intensity of killings is lower than that in the West. Panel C further confirms that civilian killings in towns in the East (occupied by UN forces) were rare and quantitatively much smaller. This regional heterogeneity lends support to our intuition that the retreat of North Korean forces from the offensive line was a key determinant of the spatial distribution of civilian killings but only in the West where there was no clear retreat route towards North Korea.

We examine this statistical relationship formally by estimating the following equation:

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

$\log(CivilianKillings_k)$ represents our preferred measure of killing intensity—the log number of victims per 1,000 population in town k—that measures the number of civilian killings that occurred after the Incheon Landing. $Retreat_k$ 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 (i.e., occupied by North Korean forces), and $Dist_k$ denotes distance to the nearest point on the offensive line.[14] The interaction term, $Retreat_k \times Dist_k$, is our variable of interest and captures the difference in killing intensity between towns located in the West and North, further from the offensive line, vis-a-vis towns 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.[15] 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. δ_p indicates province fixed effects, which account for time-invariant provincial factors that may affect long-run development outcomes. Importantly, we always control for the intensity of pre-Incheon Landing killings given that these might also have affected latter-day development independently of targeted

[14]Towns to the East, within the UN offensive line, face two lines. We use the minimum distance to the nearest of the two lines.

[15]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.

killings after the Incheon Landing occurred. Standard errors are adjusted following Conley (1999), with a distance cutoff of 30km.[16]

Table 1 summarizes results. Odd-numbered columns include town-level geoclimatic controls, while even-numbered columns additionally control for socioeconomic characteristics. We vary the estimation sample 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 estimation sample to towns located in the West and East only. Results are robust to alternative sample compositions (Appendix B). To that end, the average number of killings per 1,000 population at the township-level is 1.73 and the average township population is 24,846 in our baseline estimation sample.

Despite the strong first-stage relevance, concerns about the exogeneity condition 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.

Appendix Table A.1 tests this possibility. We restrict our estimation sample only to towns in the West and East and regress a battery of predetermined geographical features and pre-Korean War local economic characteristics on our instrument, controlling for province fixed effects.[17] We find balance across a number of variables including caloric suitability, distance to the coast; distance to rivers; distance to Seoul, as well as 1930 literacy rates and the

[16]This corresponds to approximately one-tenth of the width of the Korean Peninsula. A 30 km radius includes an average of about 167 towns.

[17]While we would ideally test for pre-Korean War measures of political preferences, the Korean Peninsula was under Japanese colonial rule up until the eve of the Korean War and, to the best of our knowledge, such data does not exist.

1930 share of employment in agriculture and manufacturing. While distance to the border is negatively correlated with elevation, slope, and ruggedness specifically in the area west of the offensive line, this is consistent with the fact that Korea's major mountain ranges are primarily located in the northeast, while its plains are concentrated in the southwest. Moreover, we control for all these variables in our baseline regressions to mitigate any potential bias related to them.

Given the plausible exogeneity and relevance of $Retreat \times Dist$ as an instrument for the intensity of targeted killings, our main estimating equation is as follows,

$$Y_k = \alpha + \beta \log(CivilianKillings_k) + \mu_G' X_k^G + \mu_S' X_k^S + \delta_p + \epsilon_k \quad (2)$$

where we instrument $\log(CivilianKillings_k)$ with $Retreat_k \times Dist_k$. All other notations are identical to those in Equation (1), except that $Retreat_k$ and $Dist_k$ are now included in X_k^G. Given that the first stage relationship does not hold for the towns in the North, we restrict our baseline estimation sample only to towns located in the West and East. Nevertheless, results are robust to alternative sample compositions (Appendix B). Standard errors are adjusted following Conley (1999), with a distance cutoff of 30km. All variables are harmonized with the 1975 town boundaries.[18]

In addition to the relevance and exogeneity conditions, our key identification assumption is that distance to the offensive line did not differentially affect post-Korean War economic development in the West, except through the intensity of targeted killings. While we cannot formally test this condition, we consider the exclusion restriction plausible given the temporary nature of the offensive line. Established in a matter of weeks during the unexpected and rapid northwestern advance of UN forces, this boundary was short-lived and became irrelevant shortly after. Specifically, in the two years after the formation of the offensive line, fluid and rapidly evolving battles erupted all across the Korean Peninsula with the front lines eventually coalescing around the present-day Armistice Line (located far from the offensive line). Hence, it is unlikely that the offensive line——a transient boundary formed in September 1950 that quickly became obsolete——had a direct impact on subsequent economic development except through the intensity of targeted killings.

Immediate demographic impacts What was the immediate impact of civilian killings on the demography of affected towns? We formally test this by regressing the demographic

[18]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 towns in the pre-1975 periods and those in 1975 by tracking records of administrative unit changes.

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 did not have a significant effect on post-war population, which aligns with our argument 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 negligible 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. In the following sections, we examine how civilian killings influenced long-term local development outcomes and explore potential channels through which these effects persisted.

5 Civilian Killings and Long-Run Development

This section examines the long-run effects of civilian killings on contemporary economic development. We find that towns with greater exposure to civilian killings during the war exhibit lower nighttime luminosity today. This long-term negative effect is corroborated by a greater prevalence of agriculture and lower industrial productivity. We present key robustness checks but defer alternative explanations to the next section.

5.1 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.[19] The estimates are negative and statistically significant. The results indicate that a 10% increase in killing intensity led to a 9.4% decrease in night light intensity in 2015. We proceed to interpret the economic significance of this number by relying on estimates of the elasticity of nightlights to GDP. Specifically, we use our baseline specification in Column (5) of Table 3 and elasticities reported in Beyer et al. (2022); Kim et al. (2023); Henderson et al. (2012). A 10% increase in killing intensity corresponds to a 3.9−6.1% decrease in GDP and a 2.2% decrease in GDP per capita.[20] While not strictly comparable, a 2.2% decrease in GDP per capita is about one-third smaller than the estimate in (Riaño and Valencia Caicedo, 2024), where a one standard deviation increase in unexploded ordinances (UXO) in Laos is associated with a 7.1% decrease in GDP per capita.[21]

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, despite their potential limitations, likely reflect the long-term negative effects of civilian killings that emerged following the end of the Korean War.

5.2 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, with only limited remote access provided. Due to data confidentiality policies, the number of towns included in the analysis of employment shares is

[19] We present reduced form estimates for key outcome variables in Table A.2.

[20] To reach the estimate on GDP: Kim et al. (2023) uses Chinese county-level data on nighttime luminosity and GDP and reports an elasticity of 0.419, thus, $9.4 \times 0.419 = 3.9\%$. Beyer et al. (2022) employs country-level data and provides an estimate of 0.65, thus, $9.4 \times 0.65 = 6.1\%$. Note that these two studies use different datasets and approaches in estimating the elasticity parameter, both of which we judge to be possibly more relevant to the Korean setting. To reach the estimate on GDP per capita, we re-estimate the same regression using the raw level of nightlights per township and obtain a correspondent coefficient of 22.0 on a sample mean of 28.3. We then use the estimated elasticity of GDP to lights of 0.277 of Henderson et al. (2012) and calculate the corresponding decrease in GDP per capita as $[(22.0/100) \times 10]/28.3) \times 0.277 = 0.0215$.

[21] The larger effects in (Riaño and Valencia Caicedo, 2024) is 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 6.3.

smaller than other regressions.[22]

Table 4 shows the long-term impacts of civilian killings on sectoral composition and productivity. Panel A focuses on the manufacturing sector. While Column (1) suggests minimal impact on the manufacturing employment share, the effects on productivity measures are significant. 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. The estimates indicate that a 1% increase in civilian killings relates to a 1.9% decrease in manufacturing value added per worker (Column (2)) and a 0.4% decrease in average salary per worker (Column (3)).

Given the prevalence of rural towns in our sample, Panel B examines agricultural outcomes. Column (1) suggests that civilian killings are positively associated with the share of employment in agriculture.[23] Interestingly, the greater prevalence of agriculture is accompanied by larger farm sizes. Column (2) shows that a 1% increase in civilian killings predicts a 0.74% increase in farm size. Two possibilities could be consistent with these results: (1) larger farm sizes may reflect higher productivity through economies of scale, or (2) they may suggest lower agricultural productivity, as suggested by the "inverse relationship" between farm size and productivity (Bardhan, 1973; Barrett, 1996; Gollin and Udry, 2021). The Census of Establishments does not record productivity measures such as farm yields or crop values to directly adjudicate between these two possibilities. Nevertheless, the result in Column (3) weakens the plausibility of productivity gains through scale economies. Using the share of mechanized farmland as the outcome variable, it shows that the increase in farm size was not accompanied by greater mechanization, suggesting that the increase in farm size is not attributable to more technologically advanced agricultural practices.

Overall, these results indicate that the long-term negative effects of civilian killings are reflected in a relative decline in structural transformation. The reduction in agricultural labor reallocation and the lower industrial productivity point to both quantitative and qualitative setbacks in this process. In Section 6, we further explore the mechanism based on structural

[22] If the labor force population falls below a certain threshold, employment-related data for the corresponding township are not allowed to be used for the analysis.

[23] Two points should be noted. First, the effect size appears substantial: a 10% increase in killing intensity leads to a 0.0044 increase in the share of agricultural employment, where the mean of the outcome variable is 0.013. However, this effect size should be interpreted with caution given the possibility of a nonlinear relationship. The share of agricultural employment is highly right-skewed, with 16% of observations at zero. Second, the mean of the outcome variable may underestimate the actual prevalence of agriculture. In rural Korea, typical family farms often count only household heads as employed in agriculture, while assisting family members are either excluded from employment statistics or classified under other categories. Furthermore, over 60% of rural wage workers are day laborers who shift to other sectors based on crop seasonality, further reducing their classification as agricultural workers (Kim et al., 2010). However, unless these measurement errors are correlated with killing intensity, they would not systematically bias our results.

transformation.

5.3 Robustness Checks

In Appendix B, we test the robustness of our findings in various contexts. First, we show that the results are not sensitive to the exclusion of outliers. A potential concern is that the estimates could be driven by the exceptionally high number of targeted killings in a small number of towns, particularly those located in the southwestern region far from the offensive line (Figure 3). To address this possibility, Table B.1 replicates the estimation using the long-run development outcomes after excluding the top-decile of towns with the highest number of targeted killings.

Proximity to Seoul might be another potential confounder. Given that the offensive line includes Seoul and nearby metropolitan areas, distance from this line may reflect underlying developmental differences or variations in economic spillovers associated with distance to Seoul. While our baseline specification already controls for distance to Seoul, Table B.2 further mitigates this concern by showing that our findings remain robust even after excluding all towns within 100 km of Seoul.

We also check the robustness to an extended sample composition. Our baseline sample excludes townships in the North, given little association between distance to the line and civilian killings in this region (Columns (5)-(6) in Table 1). However, this may raise concerns about selective sample composition. In this respect, Table B.3 presents the estimates using all available townships in South Korea, with townships in the East and North serving as controls. The results show minimal changes, confirming that our results are not affected by selective sample compositions.

Another plausible concern is that the offensive line closely follows the border between two regions: *North and South Jeolla* provinces in the West, and *North and South Gyeongsang* provinces in the East. Although quantitative evidence is limited, there is a widely held perception among Koreans that the Jeolla provinces experienced discrimination in economic policy during the industrialization period (Kim, 2012b). If these two regions indeed experienced differential policy interventions, our results could be biased. To alleviate this concern, we estimate the effect of civilian killings using only towns in the West (*Retreat* equals to one) and instrument civilian killings with distance to offensive line (*Dist*) instead of the interaction term (*Retreat* $\times$ *Dist*). Reassuringly, Table B.4 shows that our results remain robust, albeit with slightly smaller effect sizes.

Lastly, we assess the robustness of our findings to killings perpetrated by South Korean forces. As discussed in Section 2, the distribution and characteristics of these killings differed markedly from those committed by North Korean forces. In particular, many South Korean killings were not targeted at educated elites or their families. However, any potential correlation of South Korean killings with those of North Korean forces would raise concerns about the interpretation of our effects as capturing only the effects of targeted killings. To address this concern, Table B.5 presents estimates controlling for the intensity of civilian killings committed by South Korean forces, and the results remain unchanged.[24]

6 Mechanism

As discussed in Section 3, we suggest that the lasting negative impact of targeted killings resulted from a decline in human capital accumulation and its attendant effects on structural transformation. This section provides evidence in support of this channel by tracing changes in human capital and agricultural prevalence over time. Last, we discuss a range of alternative explanations.

6.1 Reduction in Human Capital Investments

6.1.1 Cross-Sectional Outcomes

To assess the short-run impact of civilian killings on human capital investment, Table 5 displays estimation results using school enrollment rates and the proportion of college graduates from the 1960 and 1966 censuses—the earliest post-Korean War census waves—as outcome variables. The 1960 census only reports school enrollment rate of children under 13, hence we augment this with data on school enrollment rates by different levels of education from the 1966 census. Column (1) considers the school enrollment rate of children aged 13 or younger from the 1960 census. The negative coefficient suggests that higher civilian casualties led to lower investment in children's education in the post-war period. In contrast, Column (2) shows that civilian killings had no immediate effect on college graduation rates (among individuals aged 25 and over). We interpret this as providing suggestive evidence that lower under-13 school enrollment rates are likely to reflect lower human capital investments rather than immediate, selective out-migration of highly educated parents.

Columns (3) and (4) report results using school enrollment rates in 1966. Targeted killings

[24]The data on killings perpetrated by South Korean forces relies on case reports investigated by Truth and Reconciliation Commission of Republic of Korea (2010). After compiling all relevant reports, we calculate the number of victims reported at the township level. However, these reports are limited to cases formally submitted for investigation, some of which remain ongoing. Thus, these data do not provide a complete account of all victims. Despite this limitation, the correlation between the number of victims attributed to North Korean forces in these reports and our baseline data is relatively strong (0.5).

appear to have little to no effects on primary schooling (Column (3)) but a large, negative and statistically significant effect on secondary school enrollment rates (Column (4)). The lack of an effect on primary school enrollment likely reflects near-universal enrollment rates resulting from compulsory primary education laws established in 1950.[25] Importantly, the coefficient in Column (4) suggests that targeted killings led to a fall in human capital accumulation for the broader township populace, not just those targeted by killings. A 10% increase in civilian killings per capita (on a mean of 2.12 killings per 1,000) led to a 3.1% decrease in secondary school enrollment rates.[26]

These results are suggestive of a direct negative demand-side effect on human capital accumulation for families of targeted individuals and an *indirect* effect on those that might have witnessed these 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. One plausible explanation is that these parents, having witnessed targeted killings of relatively educated people, might have been more hesitant to enroll their children beyond compulsory schooling due to concerns about potential repercussions—both real and perceived—in the event of future conflicts. The broader, long-term nature of the Korean Armistice signed in 1953 further reinforces this possibility: North and South Korea remain technically at war even up till today, with ideological and military tensions particularly severe until the 1970s (Agov, 2016).

It is natural to ask: given the long shadow of conflict on development (Riaño and Valencia Caicedo, 2024), do individuals in places that experienced higher civilian killings continue to invest less in education today? To examine this, we use the 2010 restricted census data to calculate township-level: (i) high school graduation rates; (ii) share of individuals with at least one year of college; (iii) college graduation rates. We do this for all adults older than or equal to 25 years of age at the time of the 2010 census.

Table 6 displays results. Columns (1)-(3) show that the share of individuals with higher education continues to be lower in 2010. Columns (1) and (3) show that an increase in civilian killings by 10% leads to a 4.5% and 9.3% decrease in high school and college graduation rates, respectively. Similarly, Column (2) shows that an increase in civilian killings by 10% leads to a 7.9% decrease in the share of individuals who have at least one year of college education. Taken together, the average level of human capital today appears to be lower in places that experienced higher killings nearly 6 decades ago.

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

[26] Alternatively, a 10% increase in killings leads to a 1.02 pp decrease in school enrollment on a sample mean of 36 pp.

6.1.2 Difference-in-Differences Analysis

Our results thus far confirm that targeted killings had significant effects on educational attainment both immediately after the Korean War and to the present day. Two crucial questions remain: (i) to what extent do the long-run effects on human capital capture lingering effects of immediate differences vis-a-vis contemporary differences in human capital investments (ii) whether effects might be driven by pre-existing trends in local educational conditions. We lack data on township-level measures of educational attainment prior to the Korean War; as an alternative, we conduct a pseudo-cohort difference-in-differences analysis using individual-level data from the 2010 census.

This analysis should be interpreted with caution. First, we are unable to test for pre-trends given the oldest cohorts still alive in 2010 would have been partially treated by the Korean War. Second, we do not observe an individual's town of birth. Instead, we proxy for town of birth using current town of residence in the 2010 census data. The 1930–1934 birth cohort serves as our reference period, as it is the earliest cohort consistently available across towns and the first to have made higher education decisions during or immediately after the Korean War. The data construction procedure is detailed in Appendix C.

Figure C.1 displays estimates using the cohort-level share of high school graduates (left panel) and college graduates (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. Negative effects peak for cohorts born in 1960-1964 and eventually become statistically significant for cohorts born in 1975-1980. This reversion can largely be attributed to the nation-wide expansion of the high school education system. High school enrollment rates in South Korea began rising rapidly in the 1970s and exceeded 95% by the 1990s (Korea Educational Development Institute, 2024), resulting in regional convergence.

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, we continue to observe a negative effect on the share of college graduates 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) who finds that average levels of human capital accumulation eventually recover in the decades following the first bombings in Laos.

6.1.3 Other Channels

The timing and magnitude of decreases in human capital accumulation suggest that "terror" effects from targeted killings could have led to a downward shift in demand for higher education. This section discusses alternative demand and supply-side factors and suggest that our findings are unlikely to be driven by these factors.

Other demand-side drivers Targeted killings might have led to immediate post-war declines in economic development and weaker labor market outcomes, which in turn could have led to lower school enrollment rates. These effects may have operated through greater resource constraints on human capital investment, both at the household level (budget limitations) and the community level (reduced local funding for education). Alternatively, lower economic development may have reduced the demand for skills, thereby lowering the returns to education.

Table 7 explores this possibility. Given the lack of direct measures of local development, we examine the impact of civilian killings on sectoral labor composition and occupational skill distribution. Columns (1) and (2) indicate that sectoral composition in 1960 is not significantly linked to the intensity of civilian killings. The share of employment in agriculture and manufacturing shows minimal association with civilian killings, suggesting that the observed reduction in school enrollment rates are unlikely to be a direct consequence of immediate declines in local economic development. Similarly, Columns (3) and (4) show little effect on the distribution of skilled and unskilled occupations within the non-agricultural sector, suggesting that the immediate effects of civilian killings on local development were not significant either within or across sectors.[27]

Alternatively, higher killings could have directly led to lower school enrollment rates through negative income shocks at the household level (Grasse, 2023). While we do not have data on household income, the relatively small number of killings per township in our estimation sample (average killings per township of 1.73 per 1,000 individuals; average township population of 24,846) suggests that direct income losses from deaths of household heads are unlikely to explain the entirety of the observed negative effect on human capital investments.

Supply-side drivers We have argued that the nature of targeted killings points to a key role of a reduced demand for human capital accumulation. An alternative explanation could

[27]The 1960 census categorizes non-agricultural occupations into eight groups. Among those, we classify experts, managers, and professionals as skilled occupations, while manual labor was classified as unskilled.

be that areas with higher targeted killings also experienced greater losses in the supply of education, such as a reduction in teachers or schools.

While data limitations prevent us from formally testing these supply-side explanations, the relatively small scale of civilian killings suggests that supply-side channels are unlikely to account for the observed declines in human capital accumulation. At the township level, the average number of killings was 1.73 per 1,000 individuals, and only 0.58% of victims had education sector-related occupations (e.g. professors or teachers). Educational infrastructure, such as schools, was also reported to have remained largely intact during the period of study, with no significant reports of widespread destruction or long-term closures that would disrupt the continuity of education (Appleman, 1961). Moreover, targeted killings were carried out by retreating North Korean forces that did not necessarily involve actual conflict episodes with UN or South Korean forces. Hence, the large-scale destruction of physical infrastructure under these circumstances were less likely (Truth and Reconciliation Commission of Republic of Korea, 2010).

In addition, the broader literature suggests that supply-side effects alone, in particular the destruction of physical infrastructure, are likely to attenuate over time (Davis and Weinstein, 2002; Miguel and Roland, 2011). Hence, supply-side explanations are unlikely to be sufficient to explain observed negative impacts on education that have persisted until today.

6.2 Declines in Structural Transformation

Our results support the negative effect of targeted killings on school enrollment rates starting from the early 1960s. In this section, we examine the sectoral outcomes in 1970 and 1980 to understand *when* the negative effects on structural transformation emerged and how they may be linked to decreases in human capital accumulation. Importantly, our hypothesis suggests that any negative effects on structural transformation are likely to emerge with a time lag, as the impact of reduced educational investment takes time to affect the labor market. Therefore, beyond examining cross-sectional relationships, analyzing the timing of these effects would be crucial for evaluating our proposed mechanism.

Ideally, a comprehensive examination of structural transformation during this period would require employment data similar to that available of the 1960s (Table 7). However, both the 1970 and 1980 Population Censuses do not record employment shares across sectors at the individual or township level. We instead rely on information from the 1970 and 1980 Agricultural Censuses, which cover over 75% of towns in our estimation sample.[28] While

[28]The dataset, officially titled Censuses of Agriculture, Forestry, and Fisheries of the Ministry of Agriculture and Forestry Statistics of Korea, is restricted-use and we are grateful to Hong et al. (2023) for generously sharing their data with us.

the Agricultural Censuses do not capture labor market outcomes within the non-agricultural sector, they allow us to investigate possible downstream effects on labor reallocation out of agriculture in rural areas—a key measure of structural transformation (Porzio et al., 2022).

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 effect of killings on the share of agricultural households in 1970 is small and statistically insignificant (Column (1)). This may reflect the fact that cohorts who were of secondary school age in the 1960s would have been too young to have married and established their own households by that time. Strikingly, however, we see a large and statistically significant effect of killings on the share of agricultural households in 1980 (Column (2)).

The timing of these effects have two key implications for our proposed mechanism. First, it suggests that lower school enrollments from targeted killings was likely a key channel behind negative effects on long-run structural transformation. Lower educational attainment possibly slowed down the reallocation of labor out of the agricultural sector, leading to persistently negative effects on contemporary development outcomes. Second, the fact that negative effects emerge only in 1980, rather than earlier, partially alleviates concerns about the potential role of other immediate post-Korean War shocks in explaining long-run outcomes.[29] Specifically, there is no clear reason why these alternative mechanisms would affect sectoral labor allocations in 1980 but not in 1970.

While our findings suggest that killings led to a lag in structural transformation, a slowdown in structural transformation might not necessarily lead to lower (rural) development if accompanied by improvements in agricultural productivity. This appears unlikely given the negative effects on long-run development measures discussed in Section 5. In addition, Table 9 examines the effects of civilian killings on three agricultural outcomes potentially associated with farm productivity: average farm size, farm size inequality, and the number of cultivators per acre, as measured from the 1980 Agricultural Census data. The coefficients for all three measures are close to zero.

The lack of differences in agricultural productivity suggest that (i) the decline in structural transformation was not coincident with productivity improvements in the agricultural sector and (ii) there were little differences in the extent of post-Korean War targeting or rollout of agricultural improvement policies. Specifically, the Korean government implemented the New Village Movement in the 1970s to improve agricultural production in rural areas. The lack of differences in agricultural productivity in 1980 suggest that these policies were

[29] Regional variations in land reform (largely completed during the Korean War) or destruction of agricultural production inputs could be examples.

not targeted towards reconstruction in areas that experienced a higher intensity of killings.[30] We further discuss this and other possibilities in the next section.

6.3 Alternative Explanations

Concerns may arise regarding alternative explanations for the negative effects of civilian killings. To address this, we examine leading alternative explanations that could account for our findings, including the direct loss of local elites, selective migration, and decline in trust.

Local elites The killing of local elites may have weakened the state capacity of affected regions to facilitate the top-down allocation of public goods towards their townships. Specifically, the Korean government introduced two major policy initiatives during the 1970s: the agricultural New Village Movement (NVM) and 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).

Studies suggest that implementation of these policies varied across regions (Kim et al., 2021; Hong et al., 2023). If policy implementation was associated with elite killings, we would expect to see an association between killing intensity and the allocation of these policies across townships. Table A.3 examines the relationship between civilian killings and the amount of per capita NVM transfers received by towns. The results indicate little variation in relation to killings. We cannot apply the same approach to the HCIDP as we do not have data on HCIDP budget expenditures. Instead, Table A.4 tests and shows that key results on long-term outcomes are robust to controlling for the location of industrial complexes built during the HCIDP period.

Selective migration If targeted killings led to selective migration based on exposure to violence, they could influence local development outcomes independently of the proposed mechanism. Although our results on short-run demographic composition suggests that spatial sorting of individuals is unlikely to account for observed short-run effects on human capital, selective migration could still reinforce the long-term impact of civilian killings. Unfortunately, we cannot extend this analysis across periods, as census data from 1970 to the

[30] Specifically, Appendix Table A.3 shows that areas with a higher intensity of civilian killings did not receive more transfers from the New Village Movement.

early 2000s do not include occupational and educational outcomes. Therefore, our analysis focuses on the share of the prime-age population (ages 25–54) as an indirect proxy for selective migration. In Korea, rural-to-urban migration has been highly age-selective, with working-age individuals more likely to move to urban areas for better economic opportunities (Kim, 1982; Lee and Kim, 2020). Given that our sample primarily consists of rural towns, examining the relationship between civilian killings and the prime-age population ratio over time may offer insights into the role of selective migration.

Table A.5 presents the results. Using the ratio of prime working-age individuals (ages 25–54) to the total population as the outcome variable, we estimate its cross-sectional relationship with civilian killings separately across five waves of 10-year intervals, based on available population census data from 1966 to 2005. Columns (1) to (3) show a negligible effect of civilian killings on the prime-age population ratio from 1966 to 1985. However, the effects turn negative in 1995 (Column (4)) and become both large and statistically significant in 2005 (Column (5)). These findings suggest that while selective migration is unlikely to have driven short- and medium-run effects on human capital investment and structural transformation, it could have amplified their persistent implications for local development outcomes.

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 generalized trust towards society and the state may also explain negative long-run development.

To investigate the long-term relationship between civilian killings and trust, we pool data from the Korean General Social Survey (KGSS), an annual survey that provides measures of economic and political attitudes (2003-2019) (Kim et al., 2019).[31] The earliest KGSS wave was conducted in 2003, preventing us from studying the short-term effects on trust as a potential mechanism. Nonetheless we view these results as informative about the potential role for differences in trust in driving our observed results. We estimate effects on trust in the leadership of the armed forces; society; and people in general, using the following individual-level regression:[32]

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

[32] Specifically, in Column (1), the question we use is "The armed forces is a core social institution. How much do you trust the people that lead these institutions?" Column (2): "How trustworthy is our society in your opinion?"; Column (3): "In general, do you think people can be trusted, or should you exercise caution?" Answers range from 0 to 10.

$$Trust_{ik} = \alpha + \beta CivKillings_k + \mu_G' X_k^G + \mu_S' X_k^S + \mu_I' X_k^I + \delta_p + \delta_w + \epsilon_k \quad (3)$$

$Trust_{ik}$ is a measure of trust for respondent i residing in town k. We control for predetermined town-level characteristics (X_k^G and X_k^S). We also control for individual-level characteristics (X^I), including age, age squared, and indicators for gender and urban/rural residence. δ_p and δ_w each denote province and survey-wave fixed effects, respectively. Standard errors are adjusted following Conley (1999) using a distance cutoff of 30km.

Table A.6 presents the results. For ease of interpretation, outcome variables are normalized to range from 0 to 1. We find a negative and statistically significant effect on trust in army (Column 1) but the effect size is quantitatively small. Specifically, the estimated coefficient suggests that a 10% increase in civilian killings leads to a 0.005 unit (0.014 s.d.) decrease in trust in the armed forces, measured on a normalized 0-to-1 scale, suggesting that trust alone is unlikely to be the main driver behind our results on negative long-run development.[33] Moreover, the relationship with generalized trust (Columns 2 and 3) — a potentially more relevant factor for economic development — is statistically insignificant.[34] However, given that our trust outcomes are measured after decades of persistently lower medium-run economic development, lower generalized trust could possibly be an outcome of lower development and might not necessarily capture the long-run effects of killings per se.

The lack of significant effects of targeted killings on trust contrasts sharply with previous studies. One possible explanation is that killings in our setting were perpetrated by foreign, North Korean forces and individuals in affected townships have not had any interactions with these forces for the past 70 years. This contrasts, for example, to the killings in the Rwandan Genocide (Blouin and Mukand, 2019), where the perpetrators originated within the community and continued to live alongside the victims.

7 Conclusion

This study provides novel evidence on the effects of civilian wartime killings on local development. To the best of our knowledge, we construct the first comprehensive dataset on

[33] These results are consistent with at least two possibilities. First, killings perpetuated by armed forces during the Korean War might have led to a broader, persistently negative perception of the military. Second, targeted killings might have led to a specific distrust towards the leadership of the South Korean army given their failure to protect civilians from North Korean atrocities. The lack of more specific survey questions prevents us from more thoroughly examining these nuanced aspects. In either case, the magnitudes on trust in Column 1 are likely too small to explain observed differences in long-run development.

[34] The point estimate for trust in people is somewhat larger but magnitude-wise is likely to be of limited economic significance given a 0.019 unit or 0.038 s.d. decrease compared to a 0.38 s.d. decrease in Tur-Prats and Valencia Caicedo (2020).

civilian killings during the Korean War and provide quantitative evidence that these events contributed to persistent regional disparities in development across South Korea. Notably, these negative effects have persisted for over 70 years, despite the relatively small scale of civilian deaths in the context we examine 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 and a subsequent slowdown in structural transformation. Our evidence supports this proposition, showing that greater exposure to civilian killings led to lower investment in child education and a relative decline in structural transformation. These findings suggest a new perspective on the enduring impact of conflicts. Beyond large-scale capital destruction or mass casualties, even localized, small-scale violence can leave a profound and lasting imprint on economic development by hindering human capital accumulation.

More broadly, this paper sheds light on how conflict can lead to lasting divergences in local development trajectories. Despite South Korea being considered a growth miracle, our findings show that historical shocks can create persistent intra-country disparities in development, highlighting how conflict leaves a deep mark even in rapidly industrializing economies. These insights contribute to wider discussions on the uneven distribution of growth's benefits and the roots of regional inequality. In future work, we aim to explore the long-term effects of such events on political behavior and the political economy of local development.

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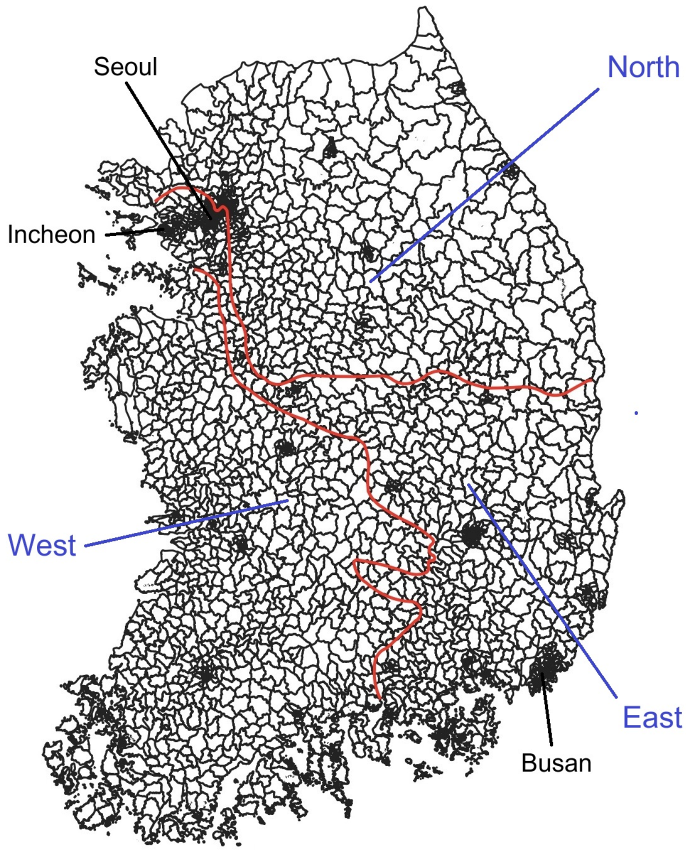
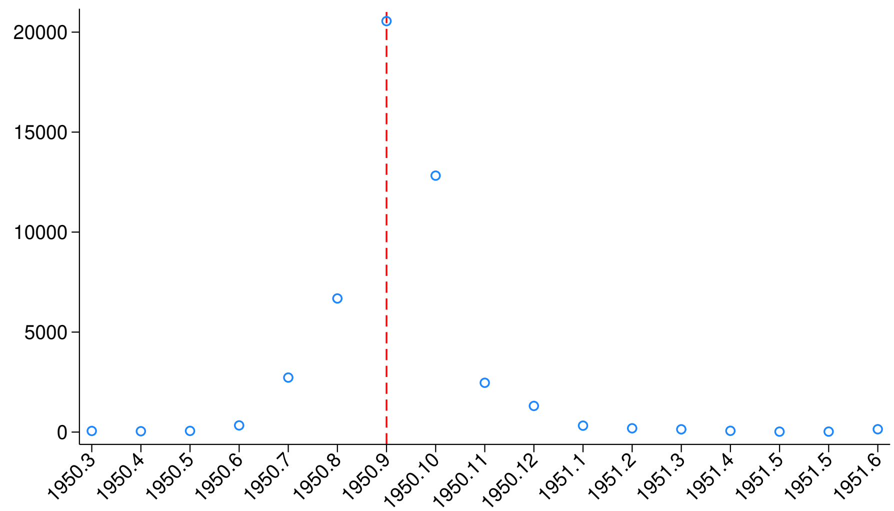
Figure 1: Map of United Nations offensive line following Incheon Landing



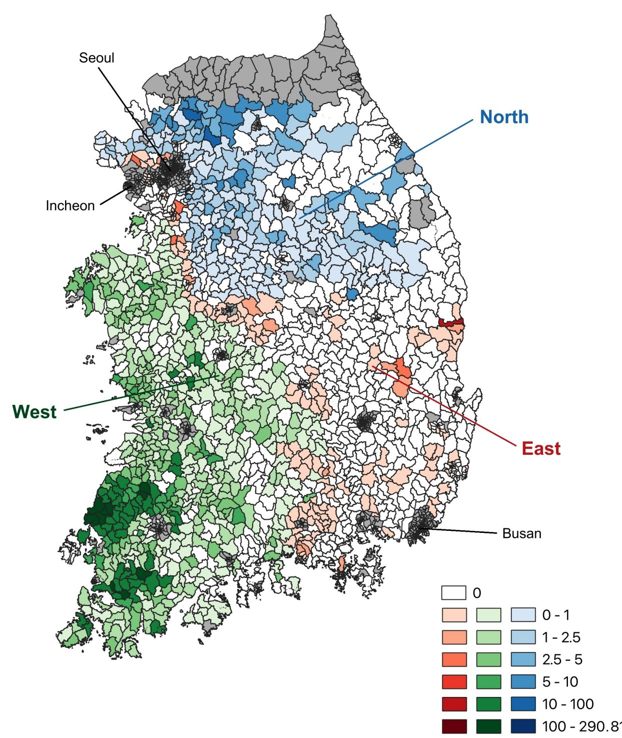
Note: The map shows the United Nations offensive line formed following the Incheon Landing and subsequent military operations between September 15 and September 26.
Source: The United States Military Academy Department of History

Figure 2: Trends in civilian killings during the Korean War



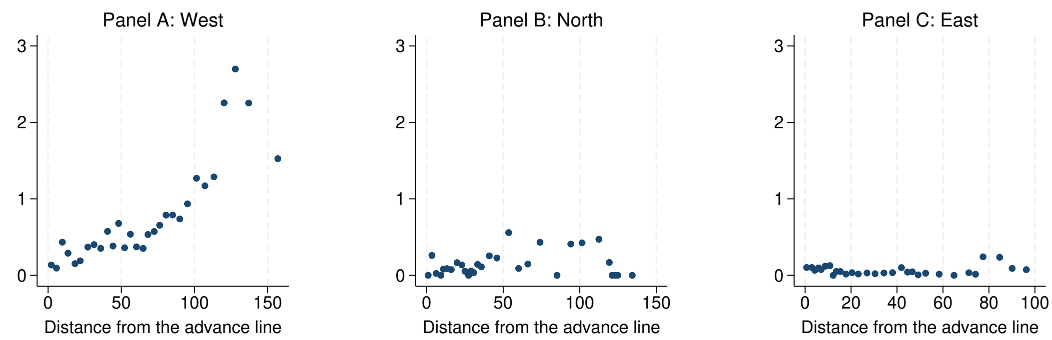
Note: This figure shows the number of civilian killings committed by North Korean forces by month. The red line indicates the month in which the Incheon Landing Operation was conducted

Figure 3: Spatial distribution of civilian killings



Note: This figure shows the number of civilian killings per 1,000 population. The mean number of killings in our entire dataset is approximately 2.1 per 1,000 population (21.2 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



Note: The X-axis shows the nearest distance from the town centroid to the offensive line in kilometers.

Table 1: First Stage Regression

Dependent Variable:	Log Number of Civilian Killings per 1,000 Population					
	All		South		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 the log number of civilian kilings per 1,000. Columns (1) - (2) include all towns. Columns (3) and (4) include towns located within and below the offensive line, and Columns (5) and (6) include towns located within and north of 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 regressions control for the number of civilian killings that occurred before the formation of the offensive line and an indicator variables that equals 1 for townships that lie on 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 Civilian Killings	-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
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 in 1955. The dependent variable in Column (2) is the ratio of persons aged 14 and under by town in the same year. 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 Density

Dependent Variable:	Log Average Night Light Density				
	1995	2000	2005	2010	2015
	(1)	(2)	(3)	(4)	(5)
Log Civilian Killings	-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
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) – (6) is the log of night light density per square kilometre, averaged across all grid-cells that lie within each township boundary. 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 Long-Run Sectoral Development (2020)

Panel A: Manufacturing			
Dependent Variables:	Manufacturing Employment Share	Log Value Added per Worker	Log Average Salary per Worker
	(1)	(2)	(3)
Log Civilian Killings	-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
Panel B: Agriculture			
Dependent Variables:	Agricultural Employment Share	Log Average Farm Size	Log Number of Tillers per Acre
	(1)	(2)	(3)
Log Civilian Killings	0.046**	0.735***	-0.039
	(0.024)	(0.204)	(0.325)
Observations	882	1103	1103
Mean	0.01	-	-
Kleibergen–Paap F–Stat	21.81	62.44	62.44
Geoclimatic Controls	Yes	Yes	Yes
Socioeconomic Controls	Yes	Yes	Yes
Province Fixed Effects	Yes	Yes	Yes

For Panel A, the dependent variable in column (1) is the percentage employed in manufacturing in the year 2020. The dependent variable in columns (2) and (3) is the log value added per worker and log average salary per worker, respectively. For Panel B, the dependent variable in column (1) is the percentage employed in agriculture in the year 2020. The dependent variable in columns (2) is the log average farm size, and the dependent variable in column (3) is the log number of tillers per acre of farmland. Notes: Conley standard errors with a distance cutoff of 30km are shown in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

Table 5: Civilian Killings and Enrollment (1960–1966)

Dependent Variable:	Enrollment Rate Under 13 (1960)	Proportion of College Graduates (1960)	Enrollment Rate Primary (1966)	Enrollment Rate Secondary (1966)	Enrollment Rate Tertiary (1966)
	(1)	(2)	(3)	(4)	(5)
Log Civilian Killings	-0.036*	0.006	0.006	-0.107*	0.007
	(0.021)	(0.009)	(0.007)	(0.059)	(0.013)
Observations	1031	1143	1143	1143	1143
Mean	0.30	0.02	0.62	0.36	0.03
Kleibergen–Paap F–Stat	44.89	53.41	53.41	53.41	53.41
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 under 13 by township in the year 1960. The dependent variable in Column (2) is the proportion of college graduates for all persons aged 25 and over in the year 1960. The dependent variables in Columns (3)–(5) are primary, secondary, and tertiary school enrollment rates for the relevant 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 Human Capital (2010)

Dependent Variable:	Share of Graduates		
	High School	Some College	College
	(1)	(2)	(3)
Log Civilian Killings	-0.254**	-0.191**	-0.138**
	(0.108)	(0.085)	(0.069)
Observations	1182	1182	1182
Mean	0.55	0.24	0.15
Kleibergen–Paap F–Stat	51.83	51.83	51.83
Geoclimatic Controls	Yes	Yes	Yes
Socioeconomic Controls	Yes	Yes	Yes
Province Fixed Effects	Yes	Yes	Yes

Notes: The dependent variables in column (1) - (3) are the share of graduates in the population depending on the highest level of educational attainment. Educational attainment is differentiated into high school, some college, and college. 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 Labor Market Outcomes (1960)

Dependent Variables:	Share of Employment			
	By Sector		Within Non–Agricultural	
	Agriculture	Manufacturing	Skilled	Unskilled
	(1)	(2)	(3)	(4)
Log Civilian Killings	-0.053	0.035	-0.002	0.020
	(0.050)	(0.024)	(0.029)	(0.054)
Observations	1031	1031	1031	1031
Mean	0.79	0.05	0.15	0.28
Kleibergen–Paap F–Stat	44.89	44.89	44.89	44.89
Geoclimatic Controls	Yes	Yes	Yes	Yes
Socioeconomic Controls	Yes	Yes	Yes	Yes
Province Fixed Effects	Yes	Yes	Yes	Yes

Notes: The dependent variables in Columns (1) and (2) are the share employed in the agricultural and manufacturing sectors, respectively. The dependent variables in Columns (3) and (4) are the share employed in skilled and unskilled occupations within the non-agricultural sector. Conley standard errors with a distance cutoff of 30km are shown in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

Table 8: Civilian Killings and Agricultural Households (1970–1980)

Dependent Variable:	Share of Agricultural Households	
	1970	1980
	(1)	(2)
Log Civilian Killings	-0.154	0.231***
	(0.107)	(0.083)
Observations	924	922
Mean	0.57	0.62
Kleibergen–Paap F–Stat	34.65	33.85
Geoclimatic Controls	Yes	Yes
Socioeconomic Controls	Yes	Yes
Province Fixed Effects	Yes	Yes

Notes: The dependent variable is the share of full-time agricultural households. Column (1) uses data from the 1970 agricultural census, while Column (2) uses data for towns in 1980. Conley standard errors with a distance cutoff of 30km are shown in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

Table 9: Civilian Killings and Agricultural Productivity (1980)

Dependent Variables:	Log Average Farm Size (1)	Farm Size Inequality (2)	Log Number of Cultivators Per Acre (3)
Log Civilian Killings	0.043	-0.016	0.041
	(0.122)	(0.021)	(0.045)
Observations	922	922	922
Kleibergen–Paap F–Stat	33.85	33.85	33.85
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 average farm size, measured in hectares. The dependent variable in Column (2) is the degree of farm size inequality calculated using the Gini index, the mean value of which is 0.35. The dependent variable in Column (3) is the log number of cultivators per acre of farmland. All variables are from the year 1980. Conley standard errors with a distance cutoff of 30km are shown in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

Appendices

A Additional Tables and Figures

Table A.1: Balance Check Results

Dependent Variables:	Caloric Suitability	Elevation	Slope	Ruggedness	Latitude	Distance to Coast
Treated × 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)
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**	**Population Density**	**Literacy**	**Share of Agricul-ture**	**Share of Manufac-turing**
Treated × 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)
Observations	1347	1347	1347	1315	1207	1207
R–Squared	0.01	0.19	0.01	0.05	0.03	0.02

Conley standard errors with a distance cutoff of 30km are shown in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A.2: Reduced Form Estimates

Dependent Variables:	Log Night Light Density 2015	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	0.56	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 density in 2015. 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 A.3: Civilian Killings and NVM Transfers Per Person

Dependent Variable:	Log NVM Transfer Amount Per Person			
	1974	1975	1977	1978
	(1)	(2)	(3)	(4)
Log Civilian Killings	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 from the New Village Movement agricultural policy. Conley standard errors with a distance cutoff of 30km are shown in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

Table A.4: Robustness in Long–run Development Outcomes: Industrial Policy

Dependent Variables:	Log Night Light Density 2015	Manufacturing Employment Share	Log Value Added per Worker	Log Average Salary per Worker
	(1)	(2)	(3)	(4)
Log Civilian Killings	-0.840**	-0.027	-1.571**	-0.294**
	(0.427)	(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 density in 2015. 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 A.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 Civilian Killings	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

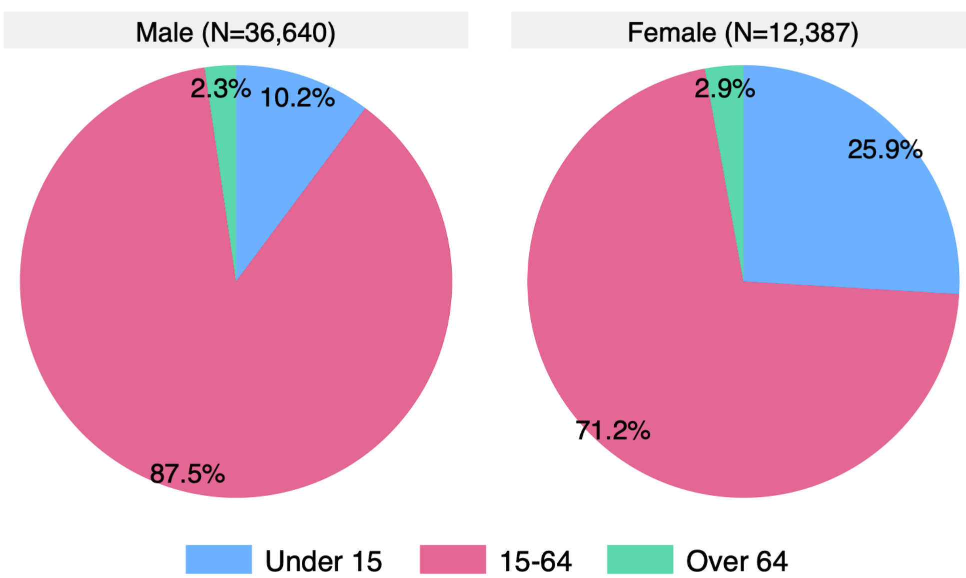
Notes: Conley standard errors with a distance cutoff of 30km are shown in parentheses. Prime–Age individuals are defined as those aged 25–54. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

Table A.6: Civilian Killings and Trust

Dependent Variables:	Trust in		
	Army	Society	People
	(1)	(2)	(3)
Log Civilian Killings	-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 outcome variables are standardized to range from 0 to 1, and are based on responses to KGSS surveys conducted between 2003 and 2019. Observations are weighted by individual survey weights. Conley standard errors with a distance cutoff of 30km are shown in parentheses. $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Figure A.1: Gender and Age group of Civilian Victims



Note: 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).

B Robustness Checks

Table B.1: Robustness in Long–run Development Outcomes: Extreme Values

Dependent Variables:	Log Night Light Density 2015	Manufacturing Employment Share	Log Value Added per Worker	Log Average Salary per Worker
	(1)	(2)	(3)	(4)
Log Civilian Killings	-2.987*	-0.207	-6.443**	-1.315**
	(1.620)	(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 density in 2015. 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.2: Robustness in Long–run Development Outcomes: Distant from Seoul

Dependent Variables:	Log Night Light Density 2015	Manufacturing Employment Share	Log Value Added per Worker	Log Average Salary per Worker
	(1)	(2)	(3)	(4)
Log Civilian Killings	-0.782*	0.059	-1.530**	-0.386***
	(0.412)	(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 density in 2015. 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.3: Robustness in Long–run Development Outcomes: All Areas

Dependent Variables:	Log Night Light Density 2015	Manufacturing Employment Share	Log Value Added per Worker	Log Average Salary per Worker
	(1)	(2)	(3)	(4)
Log Civilian Killings	-2.031***	-0.682	-3.966***	-0.924***
	(0.616)	(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 density in 2015. 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.4: Robustness in Long–run Development Outcomes: Southern Areas

Dependent Variables:	Log Night Light Density 2015	Manufacturing Employment Share	Log Value Added per Worker	Log Average Salary per Worker
	(1)	(2)	(3)	(4)
Log Civilian Killings	-0.362**	-0.094**	-0.771***	-0.099*
	(0.152)	(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 density in 2015. 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.5: Robustness in Long–run Development Outcomes: South Korean Killings

Dependent Variables:	Log Night Light Density 2015	Manufacturing Employment Share	Log Value Added per Worker	Log Average Salary per Worker
	(1)	(2)	(3)	(4)
Log Civilian Killings	-0.947**	-0.031	-1.934***	-0.407***
	(0.408)	(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 density in 2015. 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 the log number of killings by South Korean forces per 1,000 in our controls. Conley standard errors with a distance cutoff of 30km are shown in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

C Difference-in-Differences: Cohort-Level Analysis

This section describes our pseudo-cohort difference-in-difference analysis in Section 6. To complement our main findings, we conduct a cohort-level difference-in-differences analysis using data from the 2010 population census. This approach 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. Empirically, we use the following specification:

$$\text{Education}_{kc} = \alpha + \beta_c \text{ Cohort}_c \times \log(CivilianKillings_k) + \gamma_c + \tau_k + \epsilon_{kc} \tag{4}$$

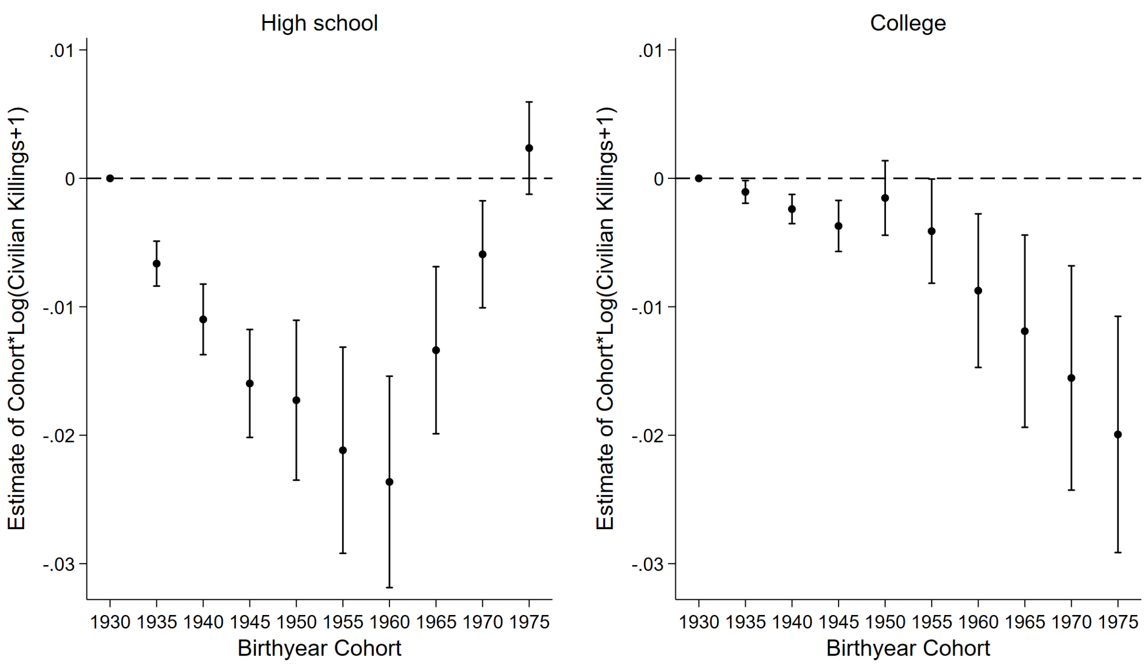
For birthyear cohort c and township k, Education_{kc} denotes the two outcome variables: the share of high school graduates or higher and the share of the population with an associate degree or higher. Cohort_c is an indicator for five-year birthyear cohorts, ranging from 1930-1934 to 1975-1979. γ_k and τ_c denote township and birthyear-cohort fixed effects, respectively. Standard errors are clustered at the township level.

There are several limitations to using the population census data in our analysis. First, the data provide information only on current residence, not birthplace. Thus, our findings based on the town of residence may reflect migration patterns that may or may not be related to human capital or local development. Second, some individuals, particularly from older cohorts, may have died and are missing from our data, raising concerns about selective attrition. Accordingly, we do not interpret our parameter of interest, β_c, as the causal effect of civilian killings on educational attainment. Instead, we view it as suggestive correlation indicating whether individuals in townships with more intense civilian killings were more or less likely to have higher education.

Figure C.1 presents the results. The left figure displays the estimates based on the share of high school graduates or above, while the right figure shows the estimates using the share of the population with an associate degree (equivalent to two years of college) or higher. For high school graduates, compared to the 1930–1934 cohort, individuals born between 1935 and 1964 were less likely to have obtained a high school education if they currently reside in a township with a higher intensity of civilian killings. 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 pre-existing local characteristics. While this trend began to reverse around 1960, it 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 were catching up with their counterparts. However, as shown in the figure on the right, the results for college education do not exhibit any reversal in the downward trend.

Figure C.1: DID Estimates of Civilian Killings and Education



Note: This figure presents coefficient estimates of the interaction between birthyear cohort indicators and log of civilian killings 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.