

Essays on the Legacy of Conflicts and Demographic Policies: Inequality Across Generations

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Declaration

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Statement of Contribution of Co-Authors

Chapters 2 to 4 consist of papers submitted for publication and under editorial review or working papers during the period of candidature. Chapter 2 is co-authored with my supervisors Dr Jonathan Norris and Dr Otto Lenhart. Chapter 3 is co-authored with my PhD colleague Zhan Shi. Both Chapter 2 and Chapter 3 have been released as working papers and were previously under peer review. The undersigned hereby certifies that:

1. they meet the criteria for co-authorship in that they have participated in the conception, execution, or interpretation, of at least that part of the working paper in their field or expertise;
2. they take public responsibility for their part of the publication, except for the responsible author who accepts overall responsibility for the working paper;
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In the case of Chapter 2, contributions to the work involved the following:

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Abstract

This thesis examines the long-term consequences of conflicts and state demographic policies, focusing on how such large-scale shocks shape human development, well-being, and inequality across generations. Leveraging both historical and contemporary data, it demonstrates that the effects of conflicts and policy interventions extend far beyond their immediate effects, influencing individuals, families, and communities in the long term. This thesis comprises three interconnected essays, each providing empirical evidence on how external shocks impact long-term outcomes in health, development, and inequality.

In the first essay (Chapter 2), I collaborated with my supervisors to investigate how past exposure to U.S. bombing shapes local development trajectories in Cambodia decades later. Drawing on extensive geo-coded datasets and a spatial regression discontinuity design adapted to multiple boundaries, we show that conflict generates distinct mechanisms depending on bomb detonation failure: a persistent *risk* channel when unexploded ordnance (UXO) remains, and a recovery *trigger* channel when bombs detonate, and no lasting hazard is left behind. Exploiting soil conditions that affect bomb detonation but are orthogonal to bombing locations, we find that in Cambodia, more than three decades after the bombing, soft-soil areas where bombs were more likely to fail experience higher UXO incidents, worse health outcomes, and weaker economic performance. In contrast, hard-soil areas where bombs were more likely to detonate face lower UXO risks and exhibit better health with stronger economic development. The legacy of conflict is thus shaped by two distinct channels: destruction that can open space for recovery, and enduring threats that impede long-term development.

In the second essay (Chapter 3), I joined with a fellow PhD student to examine the intergenerational spillover effects of China’s One-Child Policy on health. Using data from the China Family Panel Studies and a reduced-form regression discontinuity design, we estimate the local average treatment effect of the policy on urban Han Chinese. Our results show that children of policy-exposed mothers experience significant improvements in both physical and mental health, driven by improved maternal health and socioeconomic conditions, greater investment in child health, and better parenting practices. This chapter contributes to the literature on intergenerational health transmission and the quantity–quality trade-off, highlighting how family planning policies can generate long-lasting effects across generations.

The third essay (Chapter 4) examines how violence influences gender inequality, particularly female empowerment and domestic violence, using evidence from the Khmer Rouge Genocide in Cambodia. I draw on various geo-coded datasets and a spatial regression discontinuity design to demonstrate that the genocide has significant lasting negative effects on female empowerment and domestic violence. In high-repression areas, women today are less empowered and more vulnerable to domestic violence, especially emotional violence. I provide evidence for several mechanisms that possibly explain these findings, including skewed sex ratios and higher fertility rates following the genocide, lower household wealth, and significant changes in gender-related characteristics such as younger marriage age and less education. Women are more likely to work, though mainly in part-time rather than full-time positions. Men are more likely to have multiple marriages. The chapter shows that mass violence not only harms societies in the short run but also creates lasting barriers that hold back women’s empowerment for generations.

Overall, this thesis contributes to a growing body of literature on the long-term consequences of conflicts and demographic policies. It provides new empirical evidence on how large-scale shocks reverberate through health, well-being, and inequality, thereby illustrating the profound and lasting impact of historical disruptions on societies.

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Chapter 1

Introduction

This thesis examines the enduring effects of violence and demographic interventions, contributing to a growing literature on the long-term and intergenerational effects of historical shocks. It demonstrates that such events not only disrupt lives in the short run but also leave persistent marks on health, economic outcomes, and social inequality. By analyzing different contexts, the essays collectively highlight how the legacies of conflict and population policies continue to shape opportunities and constraints across generations, thereby deepening our understanding of the mechanisms through which inequality is reproduced over time.

Chapter 2 is titled “*The Aerial Bombing of Cambodia and the Recovery of Communities*”. This chapter aims to answer the research question: how does the legacy of past conflict influence long-term recovery of communities? To address this question, we draw on the U.S. bombing campaign in Cambodia between 1965 and 1975. Cambodia is recognized as one of the most heavily bombed countries in history, with more than 2.7 million tons of munitions dropped in the country during this period, far exceeding the 2 million tons deployed by the Allies in World War II (Owen and Kiernan, 2006). This extensive bombing has resulted in widespread destruction and long-term contamination from unexploded ordnance (UXO).

We examine one possible explanation by considering whether conflict leaves destruction that creates an open door for later recovery, or instead leaves lingering UXO risks that continue to depress economic activity. Conceptually, we argue that the effects of

conflict operate through two channels: a *trigger* channel that facilitates recovery and a *risk* channel that leaves persistent hazards, depending on whether bombs detonate or fail to detonate.

To empirically test these mechanisms, we compile and integrate geo-coded information from diverse data sources. We first use spatial data on 1965-1975 US airstrike missions in Cambodia to identify areas profoundly impacted by the bombings, which we call “bombing areas”, based on the grid-based clustering technique – a common approach for identifying point clustering (Ankerst et al., 1999). By leveraging pre-bombing soil conditions that are orthogonal to bombing locations but affect detonation rates, we are able to separate the short-run destruction effects of bombing from the long-run hazards posed by UXO contamination.

We use a spatial regression discontinuity design (RDD), following Bühler (2023), Dell (2010), and Dell et al. (2018), to address potential selection bias in bombing locations and identify the long-term effects of bombing. We show evidence that our design satisfies core RDD assumptions. Crucially, we also show that soil softness is unrelated to bombing locations and a range of bombing measures, allowing us to further separate the analysis into areas with soft soil (low UXO risk) and hard soil (high UXO risk).

We begin by examining how the likelihood and intensity of UXO incidents vary across bombed communities with soft versus hard soil. We link village locations from the 2008 Cambodia Population Census to geocoded information on UXO incidents. We find that villages in bombed soft-soil areas are about 5 percentage points more likely to have UXO incidents and the number of incidents is more than 50% higher relative to the mean, consistent with Lin (2022)’s observations. Soft-soil areas remain affected by lingering hazards from past bombings, whereas hard-soil areas face fewer persistent risks and are therefore safer for development.

Building on these findings regarding UXO risk, we next examine community recovery by focusing on health outcomes, which directly shape well-being and indirectly influence productivity. We use multiple individual-level health measures from four waves of the Cambodian Demographic and Health Surveys (DHS) and combine them

into an aggregated *Health Index*. Our results show a divergent pattern in health between soft-soil (high UXO) and hard-soil (low UXO) locations. In soft soil areas with a higher risk of UXO, for those living on the bombing side, we observe a deterioration in *health index* of about 8% of a standard deviation. Meanwhile, individuals living in bombed hard soil areas exhibit better health outcomes with *health index* increasing by approximately 6% of a standard deviation. We further conduct extensive sensitivity checks to show the robustness of these results.

We interpret health outcomes as indicators of community well-being and then examine community economic development to shed light on these patterns. Using the 2008 Cambodia Population Census merged with multiple infrastructure datasets, we construct an *Economic Development Index* at the village level, which captures different dimensions of local economic development and living standards across communities. Consistent with our findings on health, we observe weaker economic development in soft-soil areas and significantly stronger economic development in hard-soil areas. Economic development declines by approximately 12% of a standard deviation in soft-soil areas, while increasing by about 5% of a standard deviation in hard-soil areas. These results support the presence of a UXO-related *risk* channel in soft soils and a post-bombing recovery (*trigger*) channel in hard soils.

Finally, to further examine these community-level development patterns, we draw on household data from the Cambodian Socio-Economic Survey (CSES) to analyze households' economic conditions, agricultural productivity, and risk-taking behavior. Households in bombed hard-soil areas have higher incomes, higher value of durable assets, and better food security, while those in bombed soft-soil areas fare worse across these measures. In addition, households in soft-soil locations exhibit lower agricultural productivity and are less likely to take on new risks, such as borrowing.

Overall, the chapter contributes to the extensive literature on how conflicts and explosive ordnance shape activity in the long term. We offer empirical evidence of a *risk* channel through which conflict leaves persistent hazards that continue to undermine health and economic development long after violence has ended, aligning with prior findings in Guo (2020); Lin (2022); Nguyen et al. (2024); Riano and Valencia Caicedo

(2024). Concurrently, we identify a *trigger* channel through which regions no longer burdened by lingering hazards can seize opportunities for recovery, thereby improving health and economic outcomes. This finding highlights the potential for recovery and development once war remnants are removed, underscores the importance of demining and clearance initiatives in facilitating economic revitalisation.

Chapter 3 is titled “*The One-Child Policy in China and its Intergenerational Effects on Health*”. In this chapter, we investigate the long-term spillover effects of China’s One-Child Policy on the health outcomes of subsequent generations. The One-Child Policy was a family planning policy implemented nationwide in China from 1979 to 2016, restricting most families to a single child to slow rapid population growth. Prior research has shown that smaller family sizes lead to greater parental investment in children, thereby improving education and health outcomes (e.g. Cáceres-Delpiano (2006); Lee (2008); Rosenzweig and Zhang (2009); Zhong (2017)). These benefits can also extend across generations, as improvements in parental health can be transmitted to their offspring (Emanuel et al., 1992; Eriksson et al., 2005; Strauss and Thomas, 2007).

Our analysis draws on data from the China Family Panel Studies (CFPS), a nationally representative biennial longitudinal survey funded by the Chinese government and conducted by the Institute of Social Science Survey at Peking University since 2010. We use six survey waves up to 2020, which provide rich information on demographic, economic, and health outcomes. We restrict our sample to urban Han Chinese, who were strictly subject to the One-Child Policy. We follow Anderson (2008) to construct an aggregated *health index* comprising the following components: absence of illness, no hospitalization, self-rated health, and interviewer-rated health. In addition, we develop a separate mental health indicator, *distress*, based on the Kessler Psychological Distress Scale (K6) and the Center for Epidemiologic Studies Depression Scale (CES-D). We use a reduced-form regression discontinuity design (RDD) that leverages the 1980 policy cut-off, which generated a discontinuity in the prevalence of single-child families. This design enables us to estimate the local average treatment effect (LATE) of the policy on the health outcomes of the next generation. We further demonstrate its va-

lidity through multiple checks around core RDD assumptions. Given the substantially smaller sample size for father data and the stronger evidence that urban Han mothers benefited from the policy (Fong, 2002; Veeck et al., 2003; Zhang, 2019), our analysis centers mainly on maternal exposure to the policy.

We first assess the policy’s effects on the first generation – mothers. Our findings indicate that the policy significantly changes their sibling composition, improves health outcomes, and strengthens socioeconomic status in adulthood, in line with long-term gains in human capital and economic performance among those affected (Hu and Yang, 2025; Zhang, 2019).

We then examine the policy’s intergenerational health effects on the second generation – children. We find that children born to mothers affected by the policy exhibit significant improvements in both physical and mental health. Children of policy-affected mothers have their *health index* improved by about 15% of a standard deviation and exhibit substantially lower levels of psychological distress, with a 9.3 percentage point decline in the likelihood of distress, representing over 50% of the mean. Our results thus provide evidence for the intergenerational transmission of policy effects, demonstrating how changes in family structure and maternal outcomes can shape children’s health and well-being across generations. Lower fertility, transmitted across generations from grandparents to parents, allows for greater human capital investment per child (Kolk, 2014; Murphy and Knudsen, 2002). Moreover, the improved health and socioeconomic outcomes observed among policy-affected mothers can be carried forward to the next generation, generating positive effects on children’s health (Emanuel et al., 1992; Eriksson et al., 2005).

We then empirically investigate other potential channels for the improvement in children’s health. First, improved child health can be attributed to increased health investments as family size declines, consistent with the quantity-quality trade-off proposed by Becker (1960), which posits a negative relationship between family size and the resources available per child. Second, parenting practices and parent–child interactions suggest that policy-affected parents are more likely to adopt an “authoritative” parenting style, characterized by high responsiveness alongside appropriate supervision

(Baumrind et al., 2010; Doepke and Zilibotti, 2017). This aligns with prior studies on Chinese parents, which suggest that responsive parenting helps alleviate psychological distress, particularly among single children (Liu et al., 2010; Lu and Chang, 2013).

Overall, this chapter provides new causal evidence on the intergenerational effects of family planning policies and family size on subsequent generations. The chapter extends existing research on quantity–quality trade-offs by moving beyond the household level to investigate this relationship at a national scale. It further advances the literature on the intergenerational transmission of health and household characteristics. Our findings underscore that restrictive family planning policies such as the One-Child Policy can have lasting effects on health and well-being across generations. This also highlights the need for future fertility policies to be carefully designed to avoid unintended inequalities.

Chapter 4 is titled “*Genocide, Female Empowerment and Domestic Violence in Cambodia*”. In this chapter, I leverage the Khmer Rouge Genocide in Cambodia to investigate how violence and repression shape women’s empowerment and domestic violence in the long term. The genocide, which took place between 1975 and 1979, resulted in the deaths of an estimated 1.8 million people, approximately a quarter of Cambodia’s population, with men disproportionately targeted (Kiernan, 2003). Gender imbalance caused by the genocide reduced women’s marriage prospects (Heuveline and Poch, 2006) and left many households headed by widows (Chandler, 2018). While the immediate demographic effects are well documented, less is known about how this disruption has affected gender roles and family power dynamics over time.

To address the research question, I draw on various datasets, including the Demographic and Health Survey (DHS), the Cambodian Genocide Databases by Yale University, 2008 General Population Census of Cambodia, and other historical and contemporary data on geographic, demographic, and economic characteristics. I use a spatial RDD, using the zone border from Grasse (2024) that separates the Southwest and West Zones, to compare individuals in high-repression areas (Southwest Zone) versus moderate-repression areas (West Zone). The distance to this border serves as the running variable, allowing me to assess how genocide intensity shapes long-term outcomes in female empowerment and domestic violence.

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I first use data from the Cambodian Genocide Databases, which provide information on 309 geo-coded sites of 18,953 genocide mass graves, to empirically investigate genocide intensity across the Southwest – West Zone Border. I provide evidence that villages in the extremist Southwest Zone are more likely to have mass graves in their vicinity compared to those in the moderate West Zone. The analysis indicates substantial variation in repression intensity across the Southwest–West Zone border, which may generate long-term differences in female empowerment and domestic violence.

The baseline results show persistent adverse effects of the genocide on female empowerment and domestic violence. In locations with high repression intensity, women today are less empowered within households: they are less likely to work, less involved in household decision-making, and more likely to be controlled by their husbands. While no significant effects are observed for physical violence, women in high-repression areas face a greater risk of experiencing emotional violence, either alone or in combination with physical violence.

I then investigate several mechanisms to explain the main results. The first likely mechanism is skewed sex ratios and higher fertility rates post-genocide. Second, the genocide has significant impacts on household structure and household wealth, with households in high-repression areas today having more children and lower wealth. Third, the main findings can be attributed to changes in gender-related characteristics. Both men and women marry at younger ages and achieve lower levels of education. Women are more likely to work part-time or in occasional jobs rather than full-time positions. Meanwhile, men are more likely to have multiple marriages or unions. All of these factors contribute to persistent gender inequalities, limiting women’s bargaining power within households, which in turn increases vulnerability to domestic violence.

Overall, the chapter contributes to existing literature on how conflicts and violence affect gender inequality. The results support home bargaining theory, which states that greater economic opportunities for women reduce domestic violence by enhancing their bargaining power and raising the costs of abuse for men (Meyer et al., 2024). In this story, when women have limited economic security or autonomy, they are more vulnerable to domestic violence, as men retain greater control over household decisions. The

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chapter highlights how mass violence and institutional changes continue to affect gender inequality. Post-conflict recovery, therefore, should address the legacies of violence that restrict women's opportunities, thereby reducing domestic violence and promoting gender equality.

Chapter 5 concludes the thesis by summarizing the main findings and their broader contributions. It also highlights policy implications and outlines future research directions on the long-term and intergenerational effects of conflict and demographic policies.

Chapter 2

The Aerial Bombing of Cambodia and the Recovery of Communities

2.1 Introduction

Millions of people in developing countries live in communities previously exposed to conflict. Peace brings an opportunity for recovery; however, post-conflict, some countries experience development and growth, while others languish behind (Rohner and Thoenig, 2021). One explanation for renewed growth is that wartime destruction frees up vintage constraints, enabling new infrastructure and capital, similar to findings on development after destruction following the Boston Fire by Hornbeck and Keniston (2017). Another possibility is that instead of destruction, conflict can leave behind an increased risk to activity when explosive ordnance fails to detonate.

We study how the legacy of either past destruction or unexploded ordnance (UXO) affects the long-term recovery of communities. There is evidence that UXO exposure harms local populations (Guo, 2020; Nguyen et al., 2024; Riano and Valencia Caicedo, 2024), but little evidence on how conflict generates heterogeneous development patterns. Conceptually, we propose that conflict creates separate mechanisms that either

trigger recovery or leave *risk*, depending on the likelihood of detonation failures. We face two key empirical challenges: one, observing variation in the likelihood of UXO across locations; and two, isolating the effect of exposure to past conflict across locations with different risks of UXO.

To address these challenges, we examine the case of Cambodia, a nation profoundly impacted by US bombing campaigns during the late 1960s and early 1970s. We use geo-coded data on the universe of US airstrike missions during the conflict from the Yale University Cambodian Genocide Program. Based on this data, we identify areas impacted by explosive ordnance and establish bombing boundaries, dividing regions subjected to airstrikes from those that were not.¹ To distinguish between bombed locations with high versus low UXO risks, we draw on Lin (2022) who demonstrates with extensive ethnographic fieldwork that bombs were less likely to detonate on soft soil. We match soil conditions to locations and create a soft/hard soil measure that we interpret as high/low UXO risk, which we then use to split our analysis.

Identifying the long-term effects of the bombing is challenging due to potential selection bias in bombing site locations. To overcome this challenge, we employ a spatial regression discontinuity design (RDD), similar to Bühler (2023), Dell (2010), and Dell et al. (2018), with the geographic coordinates of locations as running variables. We assume that outcomes change smoothly across geographic space and that bombing and soil softness are orthogonal, allowing us to compare outcomes on either side of constructed bombing boundaries separately by soft and hard soil locations. We empirically confirm that our design passes a range of validity checks around these two core RDD assumptions, ensuring that observed discontinuities in outcomes are attributable to bombing exposure rather than other factors (Section 2.4) and that soil softness is independent of residing in a bombed location as well as of a range of measures on the intensity of airstrikes.

To implement our strategy and test our predictions, we construct a unique dataset drawing on a wide range of information over 30 years post-bombings. We extract rich

¹We leverage a method from epidemiology that draws grids across Cambodia, preserving the spatial clustering of strikes. We discuss this in more detail in Section 2.3.

measures of individual health outcomes with household geo-coordinates from repeated-cross sections of the Cambodian Demographic and Health Survey (DHS). Further, we match geo-coded information on UXO incidents and information on local development measures to the universe of village locations based on village coordinates from the 2008 Cambodia Population Census. Finally, we draw on extensive survey data on households' agricultural productivity, risk-taking behavior, and income matched to their village locations.²

We start by empirically assessing the likelihood and extent of UXO incidents in communities within bombed locations across soft and hard soil locations. Consistent with Lin (2022)'s observations, we find that UXO incidents are significantly more likely in bombed, soft soil communities by around 5 percentage points, and the number of incidents is higher by over 50% of the mean. In Cambodia, bombed areas with soft soil remain disproportionately affected by past exposure, whereas harder ground locations are more likely to be safe for re-development.

Motivated by these observations on UXO patterns, we turn to our key question: Is there a divergence in the recovery of communities? We begin with health, which directly affects well-being and indirectly impacts productivity (Deaton, 2003; Helliwell et al., 2020). Aggregating our health measures into an index, we test our predictions around divergence through *trigger* versus *risk* mechanisms. Empirically, we find that health outcomes significantly improve in bombed, hard soil areas by 0.027 index points or over 6% of a standard deviation, while they worsen in bombed, soft soil areas by 0.033 index points, about 8% of a standard deviation. We further show that these results are robust across a wide range of checks against threats to the empirical strategy. In Cambodia, past aerial bombing has led to a divergence in health outcomes across communities that faced different likelihoods of bombs detonating.

To explain these results, we look at our index of economic development in communities. We construct this index based on villages' distances to schools and hospitals, locations within big radio networks, and several indicators of development, such as the share of households with access to water, electricity, toilets, own dwellings, literacy,

²See Section 2.3 for further details.

school attendance, and employment rate. Thus, we capture real measures of infrastructure and development that relate to quality of life. Empirically, we again find a pattern consistent with our predictions. Bombed, soft soil locations experience a decline in economic development of 0.04 index points, or 12% of a standard deviation, while bombed, hard soil locations have improved by around 0.02 index points, or roughly 5% of a standard deviation.

To corroborate these community-level development patterns, we turn to household data from the Cambodian Socio-Economic Survey (CSES) on economic conditions, agricultural productivity, and risk-taking behavior. Over 30 years post-bombing, households in bombed, hard soil locations have higher income, more valuable durable goods, and higher food security, whereas households in bombed, soft soil locations are worse off on each measure. These results reinforce our interpretation: recovery is stronger in communities where conflict left destruction but less UXO risk and weaker where UXO risk persists.

Finally, we demonstrate that households in bombed, soft soil communities experience lower agricultural productivity and are less likely to take on new risk in the form of loans. Reduced field productivity is consistent with ethnographic observations by Lin (2024), who points out that when UXO risk is high, farmers take precautions to lower their risk. For example, they use hand tools and move slowly, reducing their productivity. Moreover, this implies they have less leverage to take out loans for agricultural investment and may be less willing to take on additional risks, consistent with our evidence on loan-taking. Altogether, these observations are consistent with constrained behavior in communities where productivity and income are lower.

Our findings help reconcile mixed evidence on post-conflict recovery by showing that conflict can generate divergent long-run trajectories across communities. In doing so, we contribute to multiple literatures. First, we contribute to the literature on conflict recovery and how explosive ordnance shapes activity in the long term. Some studies on post-conflict recovery observe a return to a balanced growth trajectory and attain a steady-state level, such as in Japanese cities (Davis and Weinstein, 2002) or Vietnam (Miguel and Roland, 2011), while others provide evidence for long-run negative

impacts from conflict with findings from Basque Country (Abadie and Gardeazabal, 2003), Laos (Yamada and Yamada, 2021) or former Yugoslavia (Kešeljević et al., 2025). Recently, attention has turned to unexploded ordnance, demonstrating the long-run cost of UXO across multiple war-torn countries (Guo, 2020; Nguyen et al., 2024; Riano and Valencia Caicedo, 2024). We advance this literature by showing how new growth can be triggered when conflict does not leave lingering risk in the long-run. In contrast, we show that locations hindered by persistent hazards can be constrained and fall behind, leaving a divergence of outcomes across communities.

Second, we contribute to the literature on demining and clearance by focusing on aerial UXO rather than conventional landmine fields. In Cambodia, estimates of clearance benefits remain mixed because removal is costly and many benefits are difficult to measure (Cameron et al., 2010; Harris, 2000); related evidence from Mozambique shows that clearance can unlock connectivity and local economic activity (Chiovelli et al., 2025). Our results suggest that evaluations of aerial bombing legacies, where UXO can be hard to pin down to particular contamination fields, may understate returns to UXO clearance when they omit long-run effects on health and household welfare alongside local development outcomes.

Third, our study also adds to existing literature on the long-term impacts of war and human conflict on health. Most studies in this field have focused on generations directly exposed to conflicts, either *in utero* or in early childhood. Numerous papers have found negative health impacts for those directly exposed to conflicts (Akbulut-Yuksel, 2014; Akresh et al., 2012, 2023; Camacho, 2008; Duque, 2017; Islam et al., 2017; Mansour and Rees, 2012; Maric et al., 2010; Sonne and Nillesen),³ and a wide range of work also finds similar effects on the next generation (Emanuel et al., 1992; Eriksson et al., 2005; Islam et al., 2017; Moyano, 2017). Additionally, conflicts also affect the next generation because parents' health inputs, family background, and environmental factors are determinants of an individual's health (Strauss and Thomas, 2008). However, only a limited number of studies have delved into how exposure to conflicts affect health

³These studies find negative effects from direct conflict exposure on a wide range of outcomes including: lower birth weights, lower height-for-age scores among children, lower adult height, overweight likelihood, or reduced life expectancy.

and well-being in the long-run (Palmer et al., 2019; Yamashita and Trinh, 2022).⁴ We show health can diverge, and even improve or fall behind, depending on the ability to re-vitalize.

Fourth, we speak to the literature assessing how historical differences affect long-run development (Dell, 2010; Dell and Olken, 2020; Dell et al., 2018). War and conflicts may have indirect repercussions on human life through the destruction of infrastructure such as hospitals, schools, and food systems (Frost et al., 2017; Levy, 2002) and through economic wealth and human capital (Ghobarah et al., 2003; Leon, 2012). However, post-war investments in public healthcare, infrastructure, and human capital accumulation have the potential to gradually mitigate and cancel out negative shocks (Strauss and Thomas, 2008). By examining populations living in areas affected by past bombing in Cambodia, we address how conflicts shape local recovery in the long run, showing the impact varies significantly dependent on how the past conflict shapes opportunity or risk today.

2.2 Historical background

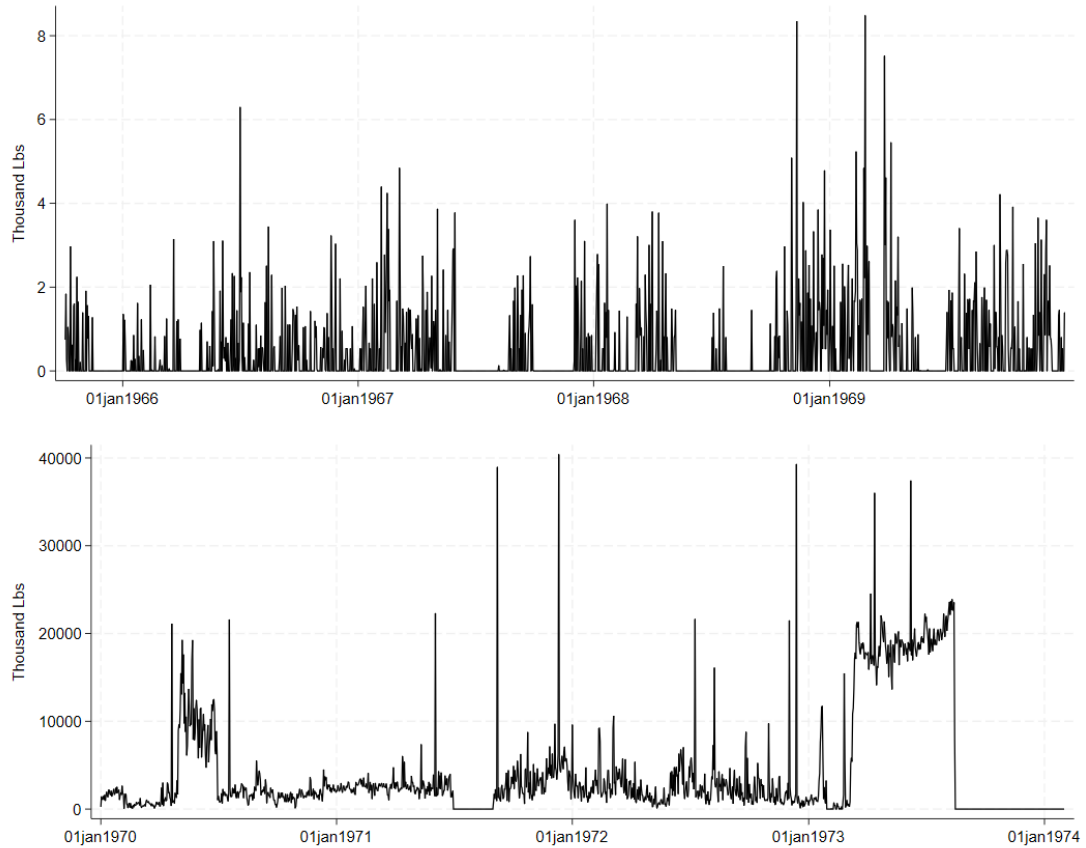
2.2.1 Cambodia’s historical backdrop and destruction from the bombing

Over the last century, Cambodia experienced a sequence of events, including colonisation, civil wars, and genocide (Chandler, 2018; Rany et al., 2012). After a 90-year period of French protectorate and colonization from 1863-1953, independence was established in the country at the Geneva Conference on November 9th, 1953. Following a coup d’état on 18 March 1970, Prince Sihanouk, who was the country’s leader at the time, was deposed by the Lon Nol Government. This event triggered a civil war within the country lasting until 1975. In April 1975, the Khmer Rouge, led by Pol Pot, took control of the country, beginning a period of genocide. The Khmer Rouge regime persisted until 1979, when a new government was established with the support

⁴Palmer et al. (2019) find more disability at present in locations exposed to intense bombing in Vietnam and Yamashita and Trinh (2022) find chemical exposure of locations to Agent Orange in southern Vietnam is still associated with greater disability 30-years later.

of Vietnamese forces. However, political instability still remained in the country until the establishment of the UN protectorate over Cambodia in 1991 at the Paris Peace Conference (Chandler, 2018; Rany et al., 2012).

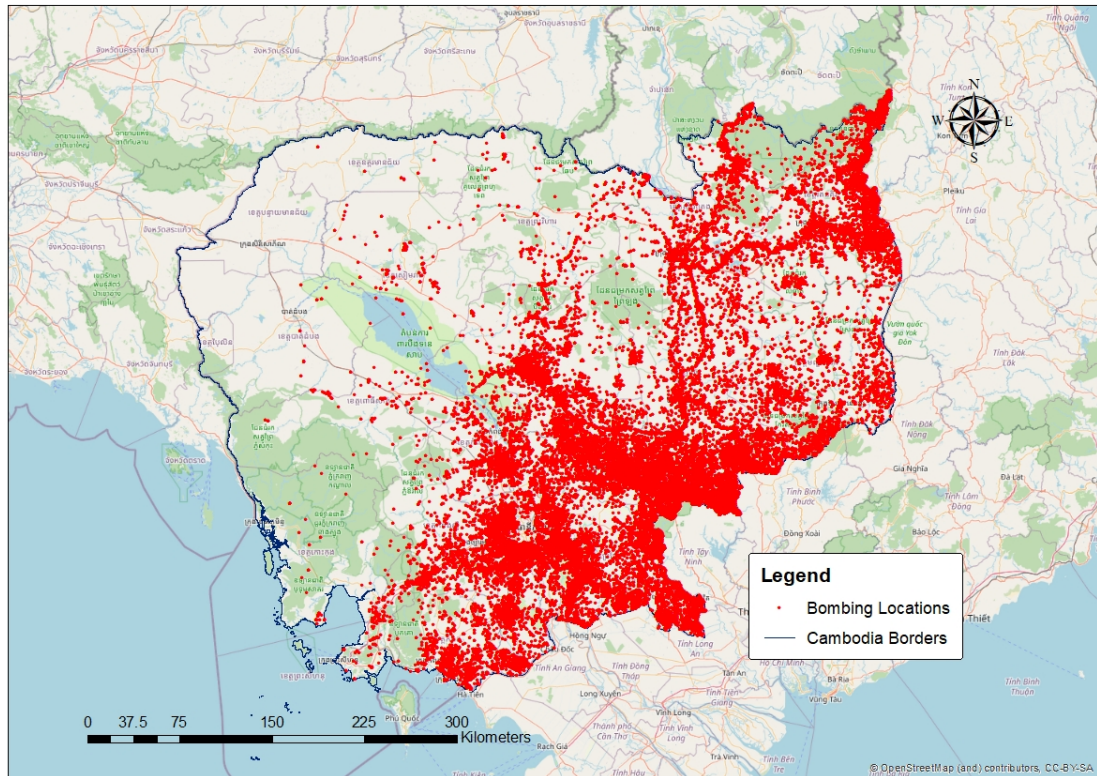
Figure 2.1: US ordnance dropped on Cambodia



Notes: Calculations by the authors from the Yale University Cambodia Genocide Project data on the universe of US airstrikes in Cambodia with detailed information on payload sizes for each strike.

Between 1965 and 1973, the country suffered from the spill-over of the Vietnam-American War through extensive bombing campaigns. Cambodia is historically recognized as one of the most heavily bombed countries (Owen and Kiernan, 2006). Beginning in 1965, under the Johnson administration, Cambodia was subjected to bombing aimed at disrupting supply lines and destroying Communist bases. The initial intense wave of bombings at this time, known as the Menu campaign, targeted Cambodia's border areas and concluded in May 1970 following the coup. After the coup in 1970,

Figure 2.2: Bombing sites targeted in Cambodia



Notes: Red dots give the location of bombing sites between October 1965 and May 1975. Data provided by Yale University (Cambodian Genocide Program). Map overlaid on OpenStreetMap base map and drawn on ArcGIS.

the US bombing campaign was not only to eradicate Vietnam Communist forces but also to support Lon Nol's regime in the internal civil war. Funding for the war was halted in 1973 when the U.S. Congress became aware of Nixon's deception regarding the military campaign (Owen and Kiernan, 2006).

Data from Yale University (Cambodian Genocide Program) reveal that 2,757,107 tons of munitions were dropped on 115,273 bombing sites in Cambodia, far exceeding the worldwide total ordnance dropped by the Allies during World War II (Owen and Kiernan, 2006). As we can see in Figure 2.1, starting from January 1st, 1970, bombing escalated in Cambodia, increasing dramatically from less than one ton per day to hundreds or even tens of thousands of tons per day. Additionally, Figure 2.2 demonstrates that while many of the bombing sites were located in Eastern Cambodia close to Vietnam's borders, the bombing campaign spread over significant portions of

the interior. The previously estimated number of casualties caused by this campaign was between 50,000 and 150,000 people, yet it is alleged that aerial bombings caused the death of 600,000 Cambodians (Ear, 1995), not to mention other consequences such as starvation and displacement. The bombing and conflict also had significant immediate impacts on health in Cambodia with the reduction in life expectancy and poor nutritional outcomes (Moyano, 2017).

2.2.2 Destruction and the opportunity to re-build versus detonation failures

The bombing campaign left widespread destruction. Indirectly confirming the scale of devastation, historical accounts report the bombing contributed to greater support for rebel forces against the western backed government of the time (Chandler, 2018; Owen and Kiernan, 2006). More directly, historical and ethnographic evidence also document that the U.S. bombing displaced rural populations and subsequent return migration after the conflicts, with populations resettling into previously devastated and abandoned landscapes and prioritizing reconstruction in areas perceived as usable and secure (Chandler, 2018; Locard, 2004). Ethnographic work suggests that local agrarian systems and infrastructure were often reset and institutions re-developed from scratch, further suggesting opportunities for a shift in vintage constraints that could potentially open doors for development (Guillou, 2012; Locard, 2004; Vickery, 1999).

The bombs, however, did not always detonate and failures left unexploded ordnance (UXO). In Cambodia, decades of armed conflicts, including the U.S. aerial bombing, the Vietnamese invasion in 1979, and civil wars in the 1970s and 1980s, have deeply contaminated the country with landmines and UXO (Martin et al., 2019).⁵ Cambodia is recognized as one of the most heavily UXO-affected countries with thousands of individuals being incapacitated and losing their lives (Martin et al., 2019; Moyes et al.,

⁵It is critical to distinguish between landmines and UXO in Cambodia. Extensive minefields were laid by the Khmer Rouge, the Royal Cambodian Armed Forces (RCAF), the Vietnamese military and also the Thai army. The majority of these minefields are found in the western regions of Cambodia, notably in “K-5 mine belt” along the border with Thailand. Meanwhile, eastern and northeastern parts of Cambodia are contaminated with unexploded ordnance (UXO) primarily from U.S. air attacks during the Vietnam War (Martin et al., 2019; Roberts, 2011).

2002). Since 1979, Cambodia has witnessed over 64,700 casualties due to UXO, leading to more than 19,700 fatalities (Martin et al., 2019). UXO also causes hindrances to infrastructure, makes land unusable, and leads to interruptions in both water supplies and irrigation systems (Hamlin et al., 2018; Martin et al., 2019).

UXO from aerial bombs is commonly discovered in soft ground, which is less likely to trigger detonation. In other words, areas with high soil fertility that were bombed during conflicts are more likely to contain UXO (Lin, 2022; Moyes et al., 2002). Ethnographic field work indicates that due to the presence of UXO, farmers today change their agricultural practices, for example, through using hand held tools in an effort not to dig too deep. The effect is to render fertile land unproductive due to the high risks associated with farming (Lin, 2022).

Areas with a higher risk of UXO today are likely to experience difficulty in rebuilding and lower growth, while areas with a low risk may be able to leverage rebuilding into growth. This pattern could explain variation in well-being and development today, and we leverage this idea in the empirical analysis to investigate UXO incidents, health, and economic development in communities. We anticipate that the impacts of residing in areas exposed to bombing in the past on UXO incidents, health, and economic development today will vary depending on the ability to re-build after destruction or the presence of persistent hazards from UXO.

Cambodia today is a democracy, but one ruled by a dominant party characterized by corruption (Bühler and Madestam, 2023; Calavan et al., 2004; Un, 2015). This context poses challenges to continued development, and factors hindering recovery for local populations can exacerbate them. As we will show, the history of the US bombing campaigns still plays an important role for communities, demonstrating that re-vitalization of locations is feasible, but challenges can remain in what is an already difficult environment.

2.3 Data

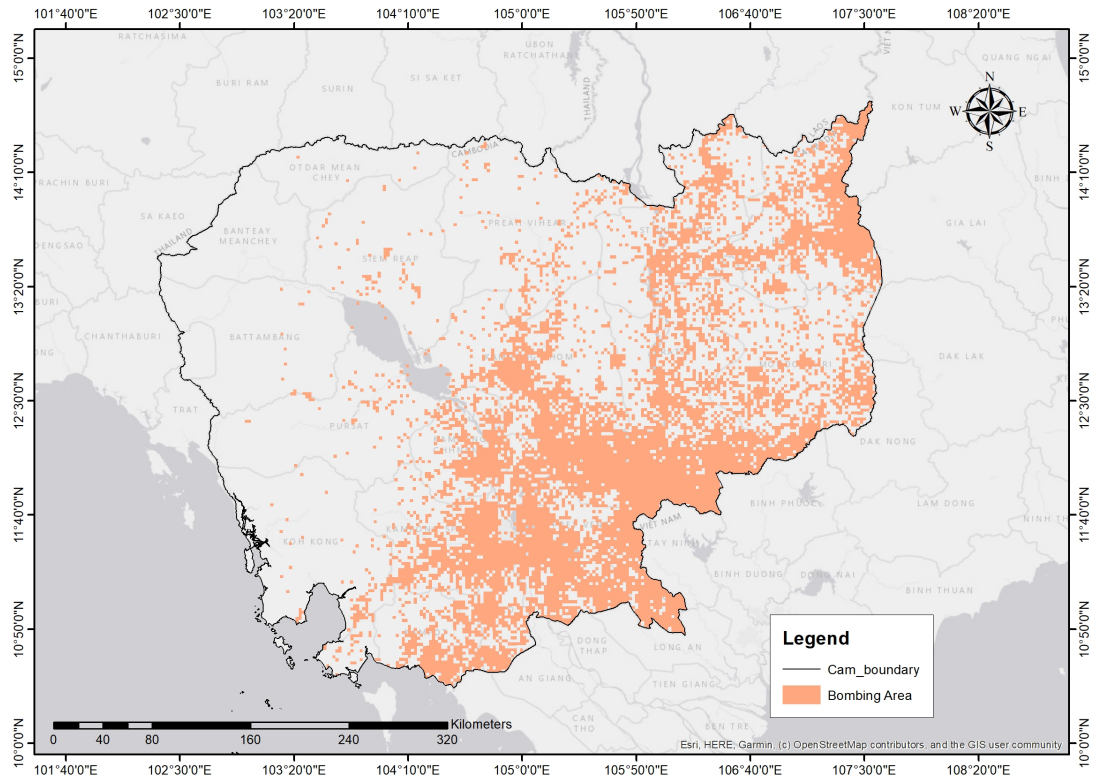
In this section, we outline the data utilized in our study. We integrate diverse data sources, including individual-level health data from spatial data on US airstrike missions, Demographic and Health Surveys (DHS), 2008 Cambodia Population Census, and other historical and contemporary data on infrastructure, economic, and health-care characteristics. Health outcomes are our primary focus and are measured at the individual level using DHS data, based on respondents from DHS sample villages. Meanwhile, UXO incidents and economic development are measured at the census village level. We use village coordinates from the census data to define the universe of village locations and to merge in additional datasets. Detailed descriptions of the datasets employed in this paper can be found in Appendix A.1.

Bombing and the identification of bombing areas. The first dataset we use in this paper is the bombing data compiled by the Yale University Cambodian Genocide Program. This dataset contains information on 115,273 U.S. bombing sites in Cambodia between October 1965 and May 1975, including bombing dates, precise locations, aircraft, bombing loads, and ordnance details. We use this dataset to identify regions heavily affected by bombing, referred to as bombing areas, by applying a grid-based clustering technique that captures the spatial concentration of bombing incidents. This method is widely used across disciplines, including geography, public health, and ecology (Aldstadt, 2009; Grubestic et al., 2014).⁶

In particular, we divide the country map into small geographic grid cells. The effectiveness of this method depends on cell size: small cells can lead to a noisy density estimate, while large cells may excessively smooth the density estimate (Ankerst et al., 1999; Cheng et al., 2018). We then use an approach commonly employed by ecologists to identify the size of the grid cells. As outlined by Boots and Getis (1988), a suitable grid size can be estimated as double the area per point, specifically: $I = \sqrt{2 \times A/n}$, where I denotes the calculated length of the side of a grid cell, A denotes the area of the focused region, and n denotes the number of features – airstrikes in our case – in the

⁶See Appendix A.1 Section A.1.1 for further details.

Figure 2.3: Areas of bombing in Cambodia(1965-1975)



Notes: The map depicts areas that suffer from bombing during 1965-1975 period. Map overlaid on World Light Gray Base map (Canvas Map) on ArcGIS.

study area. Our cell size equals 5.856 km^2 (2.42 km on each side), given Cambodia's overall area being $337,561 \text{ km}^2$ and the number of airstrikes being 115,273.⁷ In total, the country is divided into 31,799 grid cells.⁸ After identifying the bombing loads in each cell, only cells that have bombing loads greater than 0 are selected and defined as bombing areas.

Figure 2.3 illustrates the spatial distribution of identified bombing areas in Cambodia, demonstrating that they preserve the spatial distribution of strike locations. These specified areas depict the clustered spatial occurrences of bombing, and we con-

⁷This is similar to the cell size selected by Kohama et al. (2020) who examine how the economic characteristics of conflict zones influence the choice of military strategies.

⁸While this approach would produce about 57,600 cells based on total national area, not all cells were suitable for analysis. Cells that fell outside the national boundary and overlapped permanent water bodies were removed. After applying these exclusions, 31,799 grid cells remained and were used in the analysis.

sider areas outside these boundaries as the areas not exposed to bombing. The bombing areas are unevenly distributed across the country, with higher concentrations in eastern and southern regions along the border with Vietnam. We then construct a treatment variable that equals 1 for locations within bombing areas and 0 otherwise.

Data on community recovery. We draw on a wide range of datasets to measure community recovery, using Cambodia Demographic and Health Surveys (DHS) data to capture health outcomes and multiple data sources, including 2008 Population Census data and other infrastructure datasets, to measure post-bombing economic development.

We construct a variable *Health Index* based on women’s health outcomes from four waves of the Cambodia Demographic and Health Surveys (2000, 2005, 2010, and 2014). DHS surveys provide geolocations for DHS household clusters or DHS household villages, which are groups of 25–30 households. This allows us to identify whether household locations fall inside or outside the identified bombing areas.⁹ The variable *Health Index* is constructed following Anderson (2008)’s approach, which combines multiple standardized components into a weighted summary index. The component outcomes include Height-for-age Z-scores, Weight-for-height Z-scores, a healthy Body Mass Index, a healthy Corpulence Index, and an indicator for non-anemia.¹⁰

Similarly, to capture economic and infrastructure development at the village level, we follow Anderson (2008) to construct a composite index – *Economic Development Index* (or *Economic Index* for short) – by integrating multiple data sources. Specifically, we merge the locations of all villages in Cambodia from the 2008 Population Census (*census village* level) with data on School Locations in Cambodia (2012) to calculate distance to schools, Health Facilities in Cambodia (2010) to measure distance to hospitals, and Radio Stations in Cambodia (2012) to identify major radio networks. In addition, we incorporate village-level indicators from the 2008 Population Census, including the percentage of households with access to improved water, electricity, and

⁹For the rest of the paper, we call DHS household clusters *DHS villages* to distinguish them from the *census villages* in the 2008 Population Census.

¹⁰See Appendix A.1 Section A.1.2 for further details.

toilet facilities; home ownership; adult literacy rate; school attendance rate; and employment rate. Together, these variables form a comprehensive index of *real* village-level economic development.¹¹

Moreover, we use Cambodia Socio-Economic Surveys (CSES) 2009 and 2014 to investigate post-bombing households' outcome, including incomes, expenditures, agricultural activities and vulnerability to food shortages, field productivity and risk-taking behavior. Although the CSES 2009 & 2014 do not report household geolocations, it provides their village names, thus we geo-locate these households using village coordinates from the 2008 Cambodia Population Census.

Pre-bombing Data. We use the data on the distribution of soil types in Cambodia provided by Crocker (1962) to identify pre-bombing soil conditions.¹² Based on the discussion on soil type characteristics in White et al. (1997) and similar to the classification in Kohama et al. (2020), we create a dummy variable, Soil fertility in 1962, which equals 1 if a village is located in areas that are considered fertile (soft soil) and equals 0 otherwise.¹³ A detailed discussion on soil classification is provided in Appendix A.3.

To capture pre-bombing demographic and economic characteristics, we digitize three maps from the 1970 Indochina Atlas for transportation links, agriculture, and population. We argue these are “pre-bombing” characteristics, because although bombing began in the mid 1960s, the bombs escalated over a thousand times since 1970 (Owen and Kiernan, 2006), meaning that most of the strikes occurred after 1970 (see Figure 2.1). We also use the Global Agro-Ecological Zones (GAEZ) classification developed by the Food and Agriculture Organization of the United Nations (FAO) and the International Institute for Applied Systems Analysis (IIASA) to assess climate and potential crop productivity at DHS villages or census villages.¹⁴

¹¹See Appendix A.1 Section A.1.2 for further details.

¹²The data were provided to Open Development Cambodia in ESRI Shapefile format by Save Cambodia's Wildlife's 2013 Atlas Working Group. For more details, see <https://opendevelopmentcambodia.net>.

¹³Specifically, among sixteen different soil types given in Crocker (1962), the following six types of soils are classified as fertile (soft soil): Latosols, Alluvial soils, Brown alluvial soils, Lacustrine alluvial soils, Regurs, and Brown hydromorphics (Kohama et al., 2020).

¹⁴We create a binary variable indicating whether a village is located in a grid cell characterized as tropics and lowland. See Appendix A.1 Section A.1.3 for further details.

Data on unexploded ordnance. We use data on Mine/ERW casualties (2005-2013) to test whether UXO is more prevalent in regions with pre-bombing softer soil.¹⁵ This dataset provides detailed locations of landmine and explosive remnants of war incidents. Our analysis focuses on ERW incidents possibly associated with aerial bombing¹⁶ and excludes landmines and other ground-based explosive weapons. We count the number of incidents within 3 km of villages from 2008 Cambodia Population Census (*census villages*) and construct a binary outcome equal to 1 if there are any incidents in a 3-km radius.

We provide descriptive statistics for all variables in Appendix A.1 Table A.1 and Table A.2. We now turn to an identification strategy based on spatial discontinuities to better understand the long-term impact of the bombings across locations by their risk of UXO today.

2.4 Empirical strategy and validity

Empirically, we aim to provide evidence on whether there is a divergence in post-conflict recovery across communities, depending on whether conflict opened up opportunities or left lingering barriers. To answer this, we develop a spatial regression discontinuity (RD) design that exploits discontinuous transformations at bombing boundaries. We compare individuals living in locations that were bombed 30-40 years ago to those living in adjacent locations that were not. We disaggregate this effect across soft and hard soil locations, which are independent of bombing locations (see Table 2.1), and capture the risk of detonation failures. This allows us to evaluate whether there are diverging consequences of past conflict in Cambodia.

¹⁵See Section 2.5.1.

¹⁶ERW possibly associated with aerial bombing include Cluster Bomb Unit (CBU) submunitions, fuses, ammunition, other, and unknown.

2.4.1 A spatial approach

We develop a spatial regression discontinuity similar to designs in Bühler (2023) and Dell and Olken (2020). Our regressions take the form:

$$Y_{icgpt} = \alpha + \beta_1 Bombing_c + \beta_2 Bombing_c \times SoftSoil_c + \beta_3 SoftSoil_c \quad (2.1) \\ + f(Geo_c) + \lambda \mathbf{X}_c + \theta_g + \delta_p + \tau_t + \epsilon_{icgpt}.$$

Drawing on a wide range of datasets, we define outcomes for communities (c). Our main outcome for well-being is measured by our health index from the DHS survey and varies for a woman (i) who resides in a community c defined by the coordinates of her DHS village. Additionally, we examine other outcomes for a first-stage analysis of UXO incidents and economic development at the community level c based on the census of all village locations.

We define a location as bombed ($Bombing_c = 1$) if it is within one of the bombing areas we developed across Cambodia (see Figure 2.3). To address our central questions about divergence, we disaggregate the effect of living in a bombed location. First, we include an interaction between $Bombing_c$ and an indicator for soft soil to distinguish the *risk* and *trigger* mechanisms. Where a *trigger* mechanism allows long-term growth post-bombing we expect $\hat{\beta}_1 > 0$, while we expect $\hat{\beta}_2 < 0$ and $\hat{\beta}_1 + \hat{\beta}_2 < 0$ if bombed soft soil areas capture the risk mechanism through UXO. Second, we will present specifications that estimate β_1 disaggregated by sub-samples across soft and hard soil locations, allowing the coefficients on all the controls and fixed effects to vary across these locations.

To estimate these effects, we rely on a spatial design and restrict to locations near bombing boundaries, implementing an optimal bandwidth selection. We control for smooth functions of geographic locations in $f(Geo_c)$ with demeaned x- and y-coordinates of household locations as running variables. Following Gelman and Imbens (2019), we use a local linear RD polynomial for the baseline specification ($f(Geo_c) = x + y$) and examine specifications with higher orders of RD polynomials in robustness checks. These capture the distance to the bombing boundaries and

the gradual variation of unobservable factors in two geographical dimensions. As long as potential outcomes transform smoothly over geographic space, we accurately identify the discontinuity at the boundaries due to the prolonged consequences of the bombings.

In all regressions, we use a triangular kernel, where the weight assigned to each observation diminishes as the distance from the bombing boundaries increases. Finally, we implement an optimal bandwidth around the bombing borders for our main outcome – *Health Index* – following Calonico et al. (2020), with alternative bandwidth restrictions analyzed in robustness checks. Unless specified otherwise, all analyses in our paper use a bandwidth of 2km from bombing boundaries.¹⁷

We also include a set of additional controls. We include a vector of pre-bombing characteristics of locations ($\mathbf{X}_c$) and further add distance to the capital of Cambodia and to Vietnam border to capture the intensity of the conflict.¹⁸ To capture spatial fixed effects and ensure comparisons of individuals within close areas, θ_g is a vector of 50x50 km grid-cell fixed effects.¹⁹ δ_p is a vector of province-fixed effects, controlling for administrative differences. Finally, when using the DHS health data, we include survey-year fixed effects (τ_t), as this data is a set of repeated cross-sections.

Including segment-fixed effects is important in spatial RD designs to ensure comparisons within close geographic proximity (Dell, 2010; Dell and Olken, 2020; Dell et al., 2018; Lehner, 2021). In our context, bombing boundaries are numerous and spread throughout the country. In order to control for geographic treatment effect heterogeneity and to ensure that we compare individuals located very close to each other, we include 50x50 km grid-fixed effects in our main specification and province-fixed effects.²⁰ A concern, however, is that any within-grid sorting would bias our effects. We address this through refinements of 50x50km grid-fixed effects and province-fixed effects in our robustness checks. Particularly, we replace 50x50km grid-fixed effects with smaller sizes of grids and province-fixed effects with district-fixed effects, making

¹⁷*Economic Development Index* also has an optimal bandwidth of approximately 2km.

¹⁸Pre-bombing characteristics include geographic characteristics (elevation, tropics/lowland), demographic characteristics (population density in 1970) and other economic characteristics (agricultural activities and distance to main roads/railways in 1970).

¹⁹We divide the country into 79 grid cells of 50x50km. See Figure A.10.

²⁰There are 25 provinces in Cambodia. The smallest province is Kep, covering an area of 336 square kilometers, while Monduliri is the largest with an area equal to 14,288 square kilometers.

comparisons within a highly confined area. We will show later that our results from this approach remain strongly robust.

Leveraging boundaries. Why should the boundaries in our design be useful to assess discontinuities? First, in settings with historical borders, the borders are almost necessarily approximations, implying measurement error in the locations of the drawn borders. As long as this error is random, it should attenuate results. Second, in our setting, let us suppose a true set of bombing boundaries exists. This latent set of borders is unobservable, so we aim to approximate them. Importantly, our method, described in Section 2.3, intuitively draws borders that are random relative to other features, as confirmed next by our balancing tests. However, it also captures the spatial dispersion of strikes, plausibly approximating the latent borders. Thus, we expect our borders to be measured with some degree of random error that will attenuate our results. Later, we will provide evidence consistent with this intuition through a range of placebo tests in the robustness checks.

To interpret estimates of bombing across soft and hard ground locations, we need the following assumptions: unobservables evolve smoothly across bombing boundaries, there is no sorting at bombing cutoffs, and bombing locations and soil softness are orthogonal. Next, we investigate these and provide evidence to support them.

2.4.2 Validity

Independence of Bombing and 1962 Soil Types Table 2.1 shows that 1962 soil types are strongly independent of bombing in our design. In particular, they are uncorrelated with the treatment indicator (column 1) and with a range of bombing measures known to produce unexploded ordnance (Lin, 2022), including bombing loads (column 2), cluster munitions (column 3), and bombs delivered by B-52 aircraft (column 4). We conduct the analysis for both census villages and DHS villages because the DHS sample allows us to examine our main outcome – individual-level *Health Index*, while the census sample, which covers a much larger number of villages, enables us to analyze village-level outcomes with greater statistical power. Overall, evidence from both samples supports our identification strategy that separates effects using 1962 soil

types.

Table 2.1: Independence of Bombing and 1962 Soil Types

	Dependent variable is:			
	(1) Bombing	(2) Ln(Bombing Load)	(3) Cluster Bombs	(4) B52
<i>Panel A: Population Census Villages</i>				
Soil 1962	-0.011 (0.021)	-0.285 (0.319)	-0.010 (0.014)	-0.005 (0.008)
Mean	0.562	8.055	0.141	0.044
$ \beta /\text{Mean}$	0.020	0.035	0.069	0.103
Observations	7055	7055	7055	7055
Clusters	1117	1117	1117	1117
<i>Panel B: DHS Villages</i>				
Soil 1962	-0.006 (0.049)	-0.092 (0.706)	-0.001 (0.028)	0.013 (0.016)
Mean	0.505	7.371	0.120	0.055
$ \beta /\text{Mean}$	0.012	0.012	0.006	0.232
Observations	1052	1052	1052	1052
Clusters	610	610	610	610

Note: The sample is restricted to villages within a 2km bandwidth from bombing boundaries. All regressions use a local linear polynomial in spatial coordinates with a triangular kernel weight. We control for 50x50km grid fixed effects and province fixed effects in all regressions. *Soil 1962* is a dummy variable demonstrating whether the soil was fertile(soft) in 1962 (before the bombing). *Bombing* is our treatment variable indicating locations inside our defined bombing areas. *Ln(Bombing Load)* is the natural log transform of Bombing Load. *Cluster Bombs* is a dummy variable indicating whether the bombing grid contains cluster bombs. *B52* is a dummy variable equal to 1 if the grid was bombed by B-52 aircraft and 0 otherwise. B-52 bombers have a high payload capacity, allowing them to deliver large volumes of ordnance over a target area. Standard errors reported in parentheses are clustered at the commune level. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

Smooth variance of covariates at bombing boundaries. A key assumption of the RD design is the smooth variance of all relevant factors and covariates besides the treatment at the discontinuity (Dell, 2010). This assumption allows for individuals on the non-bombing side to serve as a valid counterfactual for individuals on the bombing side. In order to assess the plausibility of this assumption, we use equation 2.1 to examine a wide range of geographic, demographic and economic characteristics in the pre-bombing period, which we measure at the village level.

We conduct balance checks separately for both DHS villages and census villages.

DHS is a repeated cross-sectional dataset that provides rich health outcomes for individuals residing in DHS villages, whereas the Census covers the full universe of villages. Reporting balance tests for both samples ensures that our identification assumptions hold in each. Results are reported in Table 2.2 for DHS villages and Table 2.3 for census villages. Overall, we find no evidence that there were discontinuities of geographic, demographic or economic features at the bombing boundaries when using the entire sample (Panel A), except for some marginally significant effects for census villages. To further support our use of 1962 soil type to disaggregate our coming analysis, we split the country by locations with soft or hard ground from a 1962 measure of soil type and re-run these balance checks (Panels B and C). We do not view any statistically significant estimates as concerning because their magnitudes are small relative to the mean. In terms of the tropics/lowland in soft soil areas (Panel B - Table 2.3), this imbalance is unlikely to bias our estimates because the feature is time-invariant, pre-determined, and directly related to soil classification. Overall, our evidence for the continuity assumption holds in both soft and hard soil areas.

No sorting around bombing boundaries. Another important assumption for the validity of our design is that individuals cannot sort themselves around the cut-off boundaries. This assumption would be violated if, at the time of bombing, individuals were able to sort themselves around locations more likely to be bombed. Our method of drawing borders should be independent of features that may lead to sorting. Consistent with this assumption, we find that population density in 1970 was balanced across bombing boundaries, with no statistically significant differences (columns 4 in Tables 2.2 and 2.3).

Stable Unit-Treatment Value Assumption (SUTVA). SUTVA requires that a unit's outcome is unaffected by the treatment status of other units (Keele and Titiunik, 2015). We will focus our analysis on comparisons around boundaries in the subset of soft soil locations and in the subset of hard soil locations. A concern is that people living in soft soil, bombed areas (high UXO risk) might relocate to nearby control areas (non-bombed, soft soil). We expect this pattern to depress outcomes in our control

Table 2.2: Balance check - DHS villages

	Dependent variable is:						
	(1) Elevation	(2) Tropics/lowland	(3) Agri. Activities	(4) Pop. Density	(5) Dist. to roads	(6) Dist. VN	(7) Dist. to capital
Panel A: All observations							
Bombing	-3.156 (2.193)	0.032 (0.039)	0.003 (0.018)	-0.010 (0.015)	0.096 (0.442)	-0.200 (0.487)	-0.251 (0.968)
Bombing $\times$ Soft Soil	12.017** (5.212)	0.046 (0.066)	-0.002 (0.033)	0.004 (0.029)	-0.081 (0.781)	0.295 (0.830)	-0.472 (1.544)
Mean	39.381	0.601	0.944	0.808	6.490	87.687	102.087
$ \beta /\text{Mean}$	0.080	0.053	0.003	0.012	0.015	0.002	0.002
Observations	1050	1052	1052	1052	1052	1052	1052
Clusters	610	610	610	610	610	610	610
Panel B: Soft soil (1962)							
Bombing	9.816 (5.968)	0.077 (0.051)	0.001 (0.026)	-0.027 (0.024)	-0.117 (0.643)	-0.081 (0.688)	-0.077 (1.139)
Mean	46.406	0.560	0.924	0.812	6.013	74.679	88.875
$ \beta /\text{Mean}$	0.212	0.137	0.001	0.033	0.019	0.001	0.001
Observations	369	369	369	369	369	369	369
Clusters	241	241	241	241	241	241	241
Panel C: Hard soil (1962)							
Bombing	-2.264 (2.006)	0.025 (0.038)	0.005 (0.017)	-0.009 (0.015)	0.121 (0.444)	-0.118 (0.456)	-0.019 (0.980)
Mean	35.435	0.623	0.955	0.806	6.757	94.973	109.487
$ \beta /\text{Mean}$	0.064	0.041	0.006	0.012	0.018	0.001	0.000
Observations	668	670	670	670	670	670	670
Clusters	416	416	416	416	416	416	416

Note: The unit of analysis is DHS villages. The sample is restricted to villages within a 2km bandwidth from bombing boundaries. All regressions use a local linear polynomial in spatial coordinates with a triangular kernel weight. We control for 50x50km grid fixed effects and province fixed effects in all regressions. *Tropics/lowland* is a dummy variable indicating whether this location falls within areas classified as “tropics, humid” in the agro-ecological zones classification. Columns (3) (4) (5) use data from the Indochina Atlas, published in October 1970. *Agri. Activities* indicate whether there were any agricultural activities in these areas in 1970. *Pop. Density* is a binary variable reflecting if the population density in 1970 was at least 10 inhabitants per square kilometre. *Dist. to roads* refers to distance (in km) to 1970 main roads/railways. *Dist. VN* indicates distance to Vietnam border. *Dist. to capital* refers to distance to the capital of Cambodia. Standard errors reported in parentheses are clustered at commune level. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

locations, as those from high UXO areas likely lack resources and skills, obscuring the true harmful effect of the bombing exposure. In hard soil areas with low UXO risk, this displacement would still bias estimates toward zero because the contrast between treated and untreated locations shrinks. A second concern is that bombs may have missed their targets and destroyed both treated and control locations. In this case, we expect effects to move in the same direction – hard soil locations can re-build, soft soil locations become UXO prone – bringing the outcomes of treated and control units closer together, obscuring the true effect. Thus, we expect SUTVA violations would likely bias the bombing effect in our design toward zero. Later, in a donut exercise (Appendix A.2) that excludes observations closest to our bombing boundaries, we show that our results remain consistent and even become more efficient.

Table 2.3: Balance check - Population Census Villages

	Dependent variable is:						
	(1) Elevation	(2) Tropics/lowland	(3) Agri. Activities	(4) Pop. Density	(5) Dist. to roads	(6) Dist. VN	(7) Dist. to capital
Panel A: All observations							
Bombing	-0.844 (0.640)	-0.020 (0.016)	0.001 (0.009)	0.004 (0.008)	0.019 (0.215)	0.230 (0.192)	-0.538 (0.419)
Bombing $\times$ Soft Soil	2.142* (1.126)	0.074** (0.029)	-0.006 (0.015)	-0.023* (0.012)	-0.411 (0.362)	-0.317 (0.343)	1.309* (0.697)
Mean	31.267	0.527	0.954	0.861	7.472	80.153	82.108
$ \beta /\text{Mean}$	0.027	0.037	0.001	0.004	0.003	0.003	0.007
Observations	7054	7055	7055	7055	7055	7055	7055
Clusters	1117	1117	1117	1117	1117	1117	1117
Panel B: Soft soil (1962)							
Bombing	0.023 (0.781)	0.056** (0.025)	-0.007 (0.010)	-0.017** (0.007)	-0.290 (0.286)	-0.204 (0.255)	0.667 (0.497)
Mean	31.258	0.442	0.935	0.859	7.381	76.407	77.055
$ \beta /\text{Mean}$	0.001	0.126	0.007	0.020	0.039	0.003	0.009
Observations	2532	2532	2532	2532	2532	2532	2532
Clusters	563	563	563	563	563	563	563
Panel C: Hard soil (1962)							
Bombing	-0.174 (0.607)	-0.010 (0.016)	0.010 (0.008)	0.007 (0.008)	-0.007 (0.201)	0.026 (0.178)	-0.587 (0.404)
Mean	31.272	0.574	0.964	0.862	7.522	82.253	84.943
$ \beta /\text{Mean}$	0.006	0.017	0.011	0.008	0.001	0.000	0.007
Observations	4517	4518	4518	4518	4518	4518	4518
Clusters	836	836	836	836	836	836	836

Note: The unit of analysis is 2008 Population Census Villages. The sample is restricted to villages within a 2km bandwidth from bombing boundaries. All regressions use a local linear polynomial in spatial coordinates with a triangular kernel weight. We control for 50x50km grid fixed effects and province fixed effects in all regressions. *Tropics/lowland* is a dummy variable indicating whether this location falls within areas classified as “tropics, humid” in the agro-ecological zones classification. Columns (3) (4) (5) use data from the Indochina Atlas, published in October 1970. *Agri. Activities* indicate whether there were any agricultural activities in these areas in 1970. *Pop. Density* is a binary variable reflecting if the population density in 1970 was at least 10 inhabitants per square kilometre. *Dist. to roads* refers to distance (in km) to 1970 main roads/railways. *Dist. VN* indicates distance to Vietnam border. *Dist. to capital* refers to distance to the capital of Cambodia. Standard errors reported in parentheses are clustered at the commune level. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

2.5 The Long-Run Effects of Bombing on Communities

Our hypothesis is that the aerial bombing in Cambodia either triggered an opportunity for long-term re-vitalization in communities or created risks impeding recovery. To test this, we estimate the effects of bombing separately across locations with different risks of unexploded ordnance, but independent of whether they were bombed. We begin with a first-stage analysis of UXO incidents and then turn to health as our main measure of well-being. We then assess post-conflict recovery in terms of community-level economic development, household income, and field productivity.

2.5.1 First-stage: UXO Incidents in Communities

We begin by assessing the relationship between bombing exposed locations and UXO incidents in and around villages. In areas with softer soil, we expect a higher incident

rate due to a higher failure rate for dropped bombs. In Table 2.4, we implement our empirical approach with two outcomes: the occurrence of any UXO incident near a village (Panel A) and the overall number of UXO incidents (Panel B).²¹

Consistent with our expectations, we find that both the likelihood and the number of incidents are higher in bombed, soft soil areas. These locations are 5.2 percentage points more likely to have experienced UXO incidents (Panel A, column 1), with the estimate being statistically significant both with clustered standard errors and adjusted for spatial correlation with Conley standard errors (reported in the Appendix, Table A.8). Further, we find a statistically significant increase in the number of UXO incidents in bombed, soft soil areas (Panel B, column 1).

We then drop villages within the K5 minebelt. The K5 minebelt is a region in the northwest of Cambodia, dense with landmines from the conflict between Vietnamese forces and the Khmer Rouge, where mines may occur across land types, and this region also has fewer airstrikes from the past US bombings. In column (2), we find that our results and interpretation remain the same after removing villages in this region.

Finally, we split our sample by soft and hard ground locations and make comparisons across bombing boundaries within these sub-samples. We find very similar patterns. Soft ground locations bombed in the past have both a higher likelihood and a higher number of UXO incidents (columns 3-4), while we observe suggestive negative effects in hard ground locations (columns 5-6). While this negative link with bombing in hard locations is small and only marginally significant, it is also not entirely surprising as there can be other types of incidents in Cambodia that are not directly linked to airstrike UXO. Nevertheless, the difference in hard locations is much smaller than the difference observed in soft soil areas, suggesting bombed and unbombed hard ground locations are generally more similar in terms of UXO incidents.

Altogether, our evidence shows that UXO incidents are much more likely in bombed, soft ground locations. As a result, communities in these locations may remain at higher risk and face greater difficulty, leading them to stagnate or fall behind. Meanwhile, bombed hard ground locations experience a much lower risk, suggesting the destruction

²¹We count the number of incidents within a 3km radius around each village.

Table 2.4: Regression Discontinuity - UXO incidents

	All areas		Soft soil (1962)		Hard soil (1962)	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: UXO likelihood</i>						
Bombing	-0.016 (0.010)	-0.017 (0.010)	0.040** (0.019)	0.035* (0.019)	-0.018* (0.010)	-0.017* (0.010)
Bombing $\times$ Soft Soil	0.052** (0.021)	0.049** (0.022)				
Mean	0.066	0.059	0.077	0.064	0.060	0.057
Observations	7054	6653	2532	2407	4517	4242
Clusters	1117	1026	563	526	836	761
Exclude K5 Belt	No	Yes	No	Yes	No	Yes
<i>Panel B. Number of UXO Incidents</i>						
Bombing	-0.018 (0.012)	-0.021* (0.011)	0.048** (0.022)	0.034* (0.019)	-0.022* (0.012)	-0.021* (0.011)
Bombing $\times$ Soft Soil	0.056** (0.025)	0.050** (0.023)				
Mean	0.076	0.064	0.091	0.067	0.068	0.062
Observations	7054	6653	2532	2407	4517	4242
Clusters	1117	1026	563	526	836	761
Exclude K5 Belt	No	Yes	No	Yes	No	Yes

Note: The unit of analysis is census villages (2008 Population Census). The sample is restricted to villages whose centroids are within a 2km bandwidth from bombing boundaries. All regressions use a local linear polynomial in spatial coordinates with a triangular kernel weight. In all models, we control for households' spatial locations (x- and y- coordinates), distance to Vietnam borders, distance to the capital, 50x50km grid fixed effects, province fixed effects and other pre-bombing characteristics. We count the number of UXO incidents within 3km of villages and construct the binary outcome equal to 1 if there are any UXO incidents in a 3-km buffer. Standard errors reported in parentheses are clustered at the commune level. In the Appendix, Table A.8, we also report spatial adjusted Conley standard errors for UXO likelihood. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

is in the past. Communities in bombed but hard ground locations may then have a greater opportunity to develop, if past destruction motivates re-vitalization. Next, we turn to address these questions on the recovery of communities.

2.5.2 Health

Table 2.5 reports our spatial RD estimates for the effect of bombing on our *Health Index* across locations where bombs had different likelihoods of detonating. We proxy a *risk*

channel from past bombs in soft soil, where UXO risk is higher, potentially impeding activity, harming households' well-being – measured by health among women. We proxy a *trigger* channel through hard soil where bombs were more likely to detonate, opening a later opportunity for re-vitalization that could improve well-being.

We start with the pooled model across locations and interact bombing with an indicator for soft soil. Bombed soft soil locations experience significantly worse health outcomes relative to bombed hard soil locations over 30 years post-bombing and 10-20 years since the cessation of conflict in Cambodia (column 1). The difference between bombed soft and hard soil areas is about 0.054 index points or over 12% of a SD (SD=0.43),²² suggesting the presence of *risk* and *trigger* channels. This estimate is significant and remains robust when using Conley standard errors (Appendix, Table A.8).

Table 2.5: The long-term effects of local area exposure to bombing on health

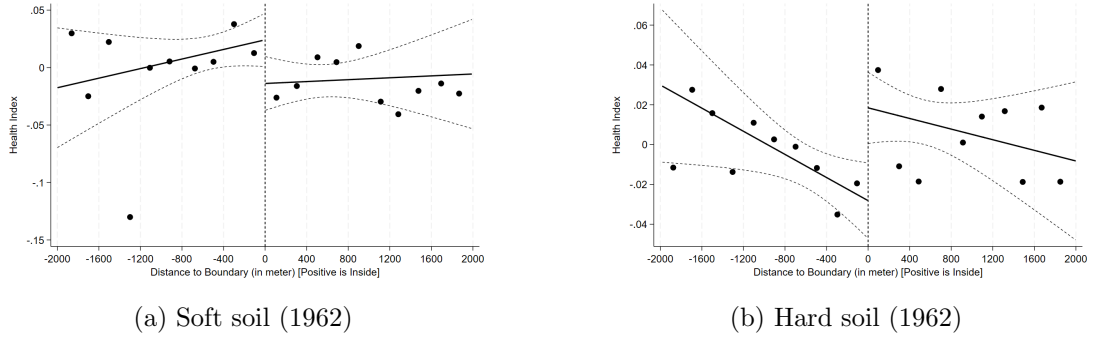
	All areas		Soft soil (1962)		Hard soil (1962)	
	(1)	(2)	(3)	(4)	(5)	(6)
Bombing	0.025** (0.010)	0.028*** (0.010)	-0.033** (0.014)	-0.031** (0.015)	0.027*** (0.010)	0.030*** (0.011)
Bombing × Soft Soil	-0.054*** (0.017)	-0.058*** (0.018)				
Mean	-0.013	-0.013	-0.010	-0.015	-0.015	-0.011
Observations	17400	15779	6260	5819	11140	9960
Clusters	1058	961	382	356	676	605
Exclude K5 Belt	No	Yes	No	Yes	No	Yes

Note: The unit of analysis is DHS survey respondents. The sample is restricted to those living within a 2km bandwidth from bombing boundaries. All regressions use a local linear polynomial in spatial coordinates with a triangular kernel weight and control for 50x50km grid fixed effects, province fixed effects, distance to the capital, distance to Vietnam borders and other pre-bombing characteristics. Regressions (2) (4) (6) exclude provinces along the K-5 mine belt. Standard errors reported in parentheses are clustered at the DHS village level. *Health Index* is the standardized weighted summary index variable created from multiple health outcomes, including Height-for-age Z-scores, Weight-for-height Z-scores, a healthy Body Mass Index, a healthy Corpulence Index, and an indicator for non-anemia (See Table A.6 for results on individual health outcomes). ***(**)(*) indicates significance at the 1%(5%)(10%) level.

We find that women living in high UXO risk locations with soft soil experience significant declines in well-being, while those living in low risk locations report im-

²²See Appendix Table A.1 for summary statistics of *Health Index*.

Figure 2.4: The impacts of local area exposure to bombing on health: RD plots



Note: The points represent binned residuals derived from a main regression of the outcome variable on a linear polynomial in spatial coordinates and other control variables. Solid lines depict local linear regressions, separately estimated on each side of the threshold, while dashed lines represent 95% confidence intervals. “Negative” values of distance indicate locations outside the bombing areas.

provements in well-being. We confirm these patterns, estimating the model restricted to only soft soil (column 3) or only hard soil (column 5) locations. In these sub-samples, we observe a stark asymmetry: bombed locations with soft soil experience significantly worse health by 0.033 index points, equivalent to about 8% of a SD, while health in bombed locations with hard soil improves by about 0.027 index points or roughly 6% of a SD. Figure 2.4 illustrates these results, demonstrating discontinuities at the bombing borders consistent with our evidence in Table 2.5. Our results show that locations with a greater risk of UXO have lower well-being many years post-conflict, and locations with a lower risk of UXO have higher well-being.

Table A.6 (Appendix A.4) reports the estimated effects on individual component outcomes of the *Health Index*. These include height-for-age, having a healthy BMI, no anemia, and others. Across all health outcomes, we consistently observe negative, though not always efficient, estimates for bombing effects in high-risk soft soil areas and positive effects in hard soil areas with a low UXO risk.

Landmines also contaminate Cambodia. In particular, when the Khmer Rouge regime collapsed in the late 1970s, its supporters fled to the west. To deny access to returning rebels, opposition forces laid dense minefields along the border with Thailand known as the “K-5 mine belt” (Roberts, 2011). For our analysis, it is important to note

that the likelihood of encountering landmines does not necessarily follow soil conditions, as with aerial-dropped bombs. Additionally, regions along the Thai border also have a much lower density of past airstrikes. Thus, in columns (2), (4), and (6) of Table 2.5, we repeat our analysis dropping provinces along the K-5 mine belt. Our results remain unchanged.

We further probe the sensitivity of our results across a wide range of checks in Appendix A.2, where we show that our estimates are remarkably robust. They remain stable across alternative bandwidth choices, different polynomial orders, and various specifications of grid-cell fixed effects. Moreover, our findings are robust to multiple specifications, including a uni-dimensional RDD,²³ and are further supported by placebo tests that show no evidence of discontinuities.

We find substantial support for a divergence in post-conflict well-being in Cambodia, where some bombed areas experience better health today, and others have fallen behind. We interpret our evidence on the *risk* channel as consistent with bombs blocking re-development. Additionally, our evidence showing that well-being even improves in the long-run in areas where bombs detonated is consistent with the *triggering* of opportunities post-conflict when feasible. To confirm these interpretations, in Section 2.5.3 and 2.5.4 we turn to the development of community economic conditions and infrastructure, as well as household productivity and income.

2.5.3 Communities' economic development

Table 2.6 presents our spatial RD estimates of the effects on economic development at the census village level. In line with our results on health outcomes, we find contrasting effects across pre-bombing soil types. Again, our results demonstrate negative impacts in soft soil areas and significant positive effects in hard soil areas, providing evidence consistent with UXO-related *risk* channels in soft soils and post-bombing revitalisation (*trigger*) channels in hard soils.

In particular, the estimate from our pooled model indicates that bombed locations with soft soil exhibit significantly worse economic development than bombed locations

²³In a uni-dimensional RDD, we use distance to bombing boundaries as a running variable.

with hard soil, with a difference of 0.044 index points or about 13% of a standard deviation (SD=0.32) (column 1).²⁴ Economic outcomes deteriorate in soft soil areas with high UXO risks but improve in hard soil locations with a low likelihood of UXO. In soft soil areas, *Economic Development Index* drops by 0.04 index points or roughly 12% of a standard deviation for villages inside the bombing areas (column 3). Meanwhile, villages in hard soil areas experience an improvement in economic development by 0.017 index points or 5% of a standard deviation (column 5). These findings remain consistent and even become slightly larger when we drop provinces along the K-5 mine belt (columns 2, 4, and 6). Figure 2.5 illustrates the effects graphically, demonstrating a significant drop in soft soil areas and a clear jump in hard soil areas at bombing boundaries.

Table 2.6: Economic Development Index (*Unit: Census Villages*)

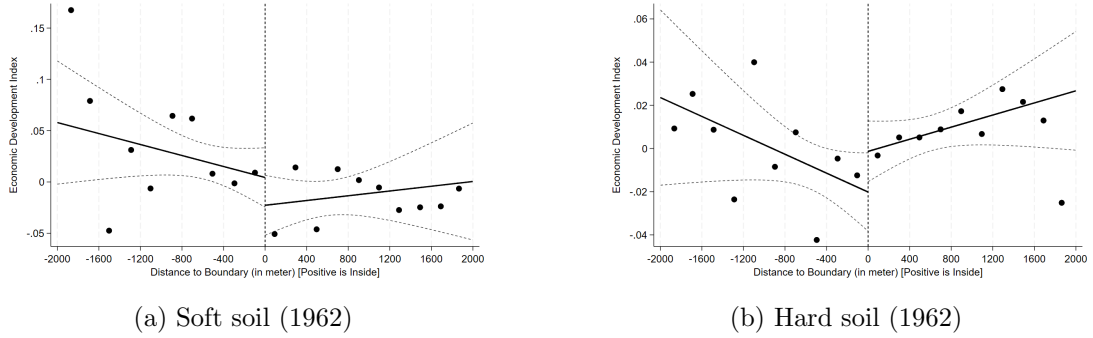
	All areas		Soft soil (1962)		Hard soil (1962)	
	(1)	(2)	(3)	(4)	(5)	(6)
Bombing	0.014 (0.010)	0.017* (0.010)	-0.040** (0.017)	-0.043** (0.017)	0.017* (0.009)	0.021** (0.010)
Bombing $\times$ Soft Soil	-0.044** (0.021)	-0.051** (0.022)				
Mean	0.023	0.029	0.026	0.031	0.020	0.028
Observations	7054	6653	2532	2407	4517	4242
Clusters	1117	1026	563	526	836	761
Exclude K5 Belt	No	Yes	No	Yes	No	Yes

Note: The unit of analysis is 2008 Population Census villages. The sample is restricted to villages within a 2km bandwidth from bombing boundaries. All regressions use a local linear polynomial in spatial coordinates with a triangular kernel weight and control for 50x50km grid fixed effects, province fixed effects, distance to the capital, distance to Vietnam borders and other pre-bombing characteristics. Regressions (2) (4) (6) exclude provinces along the K-5 mine belt. Standard errors reported in parentheses are clustered at the commune level. *Economic Development Index* is the standardized weighted summary index variable created from multiple outcomes including distance to schools, hospitals, locations within big radio network and other indicators of development such as the share of households with access to water, electricity, toilets, own dwellings, literacy, school attendance and employment rate (See Table A.7 for results on individual economic outcomes). ***(**)(*) indicates significance at the 1%(5%)(10%) level.

Table A.7 (Appendix A.4) reports the results on individual economic outcomes that

²⁴See Appendix Table Table A.2 for summary statistics of *Economic Development Index*.

Figure 2.5: Economic Development Index (Census Villages): RD plots



Note: The points represent binned residuals derived from a main regression of the outcome variable on a linear polynomial in spatial coordinates and other control variables. Solid lines depict local linear regressions, separately estimated on each side of the threshold, while dashed lines represent 95% confidence intervals. “Negative” values of distance indicate locations outside the bombing areas.

are included in our *Economic Development Index*. Consistent with the risk channel, soft soil bomb areas have significantly longer distances to health facilities, and worse access to improved water, electricity, toilets, and large radio networks. In contrast, hard soil areas display patterns consistent with a trigger channel, as demonstrated by statistically significant improvements in employment, school attendance and literacy rates, as well as an imprecisely estimated increase in the percentage of households living in their dwellings.

Overall, the results in this section provide evidence that divergences in post-conflict economic development across previously bombed locations with different UXO risks today are a likely explanation for the previously observed differences in long-term health for these two areas. It seems plausible that the observed long-term differences in development and access to infrastructure decades after the bombings relate to differences in opportunities and well-being.

2.5.4 Households’ income and productivity

Our evidence so far aligns with the *trigger* and *risk* channels, which create different futures for the well-being and economic development of communities post-bombing. Under these conditions, we would expect households to experience differences in income

and for households engaged in agriculture to have lower productivity where risk from UXO remains high. Ethnographic work by Lin (2022, 2024) suggests that where UXO risk is high, people adapt to their new living conditions, such as adopting alternative agricultural practices. However, their locations do not develop, and they work with caution to reduce risk, resulting in lower efficiency and productivity. Put simply, life becomes harder due to the ever-present nature of the UXO risk.

Table 2.7: Post-bombing Development at household level (CSES)

	Dependent variable is: <i>Unit of Analysis: Households</i>			
	Ln(Income)	Ln(Expenditure)	Ln(Durable)	Food Secure
	(1)	(2)	(3)	(4)
<i>Panel A: All observations</i>				
Bombing	0.290*** (0.107)	0.020 (0.034)	0.139** (0.070)	0.041* (0.022)
Bombing $\times$ Soft Soil	-0.549*** (0.159)	-0.060 (0.049)	-0.347*** (0.115)	-0.107*** (0.034)
Mean	14.054	16.410	14.102	0.859
Observations	11232	11232	11231	11232
Clusters	752	752	752	752
<i>Panel B: Soft soil (1962)</i>				
Bombing	-0.277** (0.123)	-0.051 (0.039)	-0.259*** (0.095)	-0.083*** (0.028)
Mean	13.950	16.540	14.387	0.871
Observations	4727	4727	4726	4727
Clusters	326	326	326	326
<i>Panel C: Hard soil (1962)</i>				
Bombing	0.275** (0.111)	0.019 (0.033)	0.119* (0.070)	0.043** (0.022)
Mean	14.130	16.315	13.895	0.850
Observations	6505	6505	6505	6505
Clusters	426	426	426	426

Note: All regressions include households living within 2 km of bombing boundaries. *Income* demonstrates households' income from agricultural and non-agricultural activities. *Expenditure* indicates households' total expenditure, including food, non-food, and housing expenses. *Durable* represents the total value of durable goods (TV, bicycle, radio, phone, etc.). All of these variables are transformed $\ln(\text{Cambodian riel}+10000)$. *Food Secure* is a dummy variable equal to 1 if households report that they do not go hungry. All regressions use a local linear polynomial in spatial coordinates with a triangular kernel weight and control for 50x50km grid fixed effects, province fixed effects, distance to the capital, distance to Vietnam borders, and other pre-bombing characteristics. Standard errors reported in parentheses are clustered at the village level. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

To test these implications, we now exploit data from the Cambodian Socio-Economic Survey (CSES) to investigate households' economic conditions, food vulnerability, agricultural productivity, and risk-taking behaviors. In Table 2.7, we present effects on economic conditions and quality of life. Consistent with our expectations on a *risk* channel, households in bombed, soft soil areas experience significantly lower income, lower value of durable assets, and lower food security (Panel B).²⁵ In contrast, in hard soil areas with low UXO risk, we find that households exhibit higher income, higher value of durable assets, and higher food security, consistent with a *trigger* channel leading to post-bombing revitalization and development (Panel C).

In Table 2.8, we turn to agricultural productivity and loans taken for agricultural activities as a measure of risk taking. We construct measures of cultivated land area and crop revenue using field-level information reported by households on all fields they cultivate and the revenues generated from each field. The unit of analysis is at the field level, and in columns (1-2), we find that both cultivated area and crop revenue are lower where bombs fell on soft soil. The magnitudes are large, with bombed soft-soil areas exhibiting 0.381 log points less cultivated area and 0.336 log points lower revenues relative to unbombed soft-soil areas, corresponding to reductions of approximately 32% and 29%, respectively. In hard soil areas, we see no effects, suggesting their higher incomes (Table 2.7) are driven by non-agricultural activities.

Turning to loans in columns (3-4), we find that households in bombed, soft soil locations are less likely to take out a loan, and when they do take out a loan, they take out considerably smaller amounts. Our results on loan taking are only significant at the 10% level (Panel B) within soft soil locations, but they suggest that these households have less leverage to take agricultural loans and are less likely to take on new risk. Again, we see no evidence that hard soil locations take out agricultural loans, consistent with the incomes of households here being driven by non-agricultural practices.

Taken altogether, our results provide strong evidence of two distinct channels. In soft soil areas, the *risk* channel operates through bombing and persistent UXO threats that discourage cultivation, investment, and borrowing, leading to persistent declines in

²⁵Lower food security indicates a higher likelihood of food shortages.

Table 2.8: Field Productivity and Risk Taking Behaviour

	Dependent variable is:			
	Field Productivity		Risk Taking	
	<i>Unit: Agricultural Fields</i>		<i>Unit: Households</i>	
	Ln(Area)	Ln(Revenue)	Loan Taken	Ln(Loan Amount)
	(1)	(2)	(3)	(4)
<i>Panel A: All observations</i>				
Bombing	-0.058 (0.075)	-0.113 (0.092)	0.025 (0.031)	0.079 (0.169)
Bombing $\times$ Soft Soil	-0.255** (0.120)	-0.222 (0.165)	-0.073 (0.047)	-0.367 (0.261)
Mean	8.280	13.298	0.262	10.521
Observations	14228	14215	3740	3740
Clusters	610	610	654	654
<i>Panel B: Soft soil (1962)</i>				
Bombing	-0.381*** (0.099)	-0.336*** (0.129)	-0.072* (0.038)	-0.434* (0.222)
Mean	8.394	13.469	0.296	10.764
Observations	5158	5150	1550	1550
Clusters	239	239	269	269
<i>Panel C: Hard soil (1962)</i>				
Bombing	-0.085 (0.072)	-0.119 (0.084)	0.043 (0.030)	0.147 (0.159)
Mean	8.215	13.202	0.238	10.350
Observations	9070	9065	2188	2188
Clusters	371	371	383	383

Note: All regressions include observations living within 2 km of bombing boundaries. The unit of analysis for the first 2 columns is fields (agricultural land). *Area Cultivated* represents the size of the cultivated area (m²). *Area Cultivated* is transformed $\ln(\text{Area Cultivated} + 1)$. *Crop Revenue* indicates total revenue from the field: $\text{Revenue} = (\text{QuantityHarvested} - \text{PostHarvestLoss}) \times \text{SalesPrice}$. *Crop Revenue* is also log transformed $\ln(\text{Cambodian riel} + 10000)$. The unit of analysis for the last 2 columns is CSES households. *Loan Taken* is a dummy variable indicating whether a household takes a loan for agricultural activities. *Agri Loan* denotes the amount of loan taken for agricultural activities. All regressions use a local linear polynomial in spatial coordinates with a triangular kernel weight and control for 50x50km grid fixed effects, province fixed effects, distance to the capital, distance to Vietnam borders, and other pre-bombing characteristics. Standard errors reported in parentheses are clustered at the village level. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

productivity and household economic outcomes and, ultimately, constraining economic growth and trapping populations in cycles of poverty consistent with evidence in Lin (2022); Riano and Valencia Caicedo (2024); Yamada and Yamada (2021). By contrast, in hard soil areas where UXO risk is lower, bombing appears to activate a *trigger*

channel, with households improving their incomes and experiencing better food security, consistent with post-bombing revitalization and long-run economic recovery.

2.6 Conclusion

Why do communities exposed to aerial bombing experience diverging long-run outcomes? We demonstrate in Cambodia that conflict generates distinct mechanisms that either *trigger* recovery or leave persistent *risk*, depending on the likelihood of bomb detonation failure. Using a spatial regression discontinuity design and a wide range of geocoded datasets, we exploit soil conditions, which are orthogonal to bombing locations but predictive of detonation rates, to separate short-run destruction effects from long-run hazard effects. More than three decades after the end of bombing in Cambodia, bombed soft-soil areas face higher rates of UXO incidents, significantly worse health outcomes, and weaker economic development, reflecting a persistent *risk* channel that undermines well-being. In contrast, bombed hard-soil locations, where bombs were more likely to detonate, exhibit fewer UXO incidents, better health, and stronger economic development, consistent with a *trigger* channel where destruction did not translate into persistent hazards and allowed for long-run recovery. Overall, our paper contributes to the growing literature on the long-run costs of UXO in war-affected countries (Guo, 2020; Lin, 2022; Nguyen et al., 2024; Riano and Valencia Caicedo, 2024), the enduring impacts of conflict on health and well-being (Palmer et al., 2019; Yamashita and Trinh, 2022), and broader research examining how historical shocks shape long-term development (Dell and Olken, 2020; Dell et al., 2018).

Our findings carry important implications for countries affected by past conflicts, highlighting the power of post-war recovery efforts and strategic investments in the aftermath of conflict. In Cambodia, areas free from persistent conflict hazards have achieved strong recovery and improvements in well-being and economic outcomes, whereas regions where UXO remains a threat continue to experience lasting setbacks. This underscores the importance of UXO clearance for long-term recovery, as regions with war remnants are more likely to be left behind and unable to develop. Land-

mine and UXO clearance not only reduces morbidity and mortality but also brings substantial socioeconomic gains to impacted communities (Frost et al., 2017). However, clearance efforts must prioritize critical areas, such as key transportation routes and trade corridors, like in the case of Mozambique, to be cost-effective and yield the greatest benefits (Chiovelli et al., 2025). Future research should therefore focus on understanding the intricate relationship between post-conflict development and UXO, and on investigating how UXO clearance can be integrated into broader development initiatives to maximize its impact.

Chapter 3

The One-Child Policy in China and its Intergenerational Effects on Health

3.1 Introduction

The One-Child Policy is a family planning policy implemented nationwide in China from late 1979 to 2016 to control the country's rapid population growth. The policy strictly limited the number of children in each family to a minimum, except for those from ethnic minorities or living in rural areas. More than three decades of implementing the policy significantly changed the social dynamics and family structures in China (Settles et al., 2012; Zhang, 2017). Low fertility rates and reduced family size have led to population aging and increasing pressures on elderly care (Bai and Lei, 2020; Nie and Zhao, 2023). However, findings indicate that reducing sibling size would prompt increased investment in children, subsequently leading to improved education and health outcomes (e.g. Cáceres-Delpiano (2006); Lee (2008); Rosenzweig and Zhang (2009); Zhong (2017)). If the parental generation receives significant investment and wealth as a result of family planning policies, we expect them to allocate more resources to their offspring. Furthermore, improved health outcomes of parents can be passed on to

the next generation, resulting in better health observed among their children (Emanuel et al., 1992; Eriksson et al., 2005; Strauss and Thomas, 2007).

While existing literature primarily examines the impact of sibling and family size, the direct intergenerational consequences of policy enforcement on health remain under-explored. Our paper addresses this gap by investigating health outcomes for children whose parents were subject to the One-Child Policy. By focusing on the urban Han Chinese population, where enforcement was most stringent, we provide a clearer understanding of how family planning policies reverberate across generations.

We leverage data from the China Family Panel Studies (CFPS), a nationally representative biennial longitudinal survey conducted by the Institute of Social Science Survey (ISSS) at Peking University since 2010. The CFPS covers both economic and non-economic aspects of the Chinese population, providing rich data on economic activities, education, family dynamics, and health (Xie and Hu, 2014). We follow Anderson (2008) to construct an aggregated *health index* comprising the following components: absence of illness, no hospitalization, self-rated health, and interviewer-rated health. We also develop a separate mental health indicator – *distress* – based on the Kessler Psychological Distress Scale (K6) and the Center for Epidemiologic Studies Depression Scale (CES-D).¹ We employ a reduced form regression discontinuity design (RDD), exploiting the policy cut-off in 1980Q1 that creates a discontinuity in the number of single-kid families,² thereby precisely isolating the policy’s local average treatment effect (LATE) on the next generation’s health outcomes. We show that our design passes multiple checks around the core RDD assumptions. Given the significantly smaller sample size for father data and numerous evidence showing that urban Han mothers benefited from the demographic pattern created by the one-child policy (Fong, 2002; Veeck et al., 2003; Zhang, 2019), we focus primarily on the effects of the policy from the mothers’ side.

We begin by examining the policy’s effects on the first generation – mothers (Section 3.5.1). We find that the policy significantly changes mothers’ sibling composition,

¹Detailed description of the data and our selection of outcome variables are presented in Section 3.3.

²We discuss the rationale for selecting the 1980Q1 policy cut-off in Section 3.2.

improves their health, and enhances their socioeconomic status in adulthood, consistent with long-term gains in human capital and economic outcomes among affected mothers (Hu and Yang, 2025; Zhang, 2019). In particular, policy-affected mothers are 7.9 percentage points more likely to be single kids, equal to 78% compared to the mean, and less likely to have been born into a pre-existing sibling group. Their *health index* significantly increases by 18% of a standard deviation, although we do not find any effects on mental health. In addition, they have lower fertility, higher college participation, higher income, and a greater likelihood of holding leadership positions. We expect these benefits may carry over to the next generation, improving child health through better maternal health, greater economic resources, and increased parental investments in nutrition, healthcare, and the home environment.

We then empirically investigate the intergenerational health effects of the policy on the second generation – children (Section 3.5.2). Our baseline results show that children born to policy-affected mothers exhibit significant improvements in both physical and mental health. Specifically, their *health index* improves by about 15% of a standard deviation. They also experience lower levels of mental distress, with the probability of having distress reduced by 9.3 percentage points, equivalent to over 50% compared to the mean.³ We conduct a wide range of sensitivity checks to show that our baseline estimates remain robust. Our results, thus, provide evidence for the intergenerational transmission of health and household characteristics across generations. A lower fertility rate, which is transmitted from grandparents to parents (Kolk, 2014; Murphy and Knudsen, 2002), leads to increased human capital investment per child, supporting our narratives on child health investment. Additionally, mothers affected by the policy exhibit better health outcomes, which can be passed on to their children (Emanuel et al., 1992; Eriksson et al., 2005).

Finally, in Section 3.5.3, we empirically investigate other potential channels for the intergenerational health effects of the policy. First, greater investment in children’s health, as family size becomes smaller, can explain better health in children. This

³We find similar but less statistically efficient results when examining children whose fathers were born after 1980 and affected by the policy. Given the smaller sample size and narrower bandwidth in the fathers’ dataset, we interpret these results with caution.

is consistent with the quantity-quality trade-off formulated by Becker (1960), which illustrates a negative correlation between family size and the resources allocated to each child. Second, parenting practices and parent-child interactions show that policy-affected parents tend to be “authoritative”, characterized by high responsiveness combined with appropriate supervision (Baumrind et al., 2010; Doepke and Zilibotti, 2017). The balance of warmth, support, and supervision from parents fosters a home environment that promotes children’s development and improves their mental health. This aligns with literature on parental demands (Lo et al., 2020; Soysa and Weiss, 2014; Wong et al., 2019) and parental responsiveness (Davidov and Grusec, 2006; Miller-Slough et al., 2018), especially in the Chinese context, where children are the only child (Liu et al., 2010; Lu and Chang, 2013).

Related Literature. Our research question is relevant to several strands of literature. First, our paper relates to the theoretical basis for the quantity-quality trade-off in children. This framework was first theorized by Becker (1960), who treated children as a consumption good, requiring a family to decide not only on the number of children but also on the corresponding expenditure allocated to them. This trade-off occurs because parents have to spread their time and resources more thinly as the number of children increases (Hanushek, 1992). The literature presents mixed empirical evidence on the quantity-quality trade-off. Numerous studies have found a negative association between family size and investment in children (Chen, 2020; Li et al., 2008; Ponzo and Scoppa, 2022; Rosenzweig and Zhang, 2009). However, several others have observed no evidence of the quantity-quality trade-off (Angrist et al., 2010; Diaz and Fiel, 2021) or even a positive relationship (Gomes, 1984; Lao and Lin, 2022; Qian, 2009).

In terms of health, several studies provide evidence of a negative association between family size and health. Liu (2014); Zhong (2017) investigate the one-child policy in China and find negative impacts of family size on child height. A similar study conducted by Liang and Gibson (2018) also shows that each additional sibling reduces a child’s nutrient intake by about 10% to 20% of the recommended level. Nevertheless, Lordan and Frijters (2013) utilizing data from the Young Lives Project in Peru find

that family size is negatively associated with child height for unplanned births but positively associated for planned births. Datar (2017) investigates the relationship between family size and obesity in the US and provides evidence that children with siblings have lower BMI and are less likely to be obese, thanks to healthier diets. Regarding subjective well-being, several studies find that being an only child negatively impacts self-reported psychological health (Wu, 2014; Zeng et al., 2020). Cameron et al. (2013), leveraging the one-child policy in China, provide evidence that these “little emperors” are less trustworthy and more pessimistic. However, Liu et al. (2010); Rao et al. (2024) compare single-kid and multiple-kid families and show that single kids reported lower psychological distress and fewer mental health problems, attributed to higher parental responsiveness.

Our paper, however, extends beyond the conventional quantity-quality trade-off, aiming to explore this trade-off at the national scale. The policy is estimated to account for at least 30% of the increase in aggregate savings, as fertility restrictions diminished anticipated old-age support, motivating parents to save more and allocate greater resources toward their children’s education (Choukhmane et al., 2023). The first generation affected by the policy, particularly in urban areas, benefited from concentrated investments in education and health (Zhang, 2019), resulting in higher educational attainment and improved health, especially for women (Fong, 2002; Huang et al., 2016a; Rao et al., 2024). Additionally, the policy led to shifts in social perceptions on traditional patriarchy (Shi, 2017), promoted female empowerment (Huang et al., 2021) and strengthened parent-child relationships (Short et al., 2001). All of these factors can affect the second generation, who are not directly affected by the policy.

In addition, our paper contributes to the literature on the intergenerational transmission of health and household characteristics across generations. Studies consistently demonstrate modest yet persistent effects in the transmission of parents’ fertility patterns to their children (Kolk, 2014). Another body of work focuses on intergenerational health transmission, grounded in the fetal origins hypothesis (Barker, 1990), which emphasizes the lasting influence of early life (in-utero) conditions on later health and economic outcomes (Emanuel et al., 1992; Eriksson et al., 2005). Numerous studies also

measure intergenerational health association, in which children inherit parents' health capital, with an estimated persistence of about 0.2 in the United Kingdom (Bencsik et al., 2023), 0.3 in the United States (Halliday et al., 2020) and between 14 and 38% in Denmark (Andersen, 2021).

Our primary contribution, therefore, is to provide new causal evidence on the intergenerational effects of family size and family-planning policies on subsequent generations. We leverage data from the China Family Panel Studies (CFPS), a nationally representative and one of the most comprehensive social panel surveys conducted in China (Xie and Hu, 2014), ensuring high reliability and coverage. We examine the spillover effects on both the physical and mental health of children whose parents were affected by the policy. We also explore several mechanisms from the CFPS data to explain our results, thereby contributing to the literature on quantity-quality trade-offs and intergenerational effects. In addition, we further address the question of parenting behavior and parent-child relationships in modern Chinese families.

The paper is structured as follows: Section 3.2 provides historical background and introduces the one-child policy in China; Section 3.3 describes the data and sample; Section 3.4 illustrates empirical strategy and identification assumptions; Section 3.5 presents the main findings of the paper, including the policy effects on the first-generation (mothers), intergenerational health effects on children, and the intra-household mechanisms that help explain our results; Section 3.6 concludes.

3.2 Background

Since beginning its industrialization in 1949, China has experienced substantial population growth, with the belief that this growth would contribute to the nation's development (Zhu, 2012). However, persistent poverty and high fertility rates have raised concerns about overpopulation (De Silva and Tenreyro, 2017). From 1970 onwards, citizens were encouraged to delay their marriages. In the early 1970s, the state introduced a series of birth planning policies. In 1978, the authorities began encouraging one-child families, and in early 1979 they announced their intention to implement this policy,

which later became national policy. Introduced in 1979, the One-Child Policy was a strict family planning policy designed to curb the country's rapid population growth (Wang et al., 2016), and it was formally written into the constitution in 1982. The policy primarily affected the Han Chinese, who make up 92% of the population (Huang et al., 2016b). From late 1979 onwards, in principle, a couple was allowed only one child, except in some rural ethnic minority areas such as Xinjiang, Yunnan, Ningxia, and Qinghai.

The evolution of the policy had several phases (Greenhalgh, 2008; Scharping, 2013). It was first announced in the second half of 1978 as "Best is one, at most two; eliminate third births". In December 1979, the National Population and Family Planning Commission announced the policy as "Best is one". From February 1980, this quickly changed to the "One for all" policy. Over time, however, various exceptions were made, and the policy was revised in early 1989 to "One child with exceptions for rural couples with only a daughter".

In 2016, China officially relaxed its One-Child Policy, marking a significant shift in its approach to population control. The policy change, which had allowed families to have two children since 2015 with certain modifications, reflected mounting concerns about the policy's adverse demographic and socio-economic impacts. The relaxation aimed to address issues such as an aging population, a shrinking workforce, and gender imbalances. Nevertheless, many urban families still choose to have only one child due to financial and social pressures, even after the policy relaxation (Qian and Jin, 2024). The long-term effects of this policy change on family dynamics, economic stability and population health remain subjects of ongoing policy debate.

Social consequences of the one-child policy (OCP). The One-Child Policy in China has led to a significant decline in the fertility rate, which was already decreasing due to earlier family planning campaigns in the 1970s (Feng et al., 2014). The average family size fell from 4.8 in the early 1970s to 3.1 by 2010 (Aird, 1983; Census Office of the State Council, 2020). Single-child families became prevalent, particularly in urban areas, where approximately 80 percent of families consisted of three members

by the end of the 20th century (Tu, 2016). Other direct outcomes included a skewed male-to-female ratio and higher fertility rates in rural areas than in urban areas, which disadvantaged rural families economically (Ebenstein, 2010). Despite criticism, the policy often benefited urban daughters, who received more family resources and achieved higher educational attainment and empowerment (Huang et al., 2016a, 2021). Long-term impacts include accelerated population aging, increased pressure on elderly care, and the rise of "empty nest" families in urban areas (Bai and Lei, 2020; Nie and Zhao, 2023; Zhu and Walker, 2021).

Policy effects on the first generation. Those subjected to the policy as the first generation experienced notable benefits, particularly in urban areas. Families could focus their resources on their single child's education and health (Zhang, 2019). This focus led to higher educational attainment among females and improved overall health outcomes (Huang et al., 2016a; Rao et al., 2024). Women born under the One-Child Policy achieved higher levels of education, and stricter early-life fertility restrictions increased female empowerment, as evidenced by an increase in female-headed households (Huang et al., 2021).

The policy also brought about qualitative changes in family dynamics. These changes included simplified family structures, reduced patriarchy in families with only daughters, and greater individual choice regarding family living arrangements and childbearing (Shi, 2017). Additionally, increased parental involvement in childcare has improved parent-child interactions (Short et al., 2001). Consequently, this generation enjoys higher income levels and reduced overall stress. The persistence of intergenerational income in urban China highlights the policy's lasting economic impact (Yi, 2016).

Policy cut-off in this paper. We examine the introduction of the policy during the 1979-1980 period to identify the policy cut-off for our study. Before the policy was officially implemented in China, the government had imposed restrictions on the number of children a couple could have. From 1977 to 1978, urban and rural couples were required to limit their family size to two children (Hardee-Cleaveland and Banister,

1988). Several intentions for the universal One-Child Policy were introduced in early 1979, and the “Best is One” policy was officially announced in December 1979. The “One for all” policy was quickly adopted in February 1980, clearly stating that every couple could have only one child. Later, on September 25, 1980, the Chinese Communist Party’s Central Committee issued a public letter urging all party members and the Communist Youth League to adhere to the one-child policy. This date is often mentioned as the policy’s “official” start date (Scharping, 2013). The revised 1980 Marriage Law, ratified during the Third Session of the National People’s Congress on September 10, 1980, also explicitly mandated birth control for all couples (Hardee-Cleaveland and Banister, 1988; Hare-Mustin, 1982; Santana Cooney et al., 1991). Strict fines for violating the one-child policy were imposed nationwide in January 1980 (Santana Cooney et al., 1991). During this period, abortions were required in several provinces, even in the second and third trimesters of pregnancy. The number of induced abortions increased sharply in 1979 and rose even higher in subsequent years (Hardee-Cleaveland and Banister, 1988). We expect to see a sharp increase in the proportion of single-child births in the first quarter of 1980, according to our data structure, which we will discuss later. Although official announcements and legislation related to the one-child policy and birth control were issued in September 1980, the policy was strictly enforced beginning in 1980 with rigorous measures such as mandatory abortions and birth control practices. Additionally, beginning in 1980, penalties were imposed on women who had a second child without official permission (Hardee-Cleaveland and Banister, 1988), confirming that it was difficult for Han mothers living in urban areas to have another child. We later verify this cut-off date by illustrating the discontinuity in the ratio of individuals with no siblings at our predicted cut-off point (the first quarter of 1980) within our dataset.

3.3 Data and Descriptive Statistics

3.3.1 Data

We use the China Family Panel Studies (CFPS) at the Institute of Social Science Survey (ISSS) at Peking University, China.⁴ The CFPS is a nationally representative, biennial, longitudinal survey of Chinese families and individuals that began in 2010. For our analysis, the CFPS dataset has several key features. First, it allows us to identify the intra-household relationships, which are necessary for our empirical strategy that uses parents' birth information. Second, in the first wave (2010), adults were asked to provide information about their parents and their siblings, providing valuable data for studying sibling composition and policy effects. Third, with six waves up to 2020, the CFPS provides national-level information on family dynamics and health outcomes for our study.

Health index. To measure children's physical health status, we follow Anderson (2008) to generate a summary *health index*⁵, which is a weighted mean of the following standardized components:

1. Whether a child was ever sick last month: CFPS defines sickness as a condition in which a child experiences physical discomfort and requires treatment (medicines or other treatments). We recode this variable (*"No Sick"*) so that higher values indicate a better health status, meaning the child did not experience illness last month.
2. Whether a child was ever hospitalized last month: CFPS defines hospitalization as a child staying overnight in a hospital for at least one night due to disease or accidental injury. This variable is also recoded so that a value of 1 denotes better

⁴The data are from the China Family Panel Studies (CFPS), funded by the 985 Program of Peking University and carried out by the Institute of Social Science Survey of Peking University.

⁵Summary index tests can reduce measurement error arising from idiosyncratic noise in individual outcome measures by aggregating information across several related outcomes into a single composite index. We first recode each variable so that a positive value always indicates a better health outcome. We then standardise each variable and calculate the weighted average. The weight is generated by the inverse of the covariance matrix of the standardised outcomes to maximize the amount of information captured by the index (Anderson, 2008).

health (“*No Hospital*”). This ensures directional consistency across component outcomes of our *health index*.

3. Children’s self-rated health: The survey asked respondents to rate their health status on a scale from 1 to 5, where 1 indicates healthy and 5 indicates very unhealthy. Only those aged 10 and above answered this question. We recode this into a binary variable with 1 for healthy and 0 for all other ratings.
4. Interviewer-observed child health: The interviewer recorded their assessment of the presented respondent’s health, choosing from 1 (worst) to 7 (best). We also recode this variable as a binary variable, with 1 for an observed health rating of 4 or higher and 0 otherwise.

Similarly, we construct a parental *health index* based on the following indicators: no discomfort, no chronic disease, no hospitalization, self-rated health, and observed health.⁶

Distress. We incorporate two psychological scales – the Kessler Psychological Distress Scale (K6) (Kessler et al., 2002) and the Center for Epidemiologic Studies Depression Scale (CES-D) (Radloff, 1977) – to measure children’s and parents’ mental health, as different scales are available in different waves. A detailed description of these scales can be found in Appendix B.1. We construct a binary variable called “*distress*”, which is coded as 1 if a child shows signs of mental distress, i.e. if the K6 score is greater than or equal to 5 (Prochaska et al., 2012), the CES-D8 score is greater than or equal to 7, or the CES-D20 score is greater than or equal to 16 (Bi et al., 2023). In Appendix B.1, we also analyze comparable items between the K6 and CES-D scales.

Mechanisms. We examine the mechanisms through which the effects of the policy on the first generation (parents) could be passed on to the second generation (their children). First, we examine the policy effects on parents’ health, number of siblings, fertility choices and socioeconomic status.⁷ Second, we examine family-level mechanisms, including income and expenditure, with a focus on expenditure on children.

⁶Information on *sickness* is not available in the parents’ dataset. However, data on *discomfort* and *chronic disease* are available.

⁷See Section 3.5.1.

These measures reflect the resources allocated to support children’s well-being and shed light on the broader context in which children are raised and develop. Lastly, we examine the interactions between parents and children, which can affect children’s mental health. The quality and nature of these interactions are crucial in determining children’s emotional and psychological well-being. Through studying these mechanisms, we aim to understand how policy effects are transmitted across generations.⁸

3.3.2 Sample selection and summary statistics

We compile data from six waves of the China Family Panel Studies (CFPS) based on information from child questionnaires. Then, we create a repeated cross-sectional dataset and merge it with data on parents and families from the corresponding waves. We first focus on a sample of parents born before or after five years from the policy cut-off in 1980. We then drop observations missing key demographic characteristics, such as children’s age, gender, and rural or urban residence; the birth information of both parents; and family size (about 1% are dropped at this stage). Next, we split the dataset into two: one containing information about mothers and the other containing information about fathers. For each dataset, we exclude observations from provinces with fewer than 50 mothers or fathers (five provinces for both). Next, due to the policy focus, we restrict the sample to Han ethnicity (87.11% of mothers and 88.01% of fathers) and to urban parents (51.56% of mothers and 53.79% of fathers after restricting to Han ethnicity).

These steps ensure that we have complete information on the birth years and months of urban Han parents. Due to the limited number of observations resulting from the specific focus on urban Han parents, we base our analysis on quarterly birth data for these parents to ensure sufficient statistical power. Then, we exclude those with missing information on children’s health outcomes at each wave.

⁸See Section 3.5.3.

Table 3.1: Descriptive statistics

	All observations				Within 13-quarter bandwidth			
	All	Control	Treated	Diff	All	Control	Treated	Diff
A. Health Outcomes								
Health Index	0.02 (0.40)	0.05 (0.47)	-0.01 (0.31)	0.06*** (10.05)	0.01 (0.39)	0.01 (0.42)	0.00 (0.36)	0.01 (1.07)
No sickness last month	0.72 (0.45)	0.77 (0.42)	0.68 (0.47)	0.09*** (12.54)	0.73 (0.44)	0.74 (0.44)	0.73 (0.45)	0.01 (1.03)
Not hospitalized last month	0.78 (0.41)	0.83 (0.38)	0.74 (0.44)	0.09*** (13.53)	0.79 (0.40)	0.80 (0.40)	0.79 (0.41)	0.01 (1.12)
Self-rated health (healthy = 1)	0.44 (0.50)	0.45 (0.50)	0.40 (0.49)	0.05** (3.26)	0.39 (0.49)	0.39 (0.49)	0.38 (0.49)	0.01 (0.31)
Interviewer-observed health (≥ 4 on 1-7 scale)	0.98 (0.14)	0.98 (0.14)	0.98 (0.15)	0.00 (1.02)	0.98 (0.15)	0.98 (0.13)	0.97 (0.16)	0.01 (1.37)
Distress indicator based on K6 and CESD	0.18 (0.39)	0.19 (0.39)	0.16 (0.37)	0.03* (2.41)	0.17 (0.38)	0.19 (0.39)	0.14 (0.35)	0.04** (2.67)
B. Demographics								
Child's age	7.90 (4.53)	10.22 (4.05)	5.88 (3.93)	4.34*** (68.83)	8.21 (4.23)	9.23 (4.16)	7.29 (4.08)	1.93*** (17.42)
Child's gender (female = 1)	0.47 (0.50)	0.46 (0.50)	0.47 (0.50)	-0.01 (-1.29)	0.48 (0.50)	0.47 (0.50)	0.49 (0.50)	-0.03* (-1.98)
Child's birthyear	2007 (5.32)	2003 (4.40)	2010 (4.09)	-6.44*** (-95.57)	2006 (4.03)	2005 (3.90)	2008 (3.72)	-2.67*** (-25.99)
Mother's age	34.89 (6.23)	39.48 (4.59)	30.75 (4.33)	8.73*** (121.98)	34.87 (3.62)	36.35 (3.33)	33.51 (3.33)	2.84*** (31.30)
Mother's birthyear	1980 (6.70)	1974 (4.01)	1985 (3.92)	-10.83*** (-172.76)	1980 (2.03)	1978 (1.01)	1981 (1.03)	-3.52*** (-128.21)
Father's age	36.83 (6.38)	41.01 (5.15)	33.18 (4.94)	7.83*** (91.87)	36.90 (4.51)	38.19 (4.38)	35.71 (4.30)	2.47*** (20.03)
Father's birthyear	1978 (6.80)	1973 (4.74)	1983 (4.64)	-9.88*** (-127.61)	1978 (3.57)	1976 (3.30)	1979 (3.07)	-3.24*** (-36.29)
Family size	5.08 (1.91)	4.62 (1.62)	5.48 (2.04)	-0.86*** (-29.71)	5.06 (1.92)	4.94 (1.91)	5.17 (1.93)	-0.23*** (-4.48)
C. Mother's characteristics								
Health Index	0.02 (0.42)	0.03 (0.46)	0.02 (0.39)	0.01 (1.25)	0.04 (0.39)	0.03 (0.41)	0.05 (0.37)	-0.02 (-1.64)
Distress indicator based on K6 and CESD	0.18 (0.39)	0.19 (0.39)	0.16 (0.37)	0.03* (2.41)	0.17 (0.38)	0.19 (0.39)	0.14 (0.35)	0.04** (2.67)
No siblings	0.08 (0.26)	0.05 (0.21)	0.12 (0.32)	-0.07*** (-12.30)	0.10 (0.30)	0.07 (0.26)	0.13 (0.34)	-0.06*** (-6.33)
Number of children	1.75 (0.79)	1.79 (0.82)	1.72 (0.75)	0.07*** (5.91)	1.76 (0.87)	1.76 (0.90)	1.77 (0.85)	-0.01 (-0.48)
College+	0.17 (0.38)	0.14 (0.34)	0.20 (0.40)	-0.07*** (-11.26)	0.21 (0.41)	0.18 (0.38)	0.24 (0.43)	-0.06*** (-5.48)
Ln(Yearly Income)	5.44 (4.86)	5.17 (4.78)	5.72 (4.94)	-0.54*** (-6.41)	5.46 (4.93)	5.23 (4.90)	5.69 (4.95)	-0.46** (-3.14)
Social Position (Leading Cadres)	0.01 (0.09)	0.01 (0.11)	0.01 (0.07)	0.01*** (4.08)	0.01 (0.11)	0.01 (0.11)	0.01 (0.10)	0.00 (0.21)
D. Grandparents' characteristics								
Grandfather's age	61.42 (9.35)	66.10 (8.05)	54.33 (6.19)	11.77*** (83.01)	59.30 (6.17)	61.59 (6.15)	56.88 (5.19)	4.71*** (26.37)
Grandmother's age	62.71 (8.71)	66.71 (7.71)	56.53 (6.19)	10.18*** (73.52)	60.93 (6.04)	62.64 (6.13)	59.14 (5.40)	3.50*** (19.18)
Literacy (grandfather)	0.75 (0.43)	0.70 (0.46)	0.82 (0.38)	-0.12*** (-14.82)	0.80 (0.40)	0.76 (0.43)	0.84 (0.36)	-0.08*** (-6.67)
Literacy (grandmother)	0.55 (0.50)	0.48 (0.50)	0.65 (0.48)	-0.17*** (-17.67)	0.60 (0.49)	0.54 (0.50)	0.66 (0.47)	-0.13*** (-8.28)
Unemployment (grandfather)	0.14 (0.35)	0.15 (0.36)	0.13 (0.33)	0.03*** (3.87)	0.12 (0.32)	0.13 (0.34)	0.11 (0.31)	0.02* (2.08)
Unemployment (grandmother)	0.24 (0.43)	0.25 (0.44)	0.22 (0.42)	0.03*** (3.52)	0.22 (0.41)	0.23 (0.42)	0.20 (0.40)	0.03* (2.27)
Either of grandparents is communist	0.11 (0.31)	0.15 (0.36)	0.07 (0.26)	0.08*** (16.23)	0.14 (0.35)	0.14 (0.35)	0.14 (0.35)	0.00 (0.15)
Observations	16105	7514	8591	16105	5533	2635	2898	5533

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses. "All" means the whole sample, "Treated" means mothers have no siblings, and "Control" means mothers have siblings. "Diff" shows the mean difference between the treated and control groups. The scale for interviewer-observed child health ranges from 1 (worst) to 7 (best).

Table 3.1 presents summary statistics for our sample, focusing on children and mothers. Most of the statistically significant differences between the control and treatment groups are related to age. This is due to the nature of the policy, which makes those born later, i.e. younger, more likely to be treated.

We further demonstrate the validity of the policy cut-off date in our data. Figure 3.1a shows the proportion of Han urban adults born within four years (16 quarters) of the 1980Q1 policy cut-off who have no siblings. As mentioned previously, adult sibling information is only available in the first wave, which is from 2010. We use the 2010 adult data with the sampling weights provided by the CFPS to ensure national representativeness. The graph shows the simple overall national average of the no-sibling ratio in each quarter. It presents an upward general trend in the proportion of adults without siblings immediately following the policy, and suggests a small jump in this ratio in 1980Q1 among Han urban residents. Looking at females and males separately in Figures 3.1b and 3.1c, we see a clear jump for both genders in 1980Q1. The no-sibling ratio for females starts very low before the policy and remains fairly stable afterwards, showing an upward trend. For males, the no-sibling ratio is stable before the policy and increases afterwards. As mentioned previously, there were policy exceptions, such as allowing a second child if the first was a daughter. This can partly explain the different patterns observed between men and women.

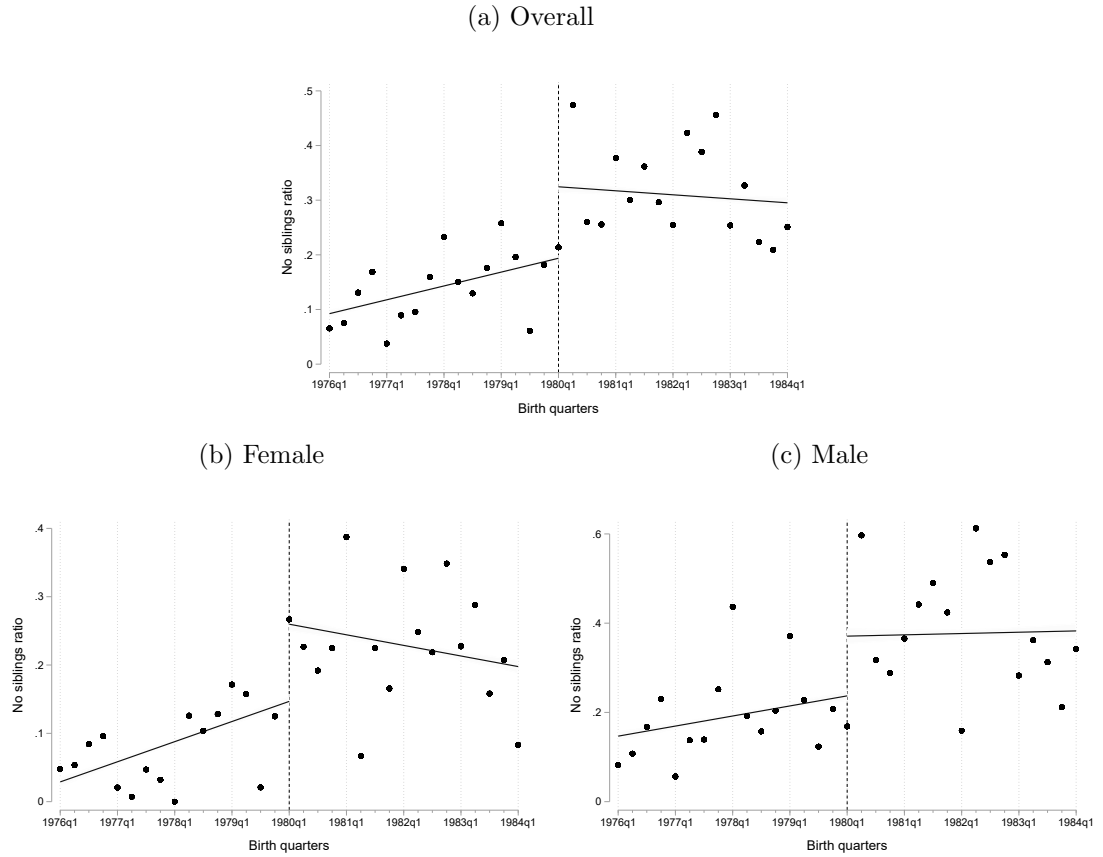
3.4 Empirical Strategy

3.4.1 Main specifications

To explore the spillovers of the policy, we employ a regression discontinuity in time (RDiT) with the number of quarters between mothers' date of birth and policy date as the running variable. In an RDiT, the running variable plays a crucial role in determining the treatment status when a discontinuity occurs at a specific cut-off point. In our context, this is the first quarter of 1980. However, all other covariates should exhibit smoothness at the cut-off.

We rely on a reduced-form regression discontinuity design (RDD) rather than a

Figure 3.1: Proportion of adults without siblings born within 16 quarters of policy cut-off



Notes: Figure (a) shows the proportion of Han urban adults nationally (using sampling weights provided by the CFPS) born within 16 quarters (4 years) of the policy cut-off with no siblings, while Figures (b) and (c) show the no siblings ratio for females and males, respectively. The policy cut-off is 1980Q1. We use the 2010 CFPS adult data as this is the only wave that provides information on adults' siblings.

fuzzy RDD for several reasons. First, our goal is to identify the effects of exposure to a policy-induced discontinuity at the cut-off, rather than to estimate a local average treatment effect (LATE) of a specific endogenous choice such as family size. In our setting, the One-Child Policy leads to gradual changes in the family environment, with sibling composition as the most direct and observable manifestation. We choose not to combine the RD with an instrumental variables (IV) approach because it is challenging to separate the effects of sibling size from those of sibling gender. As shown in Figure 3.1, we can only see a small jump in the no-sibling ratio because not all families adhered

to the policy. During its implementation, wealthier urban couples were willing to pay fines to have a second child (Burgess and Zhuang, 2002; Li et al., 2008). Moreover, literature has shown that the policy might also have an impact beyond parents' siblings composition. For example, the policy is estimated to explain at least 30% of the rise in aggregate savings, as fertility restrictions reduced expected old-age support, prompting parents to save more and invest more in their children's education (Choukhmane et al., 2023). At the household level, the policy led to significant qualitative changes in family dynamics, including diminishing patriarchal norms in daughter-only households, and greater individual freedom in decisions about living arrangements and childbearing (Shi, 2017). However, these changes at the national level (such as aggregate savings) or at the household level (such as preferences and norms) will not be captured by the RD, because individuals born before the cut-off were most likely exposed to the same changes.

In our specifications, we employ a triangular kernel weighting function in all regressions, with the weight assigned to each observation decreasing as the distance from the cut-off increases. In addition, we estimate the effects from mothers and fathers separately.

For bandwidth selection, Table 3.2 shows the data-driven optimal bandwidths for our two main outcomes – *Health Index* and *Distress* – following Calonico et al. (2020). We select the bandwidth equal to the average of optimal bandwidths generated for these outcomes within the parents' gender. In particular, we choose a bandwidth of 13 quarters when examining the effects from the mothers' side (maternal effects) and a bandwidth of 10 quarters when examining the effects from the fathers' side (paternal effects). Unless specified otherwise, all analyses in our paper will use a 13-quarter bandwidth for maternal effects and a 10-quarter bandwidth for paternal effects. Specifications with other bandwidth choices will be considered in our robustness checks.

Table 3.2: Optimal bandwidths for RDD

	Mother's information	Father's information
	(1)	(2)
Health Index	15.635	9.843
Distress	9.237	10.128

Notes: This table shows the mean square error optimal bandwidths of main outcomes following Calonico et al. (2020) – CCT bandwidths with a local linear polynomial. Standard errors are clustered by mothers' or fathers' birth years. Column (1) presents the optimal bandwidth for each outcome using mothers' information, while column (2) presents the optimal bandwidth for each outcome using fathers' information. The optimal bandwidth using the mother's information is rounded up to 13 quarters, and the optimal bandwidth using the father's information is around 10 quarters.

The regression measuring the direct effect of the policy on the first-generation's (or parents') outcomes takes the following form:

$$Y_i^P = \alpha^P + \beta^P Policy_i + f(quarter_i^P) + \gamma^P \mathbf{X}_i^{GP} + \theta^P \mathbf{Age}_i^P + \lambda^P Province_i^P \times Birthyear_i^P + \tau_t^P + \nu_i^P \quad (3.1)$$

where Y_i^P denotes a parent's outcomes; $Policy_i$ is a dummy variable equal to 1 if the mother's/father's date of birth is from 1980Q1. $f(quarter_i^P)$ is the RD polynomials controlling for the distance from the cut-off in quarters. $\mathbf{X}_i^{GP}$ contains pre-determined demographic and social characteristics of grandparents, including their age, literacy, employment status and whether either of them is a member of the communist party.⁹ $\mathbf{Age}_i^P$ is a non-linear control for the parent's age, including age and age^2 . We also control for the parent's province-birthyear fixed effects. τ_t^P is interview year fixed effects. Standard errors are clustered by parents' year of birth. The causal effect of the policy on parents' outcomes is β^P . However, our main focus is to examine the spill-over effects of the policy on the second generation: children. The regression estimating the

⁹Party members were urged to "take the lead" in the One-Child Policy campaign. In September 1980, the Central Committee of the Chinese Communist Party issued an "Open Letter" to all Party and Youth League members, asking them to lead the way in implementing the policy (Committee, 1984; White, 1990).

intergenerational effects on children's health outcomes takes the form of:

$$Y_i^C = \alpha^C + \beta^C Policy_i + f(quarter_i^P) + \gamma^C \mathbf{X}_i^{GP} + \theta^C \mathbf{Age}_i^P + \delta^C \mathbf{X}_i^C + \lambda^C Province_i^P \times Birthyear_i^P + \zeta^C Province_i^C \times Birthyear_i^C + \tau_i^C + \nu_i^C \quad (3.2)$$

where Y_i^C denotes a child's health outcomes. In addition to the pre-determined characteristics of the grandparents $\mathbf{X}_i^{GP}$ and the parent's non-linear age control $\mathbf{Age}_i^P$, we also control for the characteristics of children $\mathbf{X}_i^C$, in particular, age and gender, and children's province-birthyear fixed effects.

In our RD design, $f(quarter_i^P)$ are RD polynomials controlling for the distance from the cut-off in quarters. We use a local linear RD polynomial in the baseline specifications (Gelman and Imbens, 2019). The local linear polynomial has a function as $f(quarter_i^P) = \eta quarter_i^P + \sigma Policy_i \times quarter_i^P$. The interaction term between the treatment and running variables allows for different functions on either side of the cut-off. In robustness testing, we estimate models that exclude this interaction term and explore specifications with higher-order polynomials.

3.4.2 Identifying assumptions

Continuity assumption. The first assumption to make the RD design valid is the smoothness of all covariates at the cut-off point. We expect that the changes in our potential outcomes are solely due to the treatment initiated at the cut-off point. No other changes or discontinuities occur at the policy cut-off. Since children are considered treated if their parents were born after the cut-off policy date, this assumption is only satisfied when all other relevant covariates related to parents' birth date, in this case, grandparents' characteristics, are continuous in 1980Q1.

We conduct a balance check on a list of predetermined characteristics of both maternal and paternal grandparents using the main specifications and controlling for the time trend in grandparents' birth year, since younger people are naturally more likely to be treated by the policy. These characteristics include birth year, literacy, employment status, and whether either grandparent is a member of the Communist Party.

Table 3.3 shows evidence that our design satisfies the continuity assumption. Across all specifications, we do not observe discontinuities of grandparents' pre-determined characteristics at the cut-off quarter.¹⁰

Table 3.3: Pre-determined characteristics of mothers' parents

	Dependent variable is:						
	Maternal grandfather			Maternal grandmother			Either
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Age	Literacy	No Work	Age	Literacy	No Work	Communist
Policy	-0.000 (0.000)	-0.068 (0.061)	0.012 (0.030)	-0.000 (0.000)	-0.006 (0.022)	0.008 (0.024)	0.021 (0.018)
Mean	59.208	0.816	0.062	60.951	0.621	0.169	0.197
Observations	3,555	3,555	3,555	3,555	3,555	3,555	3,555
R^2	1.000	0.182	0.140	1.000	0.229	0.156	0.142

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. The policy cut-off is 1980Q1. All regressions include mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' and mothers' province-birthyear fixed effects. All regressions include the interaction between treatment and running variables. Columns (1)-(3) show the characteristics of mothers' fathers, and columns (4)-(6) show the characteristics of mothers' mothers.

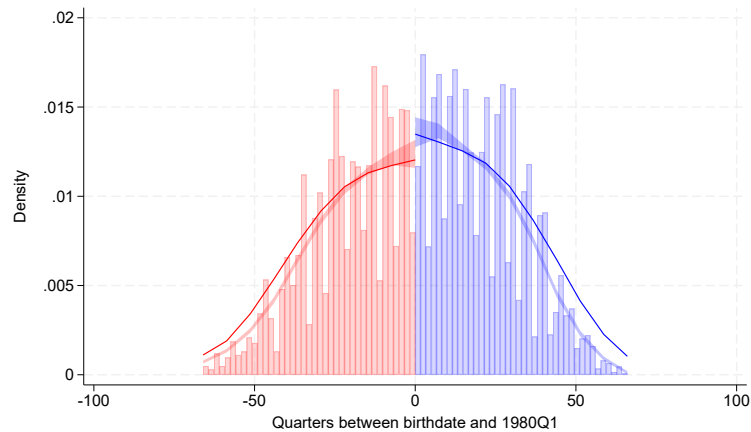
No manipulation. The second assumption ensures that participants are unable to sort themselves on either side of the cut-off point. In our context, parents' birth quarters must not be manipulated around the policy date. Even though some families were pre-aware of the policy in early 1979, because grandmothers need a gestation period of ten months, there is minimal to no opportunity for manipulation at the cut-off quarter (1980Q1).

We also conduct manipulation tests using Stata command `rddensity` proposed by Cattaneo et al. (2018). Figure 3.2 demonstrates the results. We see no discontinuity in the density of the running variable at the cutoff point, indicating no evidence of manipulation at the policy cutoff. This supports the validity of the running variable and strengthens the credibility of our regression discontinuity design.¹¹

¹⁰A balance check for the fathers' dataset also supports the continuity assumption of the design. See Table B.12 in Appendix B.5.

¹¹The manipulation test using the fathers' dataset also indicates no statistically significant discontinuity in the density of the running variable at the cutoff. See Figure B.6 in Appendix B.5.

Figure 3.2: Manipulation tests: mothers' birth quarters



Notes: The figures depict manipulation tests based on density discontinuity using Stata command `rddensity` (Cattaneo et al., 2018). Solid lines display point estimates, separately estimated on each side of the cut-off, with shaded areas indicating 95% confidence intervals. Confidence intervals are not centered at the density point estimates because they have been bias-corrected (Cattaneo et al., 2022).

3.5 Results

In this section, we present the main findings of our study. We begin by examining the policy's effects on the first generation (mothers), and then turn to its intergenerational effects on children's health. We also investigate other potential channels, including household expenditure on children's health and parent-child interactions, to clarify the mechanisms underlying the policy's intergenerational effects.

3.5.1 The policy's effects on mothers

Mothers' sibling composition. Table 3.4 shows how the policy affects mothers' sibling composition. The results indicate a significant shift toward smaller family structures after the policy was implemented. Specifically, column (1) shows that exposure to the policy significantly increases the probability that a mother has no siblings by 7.9 percentage points (pp), about 78% of the sample mean. This implies a substantial increase in the likelihood of being an only child.

Columns (2) through (5) decompose sibling composition by gender and birth order. The policy significantly reduces the likelihood of having an older brother or an older

sister. In contrast, the coefficients for having a younger brother or younger sister are small and statistically insignificant. Taken together, these results suggest that the policy effectively increases the prevalence of single-child households and reduces the likelihood that the index mother was born into a pre-existing sibling group, thereby substantially reshaping family structure.¹²

Table 3.4: Policy effects on mothers' sibling composition

	(1) No siblings	(2) Older Brother	(3) Younger Brother	(4) Older Sister	(5) Younger Sister
Policy	0.079*** (0.011)	-0.101* (0.042)	0.043 (0.035)	-0.084* (0.040)	-0.019 (0.038)
Mean	0.101	0.321	0.424	0.323	0.304
Observations	3,537	3,537	3,537	3,537	3,537
R^2	0.295	0.293	0.263	0.277	0.188

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. The policy cut-off is 1980Q1. All regressions include mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, mothers' age, and mothers' province-birthyear fixed effects. All regressions include the interaction between treatment and running variables.

Mothers' health. Table 3.5 examines the long-term effects of the policy on mothers' health outcomes. Overall, the policy improves general physical health outcomes for mothers while having no statistically significant effect on psychological distress. For mothers born after the policy date, the policy significantly increases their *health index* by 0.07, which is about 18% of a standard deviation (SD)¹³, indicating an improvement in their overall health (columns 1 and 2). In terms of mental distress (columns 3 and 4), the estimated effects are positive but small (approximately 0.025) and not statistically significant. Given that the mean of the distress variable is approximately 0.277, these magnitudes are modest from an economic perspective. Table B.8 in Appendix B.4 reports the effects on individual health components of *Health Index*. In general, mothers affected by policy exhibit better health as they are less likely to report discomfort,

¹²We conduct the same analysis using the dataset of fathers in Appendix B.5 Table B.14, and find that their sibling sizes do not change. This finding is consistent with the hypothesis that the policy was more likely to affect families with a firstborn girl.

¹³This standard deviation is 0.39 in Table 3.1.

chronic disease, or hospitalization.¹⁴

Table 3.5: Policy effects on mothers' health status

	Health Index		Mental Distress	
	(1)	(2)	(3)	(4)
Policy	0.071** (0.026)	0.068* (0.033)	0.028 (0.025)	0.025 (0.024)
Policy $\times$ Quarter	Yes	No	Yes	No
Mean	0.041	0.041	0.277	0.277
Observations	3,550	3,550	3,472	3,472
R^2	0.071	0.071	0.064	0.064

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. The policy cut-off is 1980Q1. All regressions include mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, mothers' age, and mothers' province-birthyear fixed effects. Regressions (1) and (3) include the interaction between treatment and running variables. *Health Index* is the standardized weighted summary index variable created from multiple health outcomes (See Table B.8). *Distress* is a binary variable where 1 indicates psychological distress. *Distress* equals 1 if Kessler Psychological Distress Scale (K6) is larger than or equal to 5, or CES-D8 is larger than or equal to 7, or CES-D20 is larger than or equal to 16.

Mothers' demographic socioeconomic characteristics. Table 3.6 shows the effects of the policy on mothers' demographic and socioeconomic characteristics. The policy significantly reduces the number of children by 0.133, relative to an average of 1.764 children per mother, indicating lower fertility among mothers born after the policy. At the same time, the policy increases the educational attainment and income of those affected mothers. The probability of having a college degree or higher rises by 8.4 percentage points, equivalent to 40% of the mean. Log annual income increases by 0.556, implying a substantial proportional increase in earnings. In addition, the policy raises the social position of women,¹⁵ as their likelihood of holding a leading position increases by 0.8 percentage points, which corresponds to roughly 70% compared to the

¹⁴Table B.15 in Appendix B.5 reports the policy's effects on fathers' health outcomes. We find a significant positive effect on fathers' physical health index, but a negative effect on their mental health.

¹⁵Women are considered to have a high social status when they are leading cadres, who are high-ranking managers and officials holding leadership positions within the Chinese Communist Party (CCP), the state administration, public institutions, or enterprises.

mean.¹⁶

Overall, these results suggest that policy exposure is associated with lower fertility and improved socioeconomic status in adulthood, consistent with long-term gains in human capital and economic outcomes among affected mothers (Hu and Yang, 2025).

Table 3.6: Policy effects on mothers' demographic and social characteristics

	(1) Number of children	(2) College+	(3) Ln(Yearly Income)	(4) Social Position
Policy	-0.133** (0.042)	0.084** (0.027)	0.556** (0.198)	0.008** (0.003)
Mean	1.764	0.212	5.461	0.011
Observations	3,555	3,555	3,089	3,555
R^2	0.223	0.219	0.238	0.025

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. The policy cut-off is 1980Q1. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, mothers' age, and mothers' province-birthyear fixed effects. All regressions include the interaction between treatment and running variables. *College+* takes 1 if a mother has a college degree or higher. *Social Position* is a binary variable equal to 1 if a mother holds a leading position within the Chinese Communist Party, state administration, public institutions, or enterprises.

We expect these benefits for mothers may extend to the next generation, contributing to improved health outcomes for children. Better maternal health can enhance prenatal and early-life conditions, while higher income and educational attainment enable greater investments in nutrition, healthcare, and a supportive home environment. To confirm this, we now turn to the policy's effects on children's health.

3.5.2 The policy's intergenerational health effects on children

In this section, we provide evidence for the intergenerational health effects of the policy on the second generation (children). The results suggest significant improvements in the physical and mental health of children born to mothers after the policy came into effect (Table 3.7).

In particular, children of mothers born after the policy came into effect experience

¹⁶Table B.17 in Appendix B.5 presents the policy effects on fathers' demographic and socioeconomic outcomes. In contrast to mothers, policy-exposed fathers tend to have more children, with no improvements in college attendance, income, or social status.

an improvement in *health index* by 0.061 or about 15% of a standard deviation (SD).¹⁷ Moreover, these children are also reported to have better mental health, with a 9.3 percentage point decrease in the likelihood of being distressed, equivalent to over 50% compared to the mean.¹⁸

Table B.9 in Appendix B.4 reports the effects on individual health components of *Health Index*. Children of mothers born after the policy came into effect are 3.5 percentage points less likely to be sick and 2.7 percentage points less likely to be hospitalized. Additionally, these children are 9 percentage points more likely to rate themselves as healthy, representing an increase of around 23% relative to the mean. The policy is also associated with a 1.3 percentage-point increase in the probability that the interviewer rates the child as in good health.¹⁹

Table 3.7: Children's results using mothers' birth information

	Health Index		Mental Distress	
	(1)	(2)	(3)	(4)
Policy	0.061*** (0.009)	0.055** (0.019)	-0.093*** (0.024)	-0.094** (0.027)
Policy $\times$ Quarter	Yes	No	Yes	No
Mean	0.007	0.007	0.170	0.170
Observations	3,546	3,546	1,310	1,310
R^2	0.062	0.061	0.146	0.146

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. The policy cut-off is 1980Q1. All regressions include children of mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. Regressions (1) and (3) include the interaction between treatment and running variables. *Health Index* is the standardized weighted summary index variable created from multiple health outcomes in Table B.9. *Distress* in column (4) equals 1 if Kessler Psychological Distress Scale (K6) is larger than or equal to 5, or CES-D8 is larger than or equal to 7, or CES-D20 is larger than or equal to 16.

In Figure 3.3, we also present a visual representation of the policy's impact on

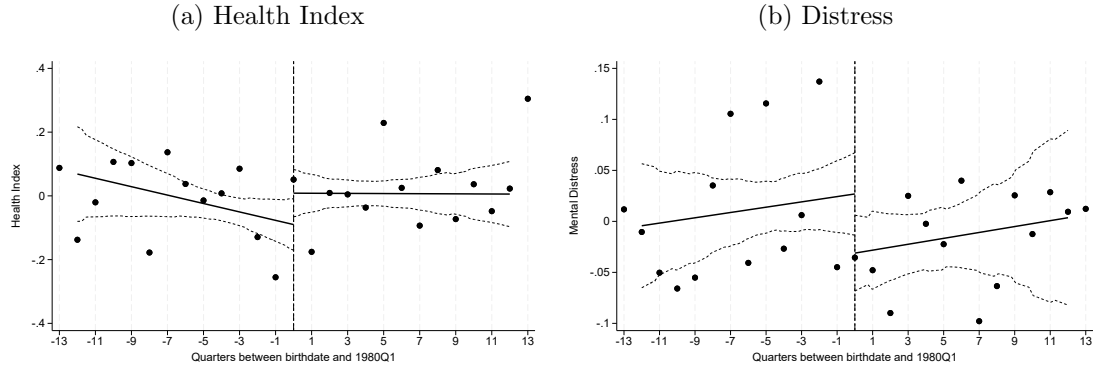
¹⁷This standard deviation is 0.39 in Table 3.1.

¹⁸In Table B.18 (Appendix B.5), we show the results for children using fathers' birth information. The results are consistent with those based on mothers' birth information, but are less statistically efficient. Children born to fathers affected by the policy experience an improvement in *health index* of approximately 0.05 SD. They also tend to be less distressed, though the difference is not statistically significant.

¹⁹See Table B.19 (Appendix B.5) for individual health outcomes using fathers' birth information.

children's physical and mental health using maternal birth information.²⁰ These figures complement the regression results and show a clear improvement in children's health and mental well-being associated with the policy-taking effects.

Figure 3.3: RD plots for children's health outcomes



Notes: The points depict binned residuals from a main regression of the outcome variable on a linear polynomial in birth quarter, along with other control variables. Solid lines display local linear regressions, separately estimated on each side of the cut-off, with dashed lines indicating 95% confidence intervals. Figure B.4 in Appendix B.4 displays RD plots for quadratic polynomial regressions.

In Appendix B.2 , we conduct a wide range of sensitivity checks to demonstrate the robustness of our results. Our results remain robust across alternative bandwidths, polynomial orders, and model specifications, including the use of months as the running variable, an alternative clustering scheme, and a donut exercise that excludes observations near the policy cutoff. We also perform placebo tests by applying the empirical strategy to different cutoff points where no policy change occurred. We find no significant effects, which supports the validity of our identification strategy. In Appendix B.1, we also examine comparable items from the K6, CESD-20, and CESD-8 scales. We find consistent beneficial policy effects, with children being less likely to experience mental health problems.

Heterogeneity. In Appendix B.3, we also examine the heterogeneous effects by children's gender, children's age groups, and by mothers' birth order status (first-born versus later-born).

²⁰See Figure B.4 for quadratic polynomial regressions.

Children’s cognitive outcomes. In addition to health outcomes, we also investigate the policy effects on children’s cognitive outcomes. Cognitive outcomes are collected for school-age children, i.e. those aged 6 to 15, in the dataset. We observe that children born to mothers affected by the policy are slightly more likely to be selected for the key class at school, while the effects on Chinese and maths grades, measured on Likert scales (good and excellent indicators here), are not statistically significant. In this paper, we therefore focus on children’s health and omit these results from the main text. The results can be found in the Appendix Table B.10.

Overall, the results on children provide evidence of the intergenerational transmission of health and household characteristics across generations. The improved health outcomes observed among children can be partly attributed to better maternal health and lower fertility shown in Section 3.5.1. The One Child policy has positive causal effects on mothers’ health (Table 3.5), which, in turn, can be passed on to their children (Emanuel et al., 1992; Eriksson et al., 2005). Reduced fertility would also lead to increased human capital investment per child, which is consistent with the quantity-quality trade-off.²¹ In addition, higher parental education, income, and social status (Table 3.6) are likely to translate into improved child health outcomes. These factors enhance parental investments in nutrition, healthcare, and early-life environments, which are vital for child development. Our findings are consistent with the existing literature documenting how parental education (Chou et al., 2010; Keats, 2018) and family socioeconomic status (Currie et al., 2007; Poulain et al., 2019; Sepehri and Guliani, 2015) improve children’s health. In the following section, we present additional evidence for increased investments in child health and explore other potential mechanisms underlying improved child health.

3.5.3 Within-household Mechanisms

In this section, we discuss other potential channels we can investigate empirically using the CFPS data to shed light on the policy’s intergenerational effects. First, better health in children can be explained by higher investment in child health as family sizes

²¹We will discuss this further in the next section.

shrink, which aligns with the quantity-quality trade-off. Second, parenting practices and parent-child interactions reveal that these parents are highly responsive while still maintaining appropriate levels of supervision, which helps explain the lower levels of psychological distress observed among their children.

Higher investment in child health. We provide evidence that children born to parents affected by the policy receive higher investments from their parents, given the fact that their family size is reduced. Table 3.8 presents the estimated effects of the policy on family income, overall expenditure, and expenditure specifically allocated towards children’s health. We observe a statistically significant increase in total family income, although the magnitude is negligible compared to the mean (column 1).²² Higher family income can be attributed to either greater inherited wealth or higher personal income, thanks to better education. This finding aligns with the positive effects on mothers’ socioeconomic characteristics reported in Section 3.5.1. Policy-affected mothers are more likely to have no siblings, potentially receiving more wealth from their parents. They are also more educated and earn more than those not affected by the policy.

We find no significant effects on total expenditure or on the likelihood of children having public insurance (columns 2 and 3). However, children born to mothers affected by the policy are 6.3 percentage points more likely to have commercial or private health insurance (column 4). Additionally, column (5) shows a considerable increase in parents’ spending on children’s commercial insurance (0.438, a 33% increase compared to the mean). This evidence suggests that policy-affected mothers are more concerned about their children’s health. They allocate more resources towards child healthcare and invest in preventive measures such as health insurance.²³

²²Total family income comprises five components: wage income, total/net business income, property income, transfer income, and other income.

²³However, our analysis of fathers’ data reveals no increase in investment in children’s health (Table B.20), which emphasizes that the improvements in child health primarily come from their mothers’ concerns and investments.

Table 3.8: Family income and expenditure

	(1)	(2)	(3)	(4)	(5)
	Total Income	Total Exp.	Public Ins.	Commercial Ins.	Commercial Ins. Spending
Policy	0.154* (0.069)	-0.018 (0.029)	-0.001 (0.025)	0.063*** (0.015)	0.438** (0.146)
Mean	10.703	10.903	0.709	0.204	1.296
Observations	3,403	3,399	3,456	3,443	3,438
R^2	0.260	0.302	0.162	0.071	0.079

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. The policy cut-off is 1980Q1. All regressions include children of mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. All regressions include the interaction between treatment and running variables. The first two are taken from family level expenditure and the rest are directly on children. Total family income comprises five components: wage income, total/net business income, property income, transfer income, and other income. We take natural logs of total income and expenditure (columns 1 and 2), medical expenditure (column 3), and commercial insurance spending (column 6). Public and commercial insurance in columns (4) and (5) are binary variables.

Parenting and Family interactions. We now examine the parenting practices and interactions between parents and children to explain the lower level of distress among children. Previous research has extensively examined the relationship between child-rearing practices and children's anxiety. Children may lose their chances to advocate for their interests under parental psychological control, which triggers higher levels of mental distress (Chyung et al., 2022; Luebbe et al., 2014). McCoby (1983), building on the work of Baumrind (1971), identifies four parenting styles characterised by levels of demandingness and responsiveness. Parental demandingness or control significantly influences children's anxiety levels (Pinquart, 2017). High demands from parents cause worry and anxiety, especially for those with executive functioning deficits, to manage these concerns. Conversely, low parental demands reduce anxiety among children because they may not be worried about meeting parents' expectations (Lo et al., 2020; Wong et al., 2019). Meanwhile, parental responsiveness is another important element that decides the level of anxiety among children. High responsiveness from parents strengthens the family bond and fosters children's social and emotional development, whereas children with less responsive parents are more prone to mental disorders and

struggle with social functioning (Davidov and Grusec, 2006; Miller-Slough et al., 2018).

We construct several variables from the CFPS surveys to explore this mechanism. Table 3.9 reports the policy effects on the interactions between parents and children. We can see positive and significant impacts on the overall home environment, as parents are more actively involved in communicating with their children (column 1). However, there is no significant difference in their concern for children’s education (column 2). From children’s responses,²⁴ we see children more frequently communicate with their parents, either talking or arguing (columns 3-4). Since increased arguments may reflect greater parent-child engagement, especially for those early teenagers who are beginning to seek independence, the positive effect on arguing (column 3) does not necessarily indicate a poorer home environment.²⁵ Besides, we see null effects of children observing parents quarrelling with each other (column 5). Overall, our results suggest that household conversations increase, and as a result, children have more opportunities to speak up for themselves.

Meanwhile, the responses of parents in Table 3.10 suggest that parents born after the policy cut-off tend to be more responsive and more engaged in children’s education. They are more likely to forgo watching television to avoid disturbing their children (column 1) and to discuss school activities with them (column 2), consistent with the improved within-household communication presented above. They are also more likely to check children’s homework or require their children to complete homework (column 3) and to impose limits on their children’s television viewing (column 4), indicating greater parental involvement in children’s education. In addition, they are more likely to have saved for their children’s education from an early age (column 5).²⁶

Literature on parenting styles suggests that the ideal parenting style is “authoritative”, characterized by high responsiveness combined with appropriate parental control that supports child autonomy (Baumrind et al., 2010; Doepke and Zilibotti, 2017). Con-

²⁴Only children aged 9-15 provide answers to these questions.

²⁵Table B.11 displays the results when we restrict our sample to those aged 9 or above. The results stay consistent, suggesting more communication between parents and children.

²⁶We see a general null or somewhat negative effects on parental practices and parent-child interaction from the fathers’ side in Table B.21 and Table B.22. In general, these results support our main finding using fathers’ dataset that there is no discontinuity in children’s level of distress at the policy cut-off.

Table 3.9: Interaction between parents and children

	Interviewers' observations		Children's Responses		
	(1) Active Communication	(2) Care about Education	(3) Quarrel	(4) Heart-to-heart Talk	(5) Parents Quarrel
Policy	0.018** (0.006)	0.006 (0.010)	0.501** (0.148)	0.441 (0.284)	0.007 (0.186)
Mean	0.871	0.861	1.273	2.383	0.844
Observations	2,871	2,927	1,226	1,139	1,199
R^2	0.566	0.383	0.078	0.108	0.057

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. The policy cut-off is 1980Q1. All regressions include children of mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. All regressions include the interaction between treatment and running variables. The first two variables are dummy variables indicating interviewers' observations of whether parents communicate actively with their child and whether the home environment indicates that parents care about their child's education. The next three variables are only reported by children aged 9-15. Quarrel refers to the number of times children quarrelled with their parents last month (column 3). Heart-to-heart talk refers to the number of times children had a heart-to-heart talk with parents last month (column 4). The last variable (column 5) refers to the number of times the parents quarrelled with each other in the last month.

sistent with this framework, our findings show that policy-affected parents are highly responsive while still maintaining an appropriate level of control, such as monitoring homework and regulating television viewing. This balance of warmth, support, and supervision creates a home environment that is conducive to children's development. Our findings align with existing research on Chinese parents, showing that greater parental responsiveness and authoritative parenting improve child outcomes and reduce psychological distress, especially among single children (Liu et al., 2010; Lu and Chang, 2013; Nie et al., 2022).

3.6 Conclusion

The One-Child Policy (OCP), while formally relaxed in 2016, has left a profound and lasting legacy on China's demographic and social landscape. By restricting most urban families to one child for over three decades, the policy fundamentally altered family structures across generations. This paper provides new causal evidence on the in-

Table 3.10: Parental care

	(1) Give up watching TV	(2) Discuss School	(3) Homework Check	(4) TV Restriction	(5) Save for Education
Policy	0.083*** (0.011)	0.026 (0.018)	0.030* (0.014)	0.035** (0.014)	0.030* (0.012)
Mean	0.245	0.105	0.327	0.200	0.251
Observations	2,170	2,247	2,196	2,231	3,457
R^2	0.095	0.083	0.130	0.075	0.099

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. The policy cut-off is 1980Q1. All regressions include children of mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. All regressions include the interaction between treatment and running variables. Outcomes in columns (1)-(4) are dummy variables constructed based on parents' responses: whether parents very often (6-7 times a week) give up watching TV to avoid disturbing their child (column 1), whether parents very often discuss happenings at school with their child this semester (column 2), whether parents very often ask their child to finish homework or check their child's homework (column 3), and whether parents restrict their child from watching TV or restrict the type of TV programs their child could watch (column 4). Column (5) is a dummy variable equal to 1 if the parent answered "yes" to the question of having started saving money for the child's education.

tergenerational spillover effects of the OCP, examining specifically how the policy's enforcement on the first generation (parents) has shaped the health outcomes of the subsequent generation (children).

Utilizing a reduced-form regression discontinuity design (RDD) and data from the China Family Panel Studies (CFPS), we find significant improvements in both physical and mental health of children whose mothers were affected by the policy's enforcement. We emphasize the beneficial impacts on children from the mothers' side, given the unique benefits observed for urban Han daughters under the one-child policy. Specifically, these children are found to have a *health index* improvement of approximately 15% of a standard deviation and a 9.3 percentage point reduction in the probability of experiencing mental distress.

Our analysis highlights three main transmission channels through which these intergenerational gains occur. First, the mothers' socioeconomic status and health. Policy-affected mothers are more likely to be the only kids, leading to higher educational attainment, increased income, and improved physical health. These factors provide a

strong foundation for their children's development. Second, reduced family size leads to greater household investment in each child's health, supporting the theoretical framework that lower fertility encourages higher-quality investment per child. Third, we find that policy-affected parents are highly responsive while maintaining appropriate control. This creates a better environment that significantly reduces psychological distress in the second generation.

These findings extend the existing literature on family planning by demonstrating that population control policies affect not only the targeted generation but also the next. While the OCP remains a subject of intense social and demographic debate, our paper suggests that it has yielded significant health benefits for subsequent generations. For policymakers, these results highlight the importance of considering the long-term, multigenerational spillover effects when evaluating the success and impact of family planning interventions.

Chapter 4

Genocide, Female Empowerment and Domestic Violence in Cambodia

4.1 Introduction

While mass atrocities such as war and state repression are known to significantly influence long-term development (Davenport et al., 2019; Grasse, 2024; Lichter et al., 2021), their effects on female empowerment and experiences of gender-based violence remain less understood. Conflicts and violence reshape household dynamics by disrupting demographic structures and redefining traditional gender roles (Buvinic et al., 2013; Ekhat-Mobayode et al., 2022). One of the most immediate consequences is a disproportionately high mortality rate among men, which increases female participation in the workforce (Boehnke and Gay, 2022; Gaikwad et al., 2023). However, these changes do not always translate into greater gender equality. Instead, altered sex ratios and evolving household roles may affect women’s bargaining power and decision-making capacity, potentially increasing their vulnerability to control or abuse by male partners (Bhalotra et al., 2021; Guarnieri and Rainer, 2021). In this context, intimate partner violence (IPV) or domestic violence becomes a critical issue. IPV or domestic violence, which refers to behaviours by a current or former partner that cause physical, sexual, or psychological harm, remains a global public health concern with nearly one in three women worldwide affected (WHO, 2021).¹ Although violence may create opportunities for changes in gender roles, it can also deepen existing inequality and increase women’s vulnerability to domestic violence. This duality raises an important research question: how do violence and repression shape women’s empowerment² and their exposure to domestic violence in the long term?

To answer this question, I examine the Cambodian genocide, which took place from 1975 to 1979 under the Khmer Rouge regime led by Pol Pot.³ During this period, the regime carried out mass executions, resulting in the deaths of 1.6 - 1.8 million people,

¹Recognizing its severity, the 2030 UN Sustainable Development Agenda established Goal 5.2, which calls for the elimination of all violence against women and girls in both public and private settings (Assembly et al., 2015).

²Female empowerment is defined as enhancing their ability to access key dimensions of development, including health, education, employment, rights, and political participation (Duflo, 2012). In this paper, female empowerment specifically refers to women’s economic power within the household and their autonomy in making household decisions.

³*Khmer Rouge* is the common name for the Communist Party of Kampuchea (CPK) and, more broadly, the Democratic Kampuchea (DK) regime that ruled Cambodia from 1975 to 1979.

roughly a quarter of the country’s population (Kiernan, 2003). Mortality during the genocide was highly selective, with adult men being disproportionately targeted for political executions, as they were more likely to be educated and to hold government positions (De Walque, 2005). Gender imbalance caused by the genocide led to a shortage of young men in the early 1980s (De Walque, 2006), which reduced women’s chances of marriage, increased remarriage opportunities for men (Heuveline and Poch, 2006), and resulted in a significant number of families being headed by widows (Chandler, 2018). After several decades, however, an open question remains regarding the long-term effects of this gender imbalance on women’s empowerment, household dynamics, and exposure to domestic violence. Although the immediate demographic consequences are well established, there is limited understanding of how these disruptions have influenced gender roles and power dynamics within families over time. Traditionally in Cambodia, men were seen as breadwinners and superior to women, while women were expected to run households, raise children, manage domestic finances, and serve their husbands (Gorman et al., 1999; Ovesen et al., 1996). These expectations reinforced women’s lower social status and restricted their autonomy within households. In this context, domestic violence was widely accepted, as it was often justified as punishment for not fulfilling wifely duties, and even condoned by the community (Gorman et al., 1999). This paper, therefore, examines how the genocide influences existing gender inequality by focusing on women’s decision-making power, their economic role within households, and their vulnerability to domestic violence.

In this paper, I leverage various geo-coded datasets, including Demographic and Health Survey (DHS), Cambodian Genocide Database by Yale University, General Population Census of Cambodia and other historical and contemporary data on geographic, demographic and economic characteristics.⁴ I employ a spatial regression discontinuity design (RDD), similar to the specifications in Gonzalez (2021) and Lehner (2024), to compare individuals residing in the formerly high-repression region with those in a comparatively moderate region. I borrow the RDD border from Grasse (2024), which divides the Southwest and West Zones, to examine how variation in genocide intensity

⁴See Section 4.3 for more details.

influences long-term outcomes in female empowerment and domestic violence. I use the distance to this zone border as a running variable.

The border between the Southwest and West Zones represents a meaningful division in the intensity of Khmer Rouge violence, shaped by the contrasting leadership of Ta Mok, who was an extremist loyalist in the Southwest, and Sy, who governed the West with relatively more moderate policies.⁵ Although both regions experienced repression, extensive historical and quantitative evidence highlight significantly greater brutality in the Southwest (Gaikwad et al., 2023; Grasse, 2024). In Section 4.5, I use data from the Cambodian Genocide Database, which documents 309 geo-coded sites containing 18,953 Democratic Kampuchea (DK) mass graves, to empirically assess the difference in violence across this border. The analysis shows that villages in the extremist Southwest Zone are significantly more likely to be located near a DK mass grave and have more graves in their vicinity compared to those in the moderate West Zone. This highlights a sharp difference in genocide repression along the Southwest–West Zone border and confirms the validity of this border.

The baseline results, in Section 4.6, show that after several decades, the genocide still has lasting negative effects on female empowerment and domestic violence. In areas where genocide was intense, women at present are less empowered in households and more vulnerable to domestic violence, especially emotional violence. Particularly, they are 4.2 percentage points less likely to work full-time (a 6% decline), 7.2 percentage points less likely to participate in important household decisions (a 12.8% decrease), and 5.8 percentage points more likely to be controlled by their partners (a 46% increase). Regarding domestic violence, I do not find any significant effects on the likelihood of women experiencing physical violence. However, women in the brutal Southwest Zone are significantly more likely to experience emotional violence, with an increase of 8.6 percentage points (over 50% above the average), and also face higher risks of experiencing either or both emotional and physical violence. Additionally, they are also more likely to justify domestic violence, with acceptance rates rising by 4.1 percentage points, or 12.7% of the mean. Furthermore, these findings remain consistent across a

⁵See Section 4.2 for more details.

wide range of sensitivity checks, reinforcing the credibility and reliability of the results.

In Section 4.7, I provide evidence for several mechanisms that can possibly explain the main findings. First, the decline in female empowerment and an increased vulnerability to domestic violence can be attributed to skewed sex ratios and higher fertility rates post-genocide. The extreme violence under the Khmer Rouge caused higher male mortality and skewed sex ratios, and although decades of demographic recovery have normalized these imbalances among younger cohorts, the overall sex ratio remains much lower than the world average, with women still outnumbering men. Additionally, in the previously high-intensity zone, fertility is significantly higher, which may strengthen women's traditional roles and undermine their empowerment. Furthermore, the genocide significantly affects household structure and household wealth, with those in high-repression areas having more children and lower wealth today. Finally, changes in gender-related characteristics can further explain the main results. In the more violent Southwest Zone, both men and women tend to marry at a younger age and achieve lower levels of education. Women are more frequently engaged in part-time or unstable employment, while men more often have multiple marriages or unions. Together, these factors reinforce gender inequality, limiting women's opportunities in education, work, and family life, and leading to lower female empowerment and greater risk of domestic violence.

This study sheds light on the puzzle of how violence and repression shape female empowerment and domestic violence in the long run. It underscores the persistent effects of mass violence on gender inequality, particularly through skewed sex ratios, higher fertility, and restricted opportunities for women. In the case of Cambodia, these legacies constrain women's decision-making power, reinforce unequal household roles, and increase their vulnerability to domestic violence. By investigating these long-term impacts, the paper shows that mass violence not only harms societies in the short run but also creates lasting barriers that hold back women's empowerment for generations.

Literature Review This paper contributes to the existing literature on how violence influences gender inequality, with a focus on female empowerment and domestic

violence. Violence reshapes households by altering demographic structures and gender roles, with men facing higher mortality rates and women more likely to participate in the labour market (Buvinic et al., 2013; Ekhtor-Mobayode et al., 2022). Existing literature presents mixed evidence on the impact of violence and conflicts on female empowerment. A variety of studies have found that conflicts have detrimental impacts on female decision-making power, with evidence found in Russia after World War II (Brainerd, 2017), in Rwanda after the 1994 genocide (La Mattina, 2017) or in Nigeria after the Civil War (Ajefu and Casale, 2021).⁶ However, several other papers suggest that conflicts can also enhance female empowerment, characterized by increased women's civil liberty, political and social participation post-conflict (Bakken and Buhaug, 2021; Hughes and Tripp, 2015; Webster et al., 2019).⁷

Meanwhile, numerous papers exploring the impact of violence and conflicts on IPV find a consistent positive association. La Mattina (2017) explores how the 1994 Rwandan genocide significantly impacted gender inequality by altering marriage market sex ratios, which in turn increased domestic violence and reduced women's decision-making power. Ekhtor-Mobayode et al. (2022) examine the Boko Haram insurgency in Nigeria and finds that the insurgency increases the likelihood of women experiencing physical or sexual IPV. Kelly et al. (2018) examine the armed conflict in Liberia and find that living in a district affected by conflict fatalities was associated with a 50% higher risk of IPV. Gutierrez and Gallegos (2016) show that childhood exposure to civil conflict

⁶These papers indicate that conflict undermines women's empowerment. Using World War II-induced changes in sex ratios, Brainerd (2017) finds that male scarcity in Russia led to fewer marriages and births, more nonmarital births, and reduced bargaining power for women. La Mattina (2017) reports that the 1994 genocide in Rwanda drastically altered local sex ratios, and women who married after the conflict, particularly in areas with high genocide intensity, faced increased domestic violence and reduced within-household decision-making power. Ajefu and Casale (2021) examine the Nigerian Civil War and find that early-life exposure to the war reduced women's decision-making power in adulthood, likely through its negative effects on their education, health, employment, and marriage outcomes.

⁷Specifically, Hughes and Tripp (2015) illuminate how civil wars in Africa between 1985 and 2010 increased women's political representation post-conflict by creating opportunities for institutional and social change. Using cross-national data from 1900 to 2015, Webster et al. (2019) show that war can temporarily increase women's empowerment by disrupting social institutions and shifting gender roles, with lasting change depending on post-conflict reforms and gender mainstreaming. Bakken and Buhaug (2021) also find that high-intensity civil conflicts, especially those ending in peace agreements with gender-specific provisions, are most likely to lead to post-conflict improvements in female empowerment, including individual civil liberties and political participation.

in Peru increases the risk of both experiencing and perpetrating domestic violence in adulthood, with women in heavily affected areas more likely to accept and remain in abusive relationships.

This paper also contributes to the broader research on the association between female empowerment and domestic violence. Several economic theories help explain this relationship, including the home bargaining model and male backlash theory (Meyer et al., 2024). Home bargaining theory states that greater economic opportunities for women can lower IPV by increasing their bargaining power within households and raising the costs for male partners to engage in domestic abuse (Meyer et al., 2024). This model is supported by a variety of papers drawing data from Rwanda (La Mattina, 2017), the UK (Anderberg et al., 2016) and U.S counties (Henke and Hsu, 2018, 2020). On the contrary, male backlash theory suggests that positive changes in women’s status and power, whether in the household or society, can lead to increased IPV. Men may feel threatened by women gaining power and status, using IPV as a way to reaffirm their control and authority (Meyer et al., 2024). A wide range of studies support this theory, with findings from Cameroon (Guarnieri and Rainer, 2021), 30 Sub-Saharan African countries (Cools and Kotsadam, 2017) and developing countries (Bhalotra et al., 2021).

The paper further adds to the limited research on the legacy of Khmer Rouge Genocide in Cambodia. Most of these studies show persistent negative effects. Grasse (2024) examines the long-term economic impacts of the genocide and finds that villages exposed to more extreme repression have persistently lower wealth and human capital. Islam et al. (2017) investigate how the Khmer Rouge genocide’s intensity affects the educational and health outcomes of children born long after the conflict. They show that higher genocide exposure among parents reduces children’s grade progression and height, with evidence suggesting the marriage market as a key channel of intergenerational transmission. A few studies reveal that enduring mental health impacts of the Khmer Rouge genocide, with both survivors and perpetrators experiencing high levels of stress, depression, and intrusive memories decades after the violence (Marshall et al., 2005; Nou, 2024).

The paper most closely aligned with this research is that of Gaikwad et al. (2023)

showing that intense Khmer Rouge violence, which disproportionately killed working-age men, led to long-term gains in women’s economic autonomy and increased participation in local government. Different from this paper, which focuses on broader political or economic consequences of conflict, my study specifically examines how the Cambodian genocide has shaped intra-household decision-making power and the prevalence of domestic violence within households. By focusing on household-level outcomes, the study offers a novel perspective on the long-term impacts of mass violence on family structures and gender dynamics, especially how gender equality evolves post-violence.

The following section provides a historical overview of the Cambodian genocide. Section 4.3 describes the data used in the study, followed by Section 4.4, which outlines the empirical strategy and underlying assumptions. Section 4.5 provides a motivating analysis on the validity of the RDD border. Section 4.6 reports the main results, and Section 4.7 explores potential mechanisms that possibly shed light on our results. Section 4.8 concludes the paper.

4.2 Background

In the twentieth century, Cambodia experienced a series of French colonization, civil conflicts, political instability and devastating genocide (Chandler, 2018; Slocomb, 2010). The country gained independence from France in 1953, ending 90 years of colonial rule. In 1955, Prince Sihanouk, who was leading the country at the time, abdicated to pursue politics and founded the Sangkum party, which won unopposed elections. For 15 years, Sihanouk and the Sangkum party dominated Cambodia, but by 1966, his political control and confidence weakened due to the Vietnam War and internal pressures. In 1970, Sihanouk was deposed, and General Lon Nol’s regime took power, leading to a civil war in the country. Sihanouk then allied with the Communists, triggering heavy fighting and carpet US bombings in the country. The war ended in April 1975 when the Communist regime - the Khmer Rouge led by Pol Pot - seized the capital. The Democratic Kampuchea (DK) was then established and controlled the country from April 17, 1975, to January 7, 1979. This period is often referred to as the mass genocide when nearly

two million people, or one in four, died from overwork, starvation, disease, or execution (Chandler, 2018; Kiernan, 2003).

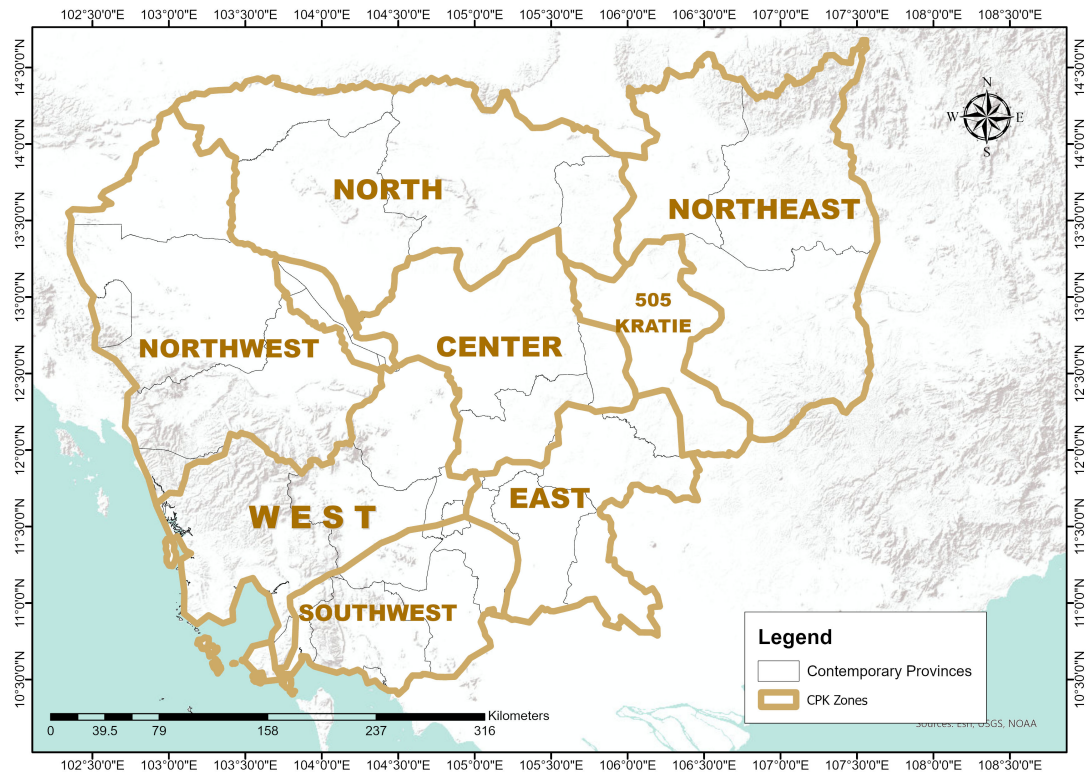
After taking control, the Khmer Rouge emptied cities, hospitals and Buddhist monasteries. All schools, postal services, factories and newspaper publishers were shut down. They eliminated money and wages and even destroyed the National Bank (Kiernan, 2023). The Khmer Rouge called urban population “new people” and displaced them to the countryside to live among the rural “base people”. They were forced to work in labor camps without payment, rights or free time. At the same time, the regime hunted down and executed thousands of former Khmer Republic officials, army officers, soldiers, educated people like doctors, teachers, monks, bureaucrats and other protesters that they thought might pose a threat to the system (Kiernan, 2002a; Slocomb, 2010). Mortality was highly selective with adult men facing a higher risk of dying (De Walque, 2005). Men, who were more likely to hold educated or government positions, were targeted more often for political executions, especially in the early months of this period (Gaikwad et al., 2023; Vickery, 1984). The extreme and selective death rate during this period significantly shaped Cambodia’s population structure (De Walque, 2006). After the DK ended its rules, there was a noticeable shortage of young men in the marriage market in the early 1980s (De Walque, 2006). The high mortality of young men led to a low sex ratio among the never-married, reducing women’s chances of marriage. It also increased remarriage opportunities for men, potentially impacting marriage stability and raising divorce rates (Heuveline and Poch, 2006). Because educated men often faced higher mortality rates under the regime, after 1979, fewer educated men were available for marriage, making male and female education levels more similar (De Walque, 2005). Additionally, with so many men who had died or disappeared, over 60% of families in some districts were led by widows, who, in many cases, struggled to raise their children alone (Chandler, 2018).

Forced marriages were a key tool for the Khmer Rouge in their vision of an agrarian utopia, where all citizens lived in village cooperatives. The regime took control of marriages and family structures, dictating where people lived, whom they lived with, and how they ate, slept, and interacted (Jacobs, 2022). They used forced marriages to

ensure loyalty to the regime, break down traditional families, and create new families aligned with the regime's ideals. In addition, these marriages were intended to secure the loyalty of the next generation through reproduction (Denov et al., 2022). Coercion was involved in every aspect of marriages, with people accepting unions out of fear of punishment, participating in mass weddings due to violence threats, and engaging in sexual relations under surveillance by armed spies (Jacobs, 2022). About one in three marriages in Cambodia between 1975 and 1979 was forced (Heuveline and Poch, 2006). The Extraordinary Chambers in the Courts of Cambodia (ECCC) estimates that around 400,000 forced marriages took place during the KR regime (Denov et al., 2022). These cohorts of marriage also had the highest proportion of women who reported meeting their husbands for the first time on their wedding day (Heuveline and Poch, 2006). Jacobs (2022) discovered that forced marriages were most common among groups seen as internal threats and in economically productive regions. In regions with high rice yields, the regime exerted tighter control to maintain high production. Moreover, internal security risks prompted the regime to enforce stricter social control measures, particularly through marriage, targeting populations seen as threats or enemies. As a result, some regions such as the Northwest and Southwest zones experienced the harshest treatment during the KR period, whereas the North and Central zones were comparatively more moderate (Jacobs, 2022).

Democratic Kampuchea was divided into seven administrative zones based on compass directions: North, Northeast, East, Southwest, West, Northwest, and Center – along with the Kratie Special Region (Figure 4.1) and, until mid-1977, the Siem Reap Special Region. The zones were newly created and did not align with previous administrative divisions. Multiple provinces could be combined into a new zone but some provinces were split between different zones (Vickery, 1984, p.66). Zones were led by zone secretaries, who were military commanders with different backgrounds and ideologies (Gaikwad et al., 2023). Zone leaders followed the general orders issued by Pol Pot from Phnom Penh but had great flexibility in deciding how to enforce these orders using their own resources and methods (Becker, 1998). Each administrative zone operated autonomously and there was little coordination between regions. Refugee accounts

Figure 4.1: Administrative Zones during Democratic Kampuchea



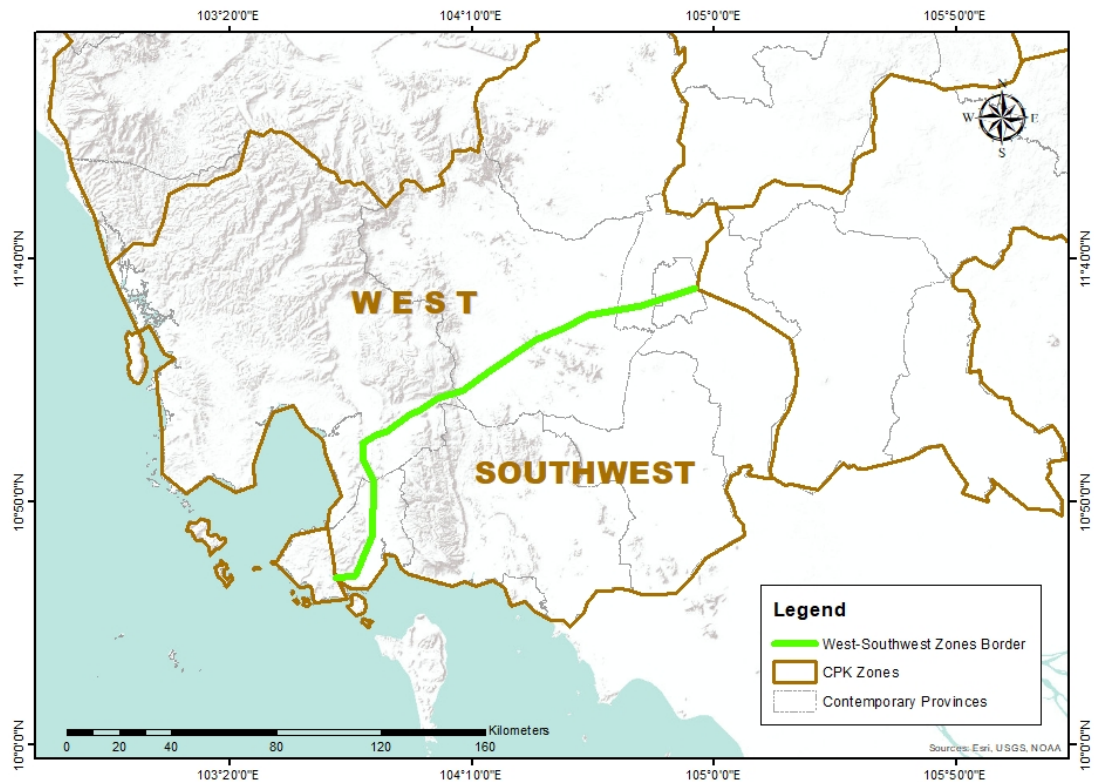
Note: The map shows the administrative divisions during Democratic Kampuchea. The country was divided into seven administrative zones based on compass directions. Map overlaid on World Terrain Base map and drawn on ArcGIS.

confirm that conditions varied greatly, including food supply, work discipline, hardship levels, execution policies, and political education (Vickery, 1984, p.68).

I borrow the RDD border from Grasse (2024) which separates the Southwest and West Zone to investigate how the difference in genocide intensity affects women's empowerment and domestic violence in the long run. During the civil war, these two zones were a single operational area with Ta Mok and Chou Chet (or Sy) sharing jurisdiction. However, a dispute over Mok's brutal tactics led to the split of this area into two separate zones along National Road 4 (NR4).⁸ NR4 was cheaply built by the U.S. in the 1950s to connect the port city (Sihanoukville) and the capital (Phnom Penh), and it became a convenient dividing line for DK. While the road's construction was politically

⁸Democratic Kampuchea used roads and natural landmarks as borders to divide rival zones, reducing conflicts over authority (Grasse, 2024).

Figure 4.2: West and Southwest Zones During Democratic Kampuchea



Note: The map highlights the border that separates the West Zone and the Southwest Zone. Map overlaid on World Terrain Base map and drawn on ArcGIS.

motivated, its precise location was determined for convenience rather than for economic factors (Grasse, 2024). Figure 4.2 shows the Southwest and West Zone border. The border does not match with any previous administrative divisions (Vickery, 1984) and also does not align with current provincial or district boundaries.⁹ This is ideal for our set-up, ensuring that it is “as good as random”.

Grasse (2024) highlights the stark contrast between the military commanders of the Southwest and West Zones. Ta Mok, who led the Southwest Zone, strictly adhered to Pol Pot’s directives and was notoriously ruthless toward “new people”. Coming from a peasant background, he embraced communism early and remained loyal to the Khmer Rouge, never surrendering or defecting like other commanders. He was called “The Butcher”, notorious for executions, torture, and forced labor. Meanwhile, Chou

⁹See Figure C.9 for pre-genocide administrative divisions and Figure C.10 for contemporary district divisions.

Chet or Sy, who led the West Zone, was relatively less violent toward them. Coming from a privileged background, he was considered an intellectual within Pol Pot’s inner circle. His zone resisted many Khmer Rouge’s harsh policies, which ultimately led to his execution at S-21 prison due to suspicions about his loyalty (Gaikwad et al., 2023). Clearly, the West Zone still experienced repression, with policies on education and labor similar to those in the Southwest. However, the key difference between Mok and Sy was not the presence or absence of violence but rather the intensity of repression (Grasse, 2024). In Section 4.5, I provide empirical evidence of the sharp contrast in genocide intensity between the Southwest and West Zones.

4.3 Data

This section presents the datasets I employ in this study. I leverage diverse data sources to investigate the long-term impacts of genocide on female empowerment and domestic violence, including individual-level data from Demographic and Health Survey (DHS), Cambodian Genocide Databases by Yale University, General Population Census of Cambodia 2008 and other historical and contemporary data on geographic, demographic and economic characteristics.

Cambodia Demographic and Health Survey. This study draws on five waves of the Cambodia Demographic and Health Survey (DHS) conducted in 2000, 2005, 2010, 2014 and 2021-22. Particularly, I rely on DHS individual women’s data, which provides extensive information on female empowerment and domestic violence. The DHS data includes precise geographic locations of survey clusters, which are groups of 25-30 households participating in the surveys, allowing me to determine which Khmer Rouge zones households currently reside in.¹⁰ Each DHS household cluster represents an enumeration area or a village. For the rest of the paper, I use the term “DHS villages” to refer to DHS clusters.

¹⁰To protect respondents’ privacy, the GPS locations of these clusters are randomly displaced by up to 2 km in urban areas and up to 10 km in rural areas. This displacement process introduces classical measurement error, which leads to unbiased estimates.

I use several outcomes as measures of female empowerment. My first outcome of interest is employment: whether a woman is working full-time. The second is decision-making power: a woman is considered to have decision-making power if she independently makes at least one of the following decisions: her own health care, major household purchases, or visits to family or relatives. The third is partner control: a woman is considered controlled by her partner if her current or most recent husband or intimate partner exhibits more than 1 of the five specified controlling behaviours: (1) Is jealous or angry if she talks to other men (2) Wrongly accuses her of being unfaithful (3) Does not permit her to meet her female friends (4) Tries to limit her contact with her family (5) Insists on knowing where she is at all times.

I also use different measures to assess domestic violence. Violence is a binary variable coded 1 if a woman reports that she experienced any physical violence and/or emotional violence committed by her current or most recent husband or intimate partner, ever, and in the 12 months preceding the survey. In addition, attitudes toward domestic violence are also measured by a dummy variable that takes the value of 1 if the woman agrees that a husband or partner is justified in beating his wife in at least one situation among the following: when she goes out without telling him, neglects the children, argues with him, refuses to have sex with him, or burns the food.

I also include household elevation data from the Cambodia DHS as a geographic characteristic in the balance check, as elevation should not be affected by the genocide. This information is sourced from the Shuttle Radar Topography Mission (SRTM) Digital Elevation Model based on the coordinates of DHS clusters.

Alongside the women’s dataset, I also utilize data on men and households to investigate potential mechanisms related to household structure, household wealth, and gender-related outcomes such as education and fertility.

General Population Census of Cambodia 2008. I use village-level data in 2008 General Population Census of Cambodia to examine post-genocide population structure and demographic characteristics. Throughout the paper, I refer to 2008 Population Census villages as “census villages” to distinguish them from DHS villages. This census

provides information on diverse population ratios, such as sex ratio, age dependency ratio, child-woman ratio and the share of female-headed households in 14,073 villages across the country. It also includes information on average age at first marriage, educational attainment, and employment status for both men and women. I use these indicators to explore possible mechanisms underlying the main findings.¹¹

Cambodian Genocide Databases I use Yale University’s Cambodian Genocide Databases to obtain information on genocide repression across the country.¹² In particular, I employ the geographic database which provides geo-information on boundaries of Democratic Kampuchea (DK) administrative zones, and 309 geo-coded locations of 18,953 DK mass graves to examine genocide repression across the border between Southwest-West Zones. I am able to match this data with DHS data and 2008 Population Census data measure genocide intensity at the DHS villages and census villages. Specifically, I count the number of graves within 5km of villages and construct a binary outcome equal to 1 if there are any mass graves within a 5km radius of villages.¹³

Pre-genocide characteristics. I digitize three maps in the 1970 Indochina Atlas to extract location-specific information on transport infrastructure, agricultural patterns, and population density pre-genocide.¹⁴ First, I geo-reference the Indochina Transportation map, which shows major roads and railways in Cambodia in 1970 (Appendix, Figure C.13), and calculate the distance from DHS villages to 1970 main roads and railways. Second, the Indochina Agriculture map identifies regions engaged in farming activities in 1970. I match this data to DHS villages to assess whether those areas were agriculturally active pre-genocide (Appendix, Figure C.14). Finally, I digitize the Indochina Population map to estimate population density at DHS villages in the pre-genocide period (Appendix, Figure C.15).

¹¹Data can be accessed via <https://opendevelopmentcambodia.net>.

¹²These databases are compiled and developed by Yale University and the University of New South Wales. Data can be accessed via <https://macmillan.yale.edu/gsp/cambodian-genocide-databases-cgdb>

¹³See Section 4.5.

¹⁴The Atlas was published in October 1970 by the Directorate of Intelligence, Office of Basic and Geographic Intelligence, U.S. Central Intelligence Agency, and is available to download at <https://maps.lib.utexas.edu>.

In addition, I utilize agro-ecological zones (AEZ) classification developed by the Food and Agriculture Organization of the United Nations (FAO) and the International Institute for Applied Systems Analysis (IIASA) to evaluate climate and crop productivity at DHS villages.¹⁵ Specifically, I use GAEZ v4 Dominant AEZ classification dataset and match DHS villages to their corresponding agro-ecological zone (Appendix, Figure C.12). I then create a binary variable capturing whether a DHS village is in a tropical lowland area.

UN-Adjusted Population Density. I utilize UN-Adjusted Population Density provided by the University of Southampton to investigate post-genocide population density. The data is developed by the WorldPop research group, based at the University of Southampton’s School of Geography and Environmental Sciences. WorldPop provides high-resolution gridded population datasets at 30 arc-second resolution, corresponding to approximately 1 kilometer at the equator. I use the annual data on UN-adjusted Population Density from 2000 to 2020, which reports the number of people per square kilometer based on country totals adjusted to match the official United Nations population estimates in Revision of World Population Prospects 2019 (United Nations, Department of Economic and Social Affairs Population Division, 2019).¹⁶

Descriptive statistics Table C.8 reports the summary statistics of all variables in this study. We can see that there are noticeable differences in both female empowerment and domestic violence outcomes between the treated (Southwest Zone) and control (West Zone) groups. In particular, although the likelihoods of decision-making power are not significantly different between the treated and control groups, women living in the former Southwest Zone are less likely to work full-time and have a higher mean likelihood of being controlled by their husbands compared to women living in the former West Zone. In terms of domestic violence, women in the treated group (Southwest Zone)

¹⁵The AEZ map captures key environmental factors, such as climate, soil, terrain, and irrigation, and identifies areas with major bio-physical constraints like deserts or steep slopes. Regions within the same AEZ share similar climate and agricultural potential. GAEZ data is commonly used in research to account for geographic and agricultural conditions. (e.g., Cagé and Rueda, 2016; Whatley and Gillezeau, 2011). Data can be accessed via <https://gaez.fao.org>.

¹⁶Data can be accessed via <https://hub.worldpop.org>.

face a higher average likelihood of experiencing all forms of domestic violence—physical, emotional, and the combination of both—compared to those in the control group (West Zone). We can also see that they are more likely to accept domestic violence with a higher mean compared to the control group.

About other geographic and pre-genocide characteristics, there are no significant differences in elevation between the two groups. However, locations in the Southwest Zone are more frequently situated in tropical lowland regions. Moreover, in 1970, this zone exhibited higher population density and a greater likelihood of agricultural activity compared to the West Zone. We also observe significantly shorter distances to 1970 main roads or railways for households living in the former Southwest Zone.

We now turn to an empirical strategy to explore the long-term impacts of the genocide on female empowerment and domestic violence.

4.4 Empirical Strategy

4.4.1 Empirical Framework

I employ a spatial regression discontinuity design (RDD) to estimate the long-term effects of the Cambodian genocide on female empowerment and domestic violence. The spatial RDD leverages sharp changes at the zone border, comparing individuals living in the extremist Southwest Zone to those living in adjacent locations but in the moderate West Zone. Similar to designs in Gonzalez (2021) and Lehner (2024), the regression takes the form:

$$Y_{icgt} = \alpha + \beta \times Genocide_c + f(Geo_c) + \lambda \mathbf{X}_c + \gamma \mathbf{D}_c + \delta_g + \tau_t + \epsilon_{icgt} \quad (4.1)$$

where Y_{icgt} is the outcome variable of interest for a woman i in DHS village c , segment g in survey year t , and $Genocide_c$ is an indicator equal to 1 if a woman is currently located in an area that was part of the extremist Southwest Zone during the Khmer Rouge era and equal to zero if she resides in an area formerly in the moderate West Zone. The function $f(Geo_c)$ is the RD polynomial controlling for smooth functions

of geographic locations of DHS village c . $\mathbf{X}_c$ is a vector of covariates, control for all pre-genocide characteristics.¹⁷ $\mathbf{D}_c$ is a vector of geographic covariates, controlling for distance to the capital of Cambodia - the largest urban city, distance to Vietnam borders and distance to Thailand borders. δ_g is 15-km segment fixed effects which control for any systematic differences that might exist along the border itself. Finally, τ_t is survey-year fixed effects. Standard errors are clustered at the DHS village level.

The RD polynomial $f(Geo_c)$ uses a uni-dimensional measure, in particular, distance to the border between the West Zone and the Southwest Zone as a running variable. Following Gelman and Imbens (2019), I select a local linear RD polynomial for the baseline specification. I examine specifications with higher-order RD polynomials in robustness checks. The local linear polynomial has a function as $f(Geo_c) = \eta dist_c + \theta Genocide_c \times dist_c$ with the forcing variable $dist_c$ denotes the Euclidean distance between a village location and the closest point on the border. Higher-order polynomials take the following form: $f(Geo_c) = \sum_{k=1}^a \eta_k dist_c^k + \theta_k Genocide_c \times dist_c^k$. The interaction term of the treatment variable ($Genocide_c$) with the distance to the border ($dist_c$) is included in all specifications, allowing for different slopes of the function on two sides of the zone border.

In all regressions, I employ a triangular kernel weighting function with the weight assigned to each observation decreasing as the distance from the border increases.

In terms of bandwidth selection, Table 4.1 presents the data-driven optimal bandwidths for the main outcomes following Calonico et al. (2020). The bandwidths are estimated using either a local linear or a quadratic polynomial. I select a bandwidth roughly equal to the average optimal bandwidth across all these outcomes. Particularly, the estimation sample is restricted to individuals within a bandwidth of 30 kilometers around the zone border. All tables in this paper report the estimates using a 30km bandwidth unless stated otherwise. I examine samples with other bandwidth restrictions in robustness checks.

In Appendix C.2, I use a multi-dimensional RDD to cross-verify the results. In

¹⁷Pre-genocide characteristics include geographic characteristics (elevation, tropics/lowland), demographic characteristics (population density in 1970) and economic characteristics (agricultural activities and distance to main roads/railways in 1970)

Table 4.1: Optimal bandwidths for RDD

Outcome	Linear	Quadratic
	(1)	(2)
<i>Panel A: Women's empowerment</i>		
Full-time Working	13.185	22.135
Decision Making	16.902	22.076
Partner Control	42.712	39.400
<i>Panel B: Domestic Violence</i>		
Physical Violence	44.747	29.368
Emotional Violence	26.745	37.100
Domestic Violence (Either)	24.838	31.782
Domestic Violence (Both)	43.862	36.243
Attitude (Consent)	24.209	21.883

Note: The table shows the mean square error optimal bandwidths of main outcomes following Calonico et al. (2020) – CCT bandwidths with a local linear polynomial or a quadratic polynomial. Column (1) presents the optimal bandwidth for each outcome with a local linear polynomial of distance to the border, while column (2) shows the results with a quadratic polynomial control.

the multi-dimensional RDD, RD polynomial includes demeaned x- and y-coordinates of household locations as running variables $f(Geo_c) = x + y$.

4.4.2 RDD Assumptions

Assumption 1: Continuity Assumption. The first key assumption for a valid RDD is the smooth variance of all covariates at the border. More precisely, if $E[c_1|dist]$ and $E[c_0|dist]$ represent the potential outcomes under treatment and control, respectively, and $dist$ denotes distance to the border; then both $E[c_1|dist]$ and $E[c_0|dist]$ must change smoothly at the discontinuity. The characteristics of individuals must be continuous across the treatment boundary, allowing individuals in the moderate (control) zone to act as a valid counterfactual group for those in the extremist (treated) zone.

I use regression 4.1 to assess the validity of the design by comparing pre-genocide geographic, demographic and economic characteristics across the boundary. Table 4.2 reports the results. All of these characteristics are measured at the DHS village level and serve as outcome variables in regression 4.1. I find no evidence of discontinuities in geographic, demographic, or economic characteristics at the border that separates

Table 4.2: Balance Check

	Dependent variable is:				
	Geography		Pre-genocide characteristics		
	(1) Elevation	(2) Tropics/Lowland	(3) Pop. Density	(4) Agriculture	(5) Dist. roads
Genocide	0.390 (1.683)	0.070 (0.065)	-0.001 (0.013)	0.092* (0.053)	0.485* (0.262)
Mean	21.63	0.546	0.808	0.682	3.223
Observations	464	464	458	464	464

Note: The unit of analysis is DHS villages. The sample is restricted to villages within a 30km bandwidth from the Southwest-West boundary. All regressions use a local linear polynomial in spatial coordinates with a triangular kernel. All regressions control for 15-km segment fixed effects and survey year fixed effects. *Tropics/lowland* is a dummy variable reflecting whether this location belongs to areas classified as “tropics, lowland” based on agro-ecological zones classification. I digitize three maps in Indochina Atlas, published in October 1970, to extract information on pre-genocide characteristics at DHS locations. *Pop. Density* is a binary variable reflecting whether the population density in 1970 was at least fifty inhabitants per square kilometre. *Agriculture* indicates whether there were any agricultural activities in these areas in 1970. *Dist. roads* refers to distance (in km) to 1970 main roads/railways. Heteroskedasticity-robust standard errors are reported in parentheses. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

the West Zone and the Southwest Zone. Particularly, the estimates for elevation and tropical/lowland areas are statistically insignificant, suggesting that geographic features vary smoothly across the boundary (columns 1–2). Additionally, there is no statistically significant difference in 1970 population density between the two sides of the border (column 3). Although pre-genocide agricultural activity is marginally imbalanced at the 10% level (column 4), the difference disappears once controlling for distances to Vietnam border, Thailand border, and the capital, suggesting that this imbalance was driven by spatial location rather than systematic selection. In terms of distance to 1970 main roads and railways, I observe a weakly significant discontinuity at the 10% level, though the effect size is small (column 5). The distance to 1970 main roads and railways was just 0.5km longer for those in the extremist Southwest Zone.

Assumption 2: No manipulation. The second assumption for a valid RDD is no manipulation across the border, meaning that individuals could not sort themselves around the cut-off border. This assumption would be violated if individuals during the Khmer Rouge regime were able to sort themselves around locations where repression

was more or less intense. Historical evidence indicates that during the genocide, people were forcibly relocated and had no freedom to decide where they lived. After the Khmer Rouge seized power, urban people were forced to migrate to the countryside, and rural peasants were also forcibly relocated to other places (Kiernan, 2002b). In addition, Table 4.2 shows that pre-genocide population density was balanced across the border with no statistically significant differences (column 3).

Assumption 3: Stable Unit-Treatment Value Assumption (SUTVA). A violation of SUTVA at the boundary can introduce bias into spatial RD designs. SUTVA holds when a unit’s outcome does not depend on the treatment status of nearby units (Keele and Titiunik, 2015; Rubin, 1986). In our case, SUTVA is violated if the outcomes of a woman residing near the border in the West Zone are influenced by the treatment status or outcomes of individuals living just across the boundary in the Southwest Zone. A potential concern is migration from treated areas (the high-repression Southwest Zone) to control areas (the moderate West Zone), which could bias the results. If individuals adversely affected by the genocide relocated to control regions, this may depress outcomes there and obscure the true effect of the genocide. Therefore, any potential violations of SUTVA in my RDD design would likely bias the estimated effects toward zero. Additionally, this spillover is unlikely, as post-genocide population density remains similar across the border (Table C.9). Moreover, in Section 4.6.2, *donut* exercises that remove observations close to the zone border show that the main results remain robust and become more efficient.

4.5 Genocide repression across Southwest-West Zones Border

I begin with some motivating analyses to examine the difference in genocide repression across the Southwest-West Zone Border. In Section 4.2, I outline historical evidence highlighting the sharp contrast in genocide intensity on two sides of the border. In this section, I use the Cambodian Genocide Databases with detailed information on 309

geo-coded locations of 18,953 mass graves during Democratic Kampuchea to provide empirical evidence for the validity of this border.

Table 4.3: Genocide repression across the Southwest-West Zone Border

	Graves within 5-km radius			
	Dummy (YES)		Number of graves	
	(1)	(2)	(3)	(4)
<i>Panel A. DHS villages</i>				
Genocide	0.139** (0.058)	0.113** (0.056)	15.657** (7.070)	13.063* (6.983)
Mean	0.293	0.293	27.55	27.55
Observations	458	458	458	458
<i>Panel B. Census Villages</i>				
Genocide	0.034* (0.020)	0.037* (0.020)	10.307*** (3.207)	9.727*** (3.225)
Mean	0.289	0.289	30.67	30.67
Observations	2793	2793	2793	2793
Dist. to border	Yes	No	Yes	No
Dist. to border $\times$ Genocide	Yes	No	Yes	No
x- and y-coordinates	No	Yes	No	Yes

Note: The unit of analysis is villages. Dependent variable for columns (1) and (2) is a dummy variable indicating whether there are any mass graves within a 5-km radius of villages. The dependent variable for columns (3) and (4) is the number of graves within a 5-km radius. The sample is restricted to those within a 30km bandwidth from the Southwest-West border. All regressions control for pre-genocide characteristics, distance to the capital, distance to Vietnam and Thailand borders, and 15-km segment fixed effects. I also control for survey year fixed effects for DHS villages. Heteroskedasticity-robust standard errors are reported in parentheses. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

In particular, I count the number of graves within a 5-kilometer radius of DHS villages and construct a binary variable equal to 1 if there are any mass graves within that radius. Based on the main specification, I test whether graves are more commonly found in the extremist Southwest Zone compared to the moderate West Zone. Results are reported in Table 4.3, Panel A. I find that villages in the extremist Southwest Zone are 13.9 (or 11.3) percentage points more likely to have mass graves in their vicinity than villages in the moderate West Zone. This represents over 40% compared to the mean. Consistent with this, the number of graves also increases significantly for those

in the Southwest Zone, with 15.6 (or 13) more graves in their vicinity, a jump equal to half the average. These findings hold steady whether I control for DHS villages' distance to the border or their spatial coordinates.

I replicate the analysis using the 2008 Cambodia Population Census which provides geo-locations of all villages in Cambodia. Table 4.3 - Panel B reports the results. I find similar results that villages in the Southwest Zone are more likely to have mass graves in their vicinity compared to those in the West Zone. They also have a significantly higher number of graves with an increase of 10 more graves, over 30% relative to the mean.

Overall, these empirical analyses clearly illustrate that the intensity of repression was significantly different across the border between the Southwest and West Zones, potentially leading to long-lasting differences in other outcomes, including female empowerment and domestic violence. We now turn to the baseline results of this study.

4.6 Results

4.6.1 Baseline results

The main results of the spatial RD design are presented in Table 4.4 and 4.5. I examine the long-term impacts of genocide on female empowerment and domestic violence in Cambodia, two to four decades after the genocide ended. Across all specifications, I observe negative long-term effects of genocide on female empowerment and domestic violence. In areas where genocide was intense, women are less empowered and more likely to suffer from domestic violence, especially emotional violence, at present.

Table 4.4 reports the impacts of genocide on female empowerment. Overall, we can see that in the Southwest Zone with high repression intensity, women at present are less empowered in households. They are 4.2 percentage points less likely to work full-time, a drop of 6% compared to the mean, though only statistically significant at the 10% level. These women are also 7.2 percentage points less likely to make important household decisions, representing a 12.8% reduction relative to the average. They are also 5.8 percentage points more likely to be controlled by their partner, an increase of

Table 4.4: The long-term impacts of Genocide on female empowerment

	Dependent variable is:		
	(1) Fulltime Working	(2) Decision Making	(3) Partner Control
Genocide	-0.042* (0.023)	-0.072** (0.030)	0.058** (0.023)
Mean	0.709	0.563	0.126
Observations	11723	7023	2173
Clusters	459	459	366

Note: The unit of analysis is DHS survey respondents. The sample is restricted to those within a 30km bandwidth from the Southwest-West boundary. All regressions use a local linear polynomial of distance to the Southwest/West border with a triangular kernel weight. All regressions control for pre-genocide characteristics, distance to the capital, distance to Vietnam and Thailand borders, 15-km segment fixed effects and survey year fixed effects. Standard errors reported in parentheses are clustered at the DHS village level. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

46% compared to the mean likelihood. Figure 4.3 illustrates these findings graphically. We can see a clear drop in full-time working and decision-making, and a significant jump in partner control for women residing in the Southwest Zone.

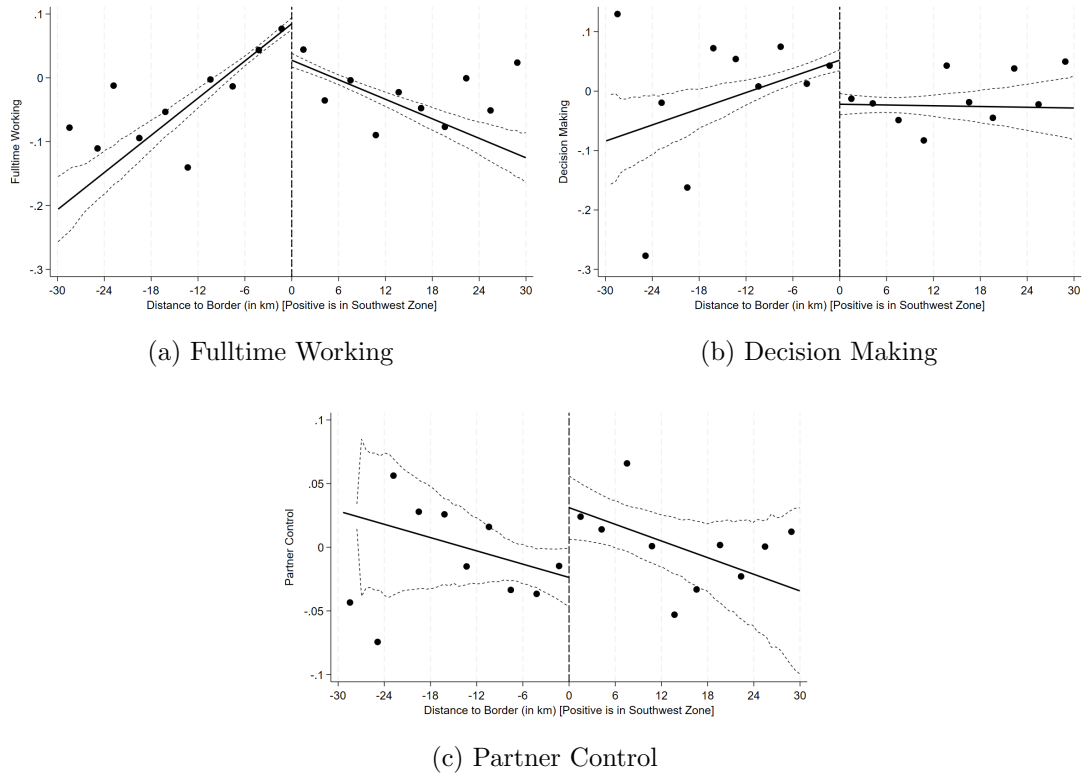
Table 4.5: The long-term impacts of Genocide on domestic violence

	Dependent variable is:				
	(1) Physical Violence	(2) Emotional Violence	(3) Violence (Either)	(4) Violence (Both)	(5) Attitudes (Consent)
Genocide	0.031 (0.022)	0.086*** (0.026)	0.067** (0.029)	0.049*** (0.018)	0.041** (0.021)
Mean	0.115	0.162	0.197	0.0802	0.322
Observations	2173	2173	2173	2173	10597
Clusters	366	366	366	366	459

Note: The unit of analysis is DHS survey respondents. The sample is restricted to those within a 30km bandwidth from the Southwest-West boundary. All regressions use a local linear polynomial of distance to the Southwest/West border with a triangular kernel weight. All regressions control for pre-genocide characteristics, distance to the capital, distance to Vietnam and Thailand borders, 15-km segment fixed effects and survey year fixed effects. Standard errors reported in parentheses are clustered at the DHS village level. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

In terms of domestic violence, in the extremist Southwest Zone, women at present are more likely to experience domestic violence (Table 4.5). Although there is no significant effect on physical violence, women living in high-repression areas are 8.6 percentage points more likely to experience emotional violence - an increase of over 50% compared to the average rate. Their likelihood of experiencing either physical or emotional violence increases by 6.7 percentage points, equivalent to 34% of the

Figure 4.3: Female Empowerment: RD plots

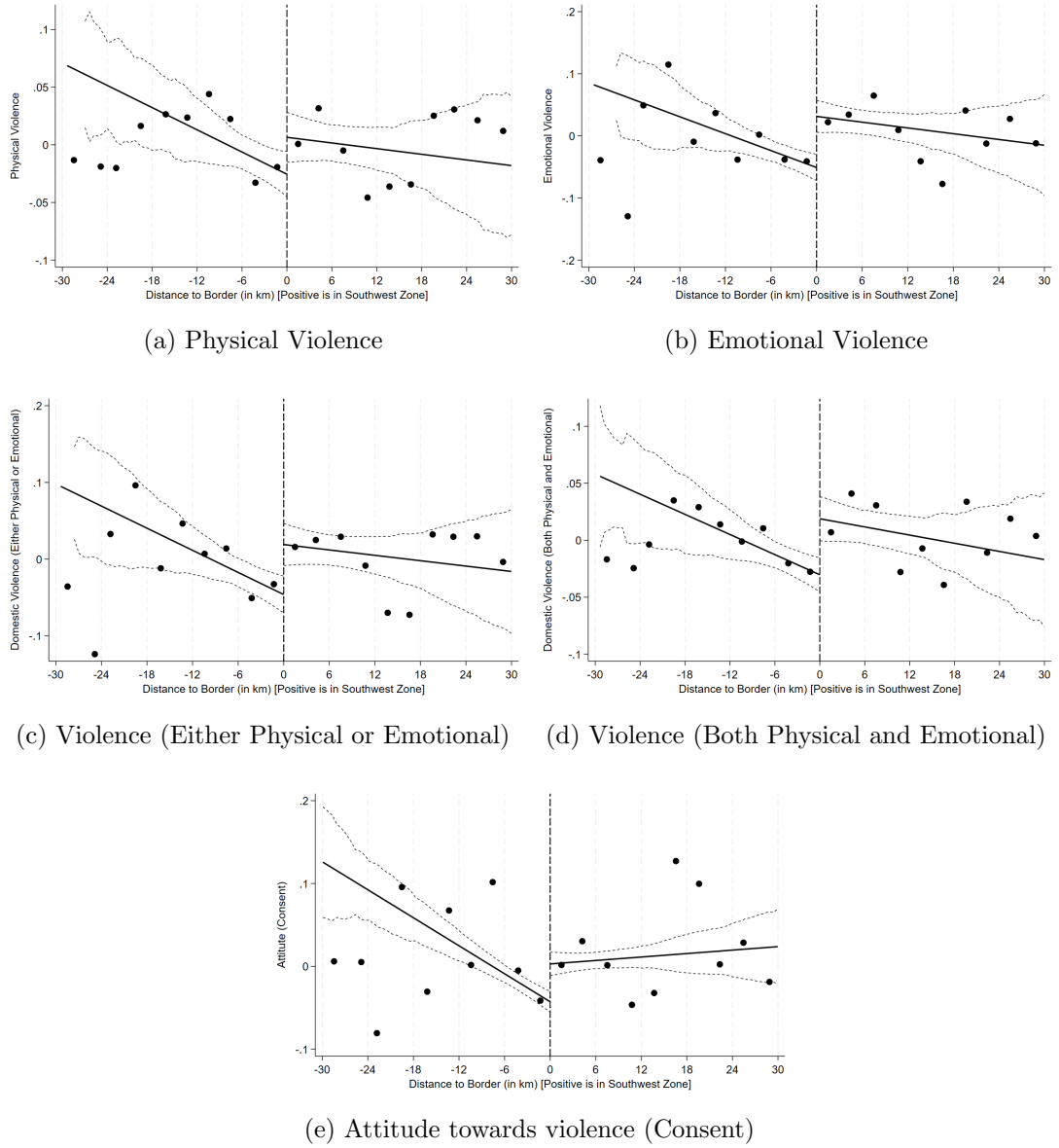


Note: The points represent binned residuals derived from a main regression of the outcome variable on a linear polynomial in distance to the border and other control variables. Solid lines depict local linear regressions, separately estimated on each side of the threshold, while dashed lines represent 95% confidence intervals. “Positive” values of distance indicate locations in the Southwest Zone.

mean, and the probability for them to face both types rises by 4.9 percentage points - representing an increase of 61% relative to the average. Especially, these women are more likely to justify domestic violence with a 4.1 percentage point increase in acceptance rate, equal to 12.7% of the mean. Figure 4.4 shows clear discontinuities in the likelihood of experiencing emotional violence, both forms of violence, and either form of violence.

These results are consistent with household bargaining model, which suggests that women’s increased access to resources reduces their exposure to domestic violence by enhancing their bargaining power within relationships (Meyer et al., 2024). In the case of Cambodia, genocide lowers women’s relative economic power and increases their vul-

Figure 4.4: Domestic Violence: RD plots



Note: The points represent binned residuals derived from a main regression of the outcome variable on a linear polynomial in distance to the border and other control variables. Solid lines depict local linear regressions, separately estimated on each side of the threshold, while dashed lines represent 95% confidence intervals. “Positive” values of distance indicate locations in the Southwest Zone.

nerability to domestic violence by weakening their negotiating position and heightening their financial dependence on male partners. The findings reinforce conclusions drawn in multiple studies, suggesting a robust relationship between women’s intra-household power and the presence of domestic violence (Aizer, 2010; Anderberg et al., 2016; Henke and Hsu, 2020).

4.6.2 Robustness checks

I conduct a wide range of robustness checks to assess the sensitivity of the results and address potential concerns associated with my RD design. I show that my results remain robust to different bandwidths, polynomial orders, segment fixed effects, alternative specifications, and placebo tests that shift the border toward different directions. These robustness results are presented in Appendix C.1. Additionally, a multidimensional RD design also produces results consistent with the main analysis (Appendix C.2).

Multi-dimensional RD design. In Appendix C.2, I show that a multi-dimensional RD design which controls for x- and y-coordinates of households instead of distance to the border yields similar results to my main design. All of the estimates are statistically significant and larger in magnitude. In high-repression areas, women are 5.4 percentage points less likely to work full-time, a larger and more significant effect than the 4.2 percentage point decrease found in the main analysis. These women are 7.1 percentage points less likely to make important household decisions, slightly smaller than 7.2 percentage points observed in the main analysis. The impact on partner control remains the same, with a 5.8 percentage-point decrease. In terms of domestic violence, there is still no effect on physical violence; however, women in the Southwest Zone are 8.7 percentage points more likely to face emotional violence, 6.9 percentage points more likely to experience either emotional or physical violence, and 5.1 percentage points more likely to suffer from both. These effects are statistically significant and similar to the main results. Additionally, these women are 4.9 percentage points more likely to accept domestic violence. This estimate is greater in magnitude than the one reported in the main analysis.

Choice of polynomial orders. Figures C.1 and C.4 display the main coefficients for different orders of RD polynomial in distance to the border. For female empowerment (Figure C.1), results remain consistent when using quadratic polynomials, but the effects disappear when cubic or higher-order polynomials are applied. This likely reflects overfitting rather than a true absence of effect, and therefore may not be a concern. In terms of domestic violence (Figure C.4), the results are consistent when higher orders of polynomials are used in the main regression, showing a higher likelihood of experiencing domestic violence among women in the high-repression zone.

Bandwidth sensitivity. I conduct sensitivity checks on different choices of bandwidth ranging from 15km to 45km with 1km intervals. Figures C.2 and C.5 show that the results are robust to different choices of bandwidths. For female empowerment outcomes, we may not observe significant effects with very narrow bandwidths, likely because smaller sample sizes reduce statistical power. The lack of significance at too narrow bandwidths is not a concern, as it reflects a common trade-off between bias and variance in RD designs. Regarding domestic violence outcomes, the estimates for domestic violence remain stable and robust across all bandwidth choices.

Segment fixed effects. The main specification includes border-segment fixed effects to ensure comparisons are made between treated and control units within the same local border segment, thereby controlling for unobserved geographic or socio-economic differences that may vary along the border itself and could otherwise bias the estimated treatment effect. The main specification uses 15-km border segment fixed effects, and I conduct a sensitivity analysis by varying the segment length from 5 km to 30 km with 5-km increments. Figures C.3 and C.6 plot the main coefficients under different segment fixed effect specifications, showing that the results remain consistent across various segment fixed effects.

Different specifications. I conduct robustness checks by exploring alternative specifications. The first column excludes the interaction between the treatment variable and the running variable – distance to the border. Column (2) excludes the distance to the

capital in the main regression. Column (3) presents estimates without a triangular kernel weight. Column (4) replaces segment fixed effects with province fixed effects. The last two columns conduct a *donut* exercise by excluding all observations within 1 km and 3 km of the border, respectively, and estimate the model using the remaining observations. This approach helps address potential concerns about systematic differences between populations living close to the border and those farther away.

Table C.1 reports the results on female empowerment. Across all specifications, the estimates remain consistently significant and stable, highlighting the robustness of the findings. Especially, in the more restrictive *donut* models (columns 5–6), the effects remain strong and significant. Women are 7.2 and 10.5 percentage points less likely to work full-time in the 1-km and 3-km *donut* exercises, exceeding the 4.2-point drop in the main analysis. While decision-making is not significant in the 1-km *donut* model, the 3-km *donut* exercise shows a significant 11.7 percentage point decline. Estimates for partner control are also larger and statistically significant compared to the main results. The findings on domestic violence are also robust to diverse specifications (Table C.3). In particular, the estimates for emotional violence, either type of violence and both types of violence remain stable and statistically significant across all specifications. However, we can only see marginal effects in accepting domestic violence, and these effects become insignificant in *donut* exercises.

Placebo tests. I perform placebo tests by shifting the border in two directions: Northwest and Southeast to verify that the observed treatment effect is not due to random variation or underlying bias.

Table C.2 presents the results of placebo tests for different female empowerment outcomes. I find some significant effects on full-time working when shifting the border northwest and southwest, but the direction and magnitude of these effects are inconsistent and do not reveal a clear pattern. About decision-making, there are no significant placebo-boundary effects. In terms of partner control, placebo tests shifting the border southeast show significant effects, but only at the 10% level. I find these significant results are not concerning as they lack a clear pattern and are expected by chance,

given the number of placebo tests. Table C.4 shows the placebo results across different measures of domestic violence. There are no significant placebo-boundary effects in all placebo tests, except positive effects on emotional violence and both types of violence in a 30-km northwest shift. Again, I do not find these significant estimates concerning, as they are isolated cases without a consistent pattern and are likely due to random variation, given the number of placebo tests conducted.

4.6.3 Heterogeneous effects

In this section, I examine the heterogeneous effects of the genocide by marital status and generation cohorts.

I disaggregate the effects by women’s marital status, dividing the sample into two groups: (1) women who are currently married or living with a partner, and (2) women who are never married, widowed, divorced, or separated.¹⁸ I expect that the impacts on female empowerment and domestic violence may differ depending on whether a woman is currently living with a partner.

Table 4.6 and 4.7 reports the results. The effects on full-time employment and household decision-making are statistically significant among women who are married or living with a partner (columns 1-2 in Table 4.6). Specifically, these women are 5.3 percentage points less likely to be employed full-time and 7.7 percentage points less likely to participate in major household decisions. In contrast, no significant effects are observed for women with other marital statuses. However, this group is more likely to experience controlling behaviours from male partners, although this finding should be interpreted with caution as it is based on a small sample and is only statistically significant at the 10 % level.

Concerning domestic violence, there are notable differences between the two groups (Table 4.7). We find no effects on the likelihood of experiencing physical violence among married or cohabiting women, but there is a marginally significant positive effect for women with other marital statuses. Women in both marital groups show increased

¹⁸Previously, the DHS domestic violence module focused only on ever-married women and their current or most recent partners. Starting with surveys implemented in 2021, the module has expanded to include never-married women if they report ever having had an intimate partner.

Table 4.6: Female Empowerment: Heterogeneous effects by marital status

	Dependent variable is:		
	(1) Fulltime Working	(2) Decision Making	(3) Partner control
<i>Panel A. Married or cohabiting women</i>			
Genocide	-0.053** (0.025)	-0.077** (0.032)	0.044* (0.022)
Mean	0.671	0.557	0.112
Observations	7277	6439	1899
Clusters	459	459	366
<i>Panel B. Other marital statuses</i>			
Genocide	-0.023 (0.025)	-0.011 (0.062)	0.139* (0.084)
Mean	0.770	0.631	0.219
Observations	4445	584	273
Clusters	457	152	178

Note: The unit of analysis is DHS survey respondents. The sample is restricted to those within a 30km bandwidth from the Southwest-West boundary. All regressions use a local linear polynomial of distance to the Southwest/West border with a triangular kernel weight. All regressions control for pre-genocide characteristics, distance to the capital, distance to Vietnam and Thailand borders, 15-km segment fixed effects and survey year fixed effects. Standard errors reported in parentheses are clustered at the DHS village level. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

likelihood of experiencing emotional violence, as well as experiencing both physical and emotional violence. However, the effects are bigger in magnitude for those with other marital statuses. As before, we interpret these effects with caution, given the small sample of women with other marital statuses.

I also split the effects by those born before and after the genocide ended. Table 4.8 shows the impacts of the genocide on female empowerment for different generations. The effect on full-time working is larger in magnitude for women born by 1979, but more significant for those born after. In terms of household decision-making power, women born by 1970 are 10.7 percentage points less likely to participate in important household decision-making. In contrast, no significant effects are observed among those born after the genocide. Regarding partner control, effects are significant only for those born after the genocide, with the likelihood of being controlled by a partner increasing by 6.2 percentage points, over 50% relative to the mean.

In terms of domestic violence (Table 4.9), we can observe significant effects only

Table 4.7: Domestic Violence: Heterogeneous effects by marital status

	Dependent variable is:				
	(1) Physical Violence	(2) Emotional Violence	(3) Violence (Either)	(4) Violence (Both)	(5) Attitudes (Consent)
Panel A. Married or cohabiting women					
Genocide	0.016 (0.023)	0.072*** (0.026)	0.054* (0.030)	0.034* (0.018)	0.035 (0.023)
Mean	0.104	0.150	0.182	0.0726	0.349
Observations	1899	1899	1899	1899	6413
Clusters	366	366	366	366	459
Panel B. Other marital statuses					
Genocide	0.125* (0.072)	0.176** (0.081)	0.151 (0.092)	0.150*** (0.057)	0.052** (0.026)
Mean	0.187	0.245	0.299	0.133	0.282
Observations	273	273	273	273	4184
Clusters	178	178	178	178	438

Note: The unit of analysis is DHS survey respondents. The sample is restricted to those within a 30km bandwidth from the Southwest-West boundary. All regressions use a local linear polynomial of distance to the Southwest/West border with a triangular kernel weight. All regressions control for pre-genocide characteristics, distance to the capital, distance to Vietnam and Thailand borders, 15-km segment fixed effects and survey year fixed effects. Standard errors reported in parentheses are at the DHS village level. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

among individuals born after the genocide. Individuals born by 1979 who experienced the genocide show a modest increase in the likelihood of experiencing emotional violence and both forms of violence, with effects only statistically significant at the 10% level. Meanwhile, those born after the genocide exhibit statistically significant increases in the likelihood of suffering from emotional violence, either physical or emotional violence and both forms of abuse. They are also more likely to accept domestic violence. These results show that the impact of the genocide varies across cohorts, with those born after the conflict facing greater risks of domestic violence, while less effects are observed for those directly exposed to the genocide. This divergence may reflect underlying mechanisms such as shifts in demographic structures, particularly changes in sex ratios and household composition, as well as the long-term disruption of gender roles caused by the genocide. These findings lead me to the following investigation of mechanisms that can possibly explain the story.

Table 4.8: Female Empowerment: Heterogeneous effects by age cohorts

	Dependent variable is:		
	(1) Fulltime Working	(2) Decision Making	(3) Partner Control
<i>Panel A. Born by 1979</i>			
Genocide	-0.061* (0.033)	-0.107*** (0.033)	0.042 (0.037)
Mean	0.583	0.638	0.136
Observations	5107	2894	970
Clusters	455	454	327
<i>Panel B. Born after 1979</i>			
Genocide	-0.039** (0.019)	-0.054 (0.037)	0.062** (0.026)
Mean	0.805	0.511	0.117
Observations	6616	4129	1203
Clusters	458	443	270

Note: The unit of analysis is DHS survey respondents. The sample is restricted to those within a 30km bandwidth from the Southwest-West boundary. All regressions use a local linear polynomial of distance to the Southwest/West border with a triangular kernel weight. All regressions control for pre-genocide characteristics, distance to the capital, distance to Vietnam and Thailand borders, 15-km segment fixed effects and survey year fixed effects. Standard errors reported in parentheses are at the DHS village level. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

4.7 Mechanisms

In this section, I explore several possible mechanisms that can explain the long-term impacts of genocide on female empowerment and domestic violence. First, decreased female empowerment and increased domestic violence may be partially attributed to skewed sex ratios and increased fertility rates following the genocide. Second, I provide evidence that the genocide significantly affects household structure and household wealth, with households in high repression areas having more children and poorer today. Finally, changes in gender-related characteristics further help explain reduced female empowerment and increased vulnerability to domestic violence. Both men and women are more likely to marry at a younger age and have lower educational attainment. Women are more likely to participate in the labor force, though predominantly through part-time or irregular employment. In addition, men are more likely to engage in multiple marital unions.

Table 4.9: Domestic Violence: Heterogeneous effects by age cohorts

	Dependent variable is:				
	(1) Physical Violence	(2) Emotional Violence	(3) Violence (Either)	(4) Violence (Both)	(5) Attitudes (Consent)
Panel A. Born by 1979					
Genocide	0.046 (0.040)	0.081* (0.041)	0.065 (0.046)	0.062* (0.033)	0.045 (0.032)
Mean	0.152	0.191	0.236	0.108	0.414
Observations	970	970	970	970	3338
Clusters	327	327	327	327	456
Panel B. Born after 1979					
Genocide	0.027 (0.024)	0.088*** (0.028)	0.074** (0.033)	0.040** (0.020)	0.041* (0.021)
Mean	0.0843	0.139	0.165	0.0581	0.280
Observations	1203	1203	1203	1203	7259
Clusters	270	270	270	270	435

Note: The unit of analysis is DHS survey respondents. The sample is restricted to those within a 30km bandwidth from the Southwest-West boundary. All regressions use a local linear polynomial of distance to the Southwest/West border with a triangular kernel weight. All regressions control for pre-genocide characteristics, distance to the capital, distance to Vietnam and Thailand borders, 15-km segment fixed effects and survey year fixed effects. Standard errors reported in parentheses are at the DHS village level. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

Change in Sex Ratios and Population Structure. I investigate post-genocide population ratios using the 2008 Population Census. Table 4.10 presents the results. First of all, average sex ratios across most age groups in Cambodia are much lower than the world average,¹⁹ indicating fewer men than women in the country. Sex ratios are comparable on two sides of the West–Southwest Zone border (columns 1-2), except for the population older than 65 years old (column 3). This divergence in the older age group aligns with our broader narrative, suggesting that the more extreme violence in the Southwest Zone resulted in higher male mortality during the Khmer Rouge period. However, after several decades, its effects on population structure have diminished over time, and demographic recovery has contributed to normalising sex ratios among younger cohorts.

Second, column 4 shows that the percentage of female-headed households is lower for villages in the high repression zone. In Cambodia, men are often traditionally

¹⁹Sex ratio is the number of males per 100 females. According to the 2024 Revision of the World Population Prospects (United Nations, Department of Economic and Social Affairs, Population Division, 2024), the global average sex ratio is approximately 101 males per 100 females, while in Asia, the ratio is higher at around 104 males per 100 females.

Table 4.10: Mechanism: Population Ratios (2008 Population Census)

	Dependent variable is: (Unit of analysis: census villages)					
	Sex Ratio			Other ratios		
	(1) Total	(2) 15-64	(3) 65+	(4) Female-headed	(5) Dependency	(6) Child-woman
Genocide	2.240* (1.189)	2.741* (1.425)	-5.042** (2.316)	-2.937*** (0.601)	1.405** (0.619)	10.559* (5.517)
Mean	93.83	91.16	70.58	27.07	58.21	355.3
Observations	2793	2793	2786	2793	2793	2793

Note: The unit of analysis is census villages. The sample is restricted to villages within a 30km bandwidth from the Southwest-West boundary. All regressions use a local linear polynomial of distance to the Southwest/West border with a triangular kernel weight. All regressions control for pre-genocide characteristics, distance to the capital, distance to Vietnam and Thailand borders, and 15-km segment fixed effects. *Sex ratio* is the ratio of males to females in the population (normalized to 100). *Female-headed* indicates the percentage of female-headed households. *Dependency ratio* represents the number of individuals who are economically inactive for every 100 economically active individuals in a population. *Child-woman ratio* measures the number of children under age 5 per 1,000 women of reproductive age. Heteroskedasticity-robust standard errors are reported in parentheses. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

listed as the head of the household. A woman may be the main decision-maker but still report a male as the head due to cultural norms or social expectations (Gorman et al., 1999; Ovesen et al., 1996). This pattern is also consistent with the demographic recovery following the genocide, as villages in the extremist zone exhibit slightly higher sex ratios (columns 1-2).

Third, for villages in the extremist Southwest zone, the dependency ratio, which is defined as the ratio of dependents (individuals under 15 and over 64 years old) to the working-age population (ages 15–64), increases by 1.405, which represents 2.4% rise compared to the mean (column 5). Similarly, the child-woman ratio, measured as the number of children under the age of five per 1,000 women aged 15 to 49, also increases by 10.559, equal to 3% of the mean (column 6). Together, these indicators suggest that villages in the high-repression Southwest Zone have experienced a relatively higher birth rate, reflecting a period of increased fertility following earlier population disruptions due to the genocide.

Household structure and household wealth. I use DHS data to empirically investigate how the genocide affects household structure and household wealth in the

Table 4.11: Mechanism: Household Characteristics (DHS)

	Dependent variable is: (Unit of analysis: DHS households)			
	(1) Female-headed	(2) Household size	(3) Kids ≤ 5	(4) Wealth quintile
Genocide	-0.011 (0.017)	0.006 (0.083)	0.047** (0.024)	-0.157** (0.067)
Mean	0.287	4.911	0.576	3.772
Observations	11923	11923	11923	9873

Note: The unit of analysis is DHS households. The sample is restricted to households within a 30km bandwidth from the Southwest-West boundary. All regressions use a local linear polynomial of distance to the Southwest/West border with a triangular kernel weight. All regressions control for pre-genocide characteristics, distance to the capital, distance to Vietnam and Thailand borders, 15-km segment fixed effects and survey year fixed effects. *Female-headed* is a binary variable indicating whether the household is headed by a woman. *Household size* denotes the number of household members. *Kids ≤ 5* indicates the number of children aged 5 and under. *Wealth quintile* indicates a household's relative economic status based on a wealth index derived from DHS data on assets (e.g, type of floor, water source, electricity, and ownership of durable goods). Households are ranked and divided into five equal groups, from the poorest 20% (first quintile) to the richest 20% (fifth quintile). Standard errors reported in parentheses are at the DHS village level. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

long run. Table 4.11 reports the results. Although I do not see significant effects on the likelihood of a household being female-headed or overall household size (columns 1-2), households in the high-repression Southwest Zone have more children aged 5 and younger, suggesting a higher fertility rate in this region (column 3). This is consistent with the effects observed at the village level in Table 4.10 above. In Cambodian culture, women have traditionally been expected to take care of children, and this norm continues to shape gender roles (Gorman et al., 1999). Higher fertility rates may contribute to women's lower likelihood of working full-time, reduced decision-making power within households, and increased exposure to domestic violence, as caregiving responsibilities limit their economic and social autonomy.

In addition, households are poorer if they are located in the Southwest Zone (column 4). This finding aligns with evidence from Grasse (2024), which shows that villages in the more extremist Southwest Zone are poorer today than those in the adjacent West Zone. Poor economic conditions can partly explain the reduction in female empowerment by increasing women's financial dependence on male partners and restricting

Table 4.12: Characteristics by Gender: Village-level analysis (2008 Population Census)

Dependent variable is: (Unit of analysis: census villages)				
	(1) Marriage Age	(2) Secondary	(3) Economic Active	(4) Unemployment
Panel A. Females				
Genocide	-0.291** (0.139)	-0.003 (0.004)	1.596*** (0.615)	-1.350*** (0.337)
Mean	23.60	0.0288	62.05	2.324
Observations	2793	2793	2793	2793
Panel B. Males				
Genocide	-0.334*** (0.121)	-0.015** (0.006)	0.589 (0.429)	-0.503** (0.215)
Mean	25.65	0.0696	61.72	1.707
Observations	2793	2793	2793	2793

Note: The unit of analysis is census villages. The sample is restricted to villages within a 30km bandwidth from the Southwest-West boundary. All regressions use a local linear polynomial of distance to the Southwest/West border with a triangular kernel weight. All regressions control for pre-genocide characteristics, distance to the capital, distance to Vietnam and Thailand borders, and 15-km segment fixed effects. *Marriage Age* is the Singulate Mean Age at Marriage (SMAM) - an estimate of the average age of women or men before first marriage (Hajnal, 1953). *Secondary* indicates the percentage of population who have completed secondary education. *Economic Active* is the percentage of population that is economically active. *Unemployment* indicates unemployment rate. Heteroskedasticity-robust standard errors are reported in parentheses. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

their access to education and employment. When household income is limited, families may prioritize boys' schooling, which reinforces traditional gender roles (Brereton et al., 2004; Gorman et al., 1999)

Change in gender characteristics. The genocide also leads to significant changes in gender characteristics. To examine these changes, I utilize data from both the 2008 Population Census (Table 4.12) and the Demographic and Health Surveys (Table 4.13).

Table 4.12 presents gender characteristics at the village level based on data from the 2008 Population Census. First, both women and men in the high-repression zone marry at younger ages (column 1). Second, while the genocide has no significant effect on the percentage of women completing secondary education (column 2 - Panel A), it has a significant negative impact on men's secondary education completion (column 2

Table 4.13: Characteristics by Gender: Individual-level analysis (DHS)

	Dependent variable is: (Unit of analysis: DHS individuals)							
	Education		Employment			Family		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Secondary	High Edu	Work	Fulltime	Seasonal	Never-Married	Union	Children
Panel A. Females								
Genocide	-0.011 (0.021)	-0.024** (0.012)	0.006 (0.015)	-0.042* (0.023)	0.042* (0.023)	-0.037** (0.014)	0.005 (0.010)	0.104* (0.053)
Mean	0.442	0.0662	0.785	0.709	0.291	0.329	0.0791	1.689
Observations	14935	14935	14932	11723	11723	14935	10003	14935
Panel B. Males								
Genocide	-0.030 (0.027)	-0.053** (0.025)	-0.003 (0.021)	-0.054* (0.028)	0.054** (0.028)	0.000 (0.026)	0.043** (0.019)	0.125* (0.075)
Mean	0.639	0.133	0.838	0.760	0.240	0.414	0.0842	1.355
Observations	4742	4825	4742	3125	3125	4742	2771	4825

Note: The unit of analysis is DHS survey respondents. The sample is restricted to those within a 30km bandwidth from the Southwest-West boundary. All regressions use a local linear polynomial of distance to the Southwest/West border with a triangular kernel weight. All regressions control for pre-genocide characteristics, distance to the capital, distance to Vietnam and Thailand borders, 15-km segment fixed effects and survey year fixed effects. *Secondary* denotes whether an individual has completed secondary education. *High Edu* represents attainment of education beyond the secondary level. *Work* is a binary variable capturing an individual's employment status. *Fulltime* indicates whether an individual is employed full-time. *Seasonal* denotes individuals engaged in seasonal, part-time, or occasional work. *Never-Married* is a binary variable denoting whether a woman is never married. *Union* is a binary variable indicating whether an individual has had more than one marital or cohabiting union. *Children* refers to the total number of children an individual has. Standard errors reported in parentheses are at the DHS village level. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

- Panel B). Historical narrative reports that educated men were more likely to be killed during the genocide (Gaikwad et al., 2023; Vickery, 1984), and this negative effect on men's education still persists decades after the regime ended. Table 4.13, based on DHS data, also reveals significant negative effects of the genocide on educational attainment for both males and females. Both genders are less likely to complete higher levels of education (columns 1-2 in Table 4.13), highlighting the long-term adverse impact of the genocide on human capital development.

Third, in terms of employment, Table 4.12 shows that females in the repression zone are more economically active and less likely to be unemployed (Panel A, columns 3-4). Meanwhile, although there is no significant effect on the percentage of economically active males, the male unemployment rate is also lower for villages in the high-intensity zone (Panel B, columns 3-4). These effects become clearer in Table 4.13, which shows

the empirical evidence from the DHS data. In the extremist Southwest Zone, women are less likely to engage in full-time employment but more likely to participate in part-time, seasonal, or occasional work (columns 4-5). I also observe a similar pattern among men. Overall, the shortage of men post-genocide has increased women's participation in the labor force, which aligns with evidence found in Buvinic et al. (2013); Gaikwad et al. (2023). However, this participation is more often in part-time, seasonal, or occasional employment rather than full-time work.

Finally, genocide also has long-term effects on union and marital patterns (Table 4.13). In terms of marital status, women residing in the Southwest Zone are 3.7 percentage points less likely to have never been married (column 6), implying they are more likely to enter a union, potentially due to post-genocide demographic imbalances and economic pressures that encouraged earlier or more widespread union formation. Regarding the number of unions, men in the high repression zone are 4.3 percentage points, equivalent to over 50% of the mean, and are more likely to have more than one marital or cohabiting union (column 7). In contrast, there are no statistically significant effects on the number of unions among women. The total number of children also increases for both genders, indicating higher fertility (column 8). Briefly, men in the extremist Southwest Zone are more likely to have multiple unions, whereas women typically have only one. These patterns undermine women's autonomy and heighten their dependency, since men's ability to remarry strengthens their leverage in relationships, while women's restricted options diminish their bargaining power.

In Appendix C.3, I empirically investigate forced marriage during the genocide. I find that women getting married during the genocide are more likely to know their husband on their wedding day and less likely to participate in choosing their spouse. Additionally, these women are more likely to be absent from their husbands' household books. However, I do not see statistically significant effects across the zone border. This can be due to the uniform implementation of marriage policies across different zones and limited sample sizes.

In summary, there are several mechanisms that can possibly explain how the genocide leads to decreased female empowerment and increased domestic violence in Cam-

bodia. First, the genocide resulted in notable demographic changes, including altered sex ratios and higher fertility rates. Second, the genocide appears to have lasting effects on household structure and economic conditions, as evidenced by higher numbers of children and lower household wealth in the high-repression zone. Third, the genocide brought about significant shifts in gender-related outcomes. Both men and women tend to marry at younger ages and attain lower levels of education. Women are more likely to participate in the labor force, though primarily in part-time rather than full-time employment. Men are more likely to have multiple marital unions. Together, these factors contribute to persistent gender inequality, with women facing lower empowerment and greater vulnerability to domestic violence after the genocide.

4.8 Conclusion

This paper answers the question of how violence and repression influence female empowerment and domestic violence in the long run, using evidence from the Khmer Rouge genocide in Cambodia. Drawing on multiple geo-coded datasets and a spatial regression discontinuity design, I show that the genocide has significant lasting negative effects on female empowerment and vulnerability to domestic violence. In high-repression areas, women today are less likely to work full-time, have reduced decision-making power, and are more often controlled by their partners. They are also more vulnerable to domestic violence, particularly emotional abuse.

I interpret these results as evidence that mass violence creates enduring gender inequality, which can be explained by a set of underlying mechanisms. First, decreased female empowerment and increased domestic violence can stem from skewed sex ratios and higher fertility rates following the genocide. The Khmer Rouge's violence caused high male mortality, and although younger generations are more balanced today, the sex ratio still remains much below the world average. At the same time, higher fertility reinforces women's traditional role as primary caregivers. Second, households in the more violent areas remain poorer today, which may restrict women's access to education and reduce their overall decision-making power within the household. Additionally, the

genocide leads to significant changes in gender characteristics, including earlier marriage and lower educational attainment for both men and women. Women are more likely to work, though mainly in part-time rather than full-time positions. Men often enter multiple unions or marriages, whereas women generally have only one. All these factors together contribute to persistent gender inequality, which in turn leads to lower women's empowerment and greater vulnerability to domestic violence.

The paper contributes to the literature on the long-term consequences of mass violence on gender inequality, with a focus on female empowerment and domestic violence. I provide empirical evidence that mass violence has lasting negative effects, consistent with home bargaining theory and with findings from Aizer (2010); Anderberg et al. (2016); Henke and Hsu (2020). The paper also contributes to a limited research on the legacy of the Khmer Rouge Genocide in particular. Most of these papers focus on economic outcomes, education, and health in the aftermath of the genocide, emphasising broad socioeconomic consequences (Grasse, 2024; Islam et al., 2017; Marshall et al., 2005; Nou, 2024). Limited attention has been given to how the genocide shapes gender inequality. The study most closely related to this research is the work of Gaikwad et al. (2023), which demonstrates that the intense violence of the Khmer Rouge, which disproportionately killed working-age men, resulted in lasting increases in women's economic autonomy and greater participation in local government. However, unlike that study, my focus is on women's bargaining power within the household and their vulnerability to domestic violence. Even if women seem more empowered in society through greater labor force participation, they remain less empowered at home, with weaker decision-making power and higher exposure to violence.

My findings have important implications for how mass violence shapes societies in the long run and for policies that aim to reduce gender inequality, strengthen women's empowerment, and address domestic violence. With nearly one in three women worldwide experiencing domestic violence (WHO, 2021), the 2030 UN Agenda for Sustainable Development set a global target (SDG 5.2) to eliminate all forms of violence against women and girls (Assembly et al., 2015). My results show that post-conflict recovery must go beyond rebuilding the economy to address the social and institutional legacies

of violence that limit women's opportunities in education, work, and family life. Any efforts to reduce gender inequality must ensure that women's empowerment extends beyond labor market participation to encompass greater autonomy and safety within households. Future research should investigate whether similar patterns of gender inequality emerge in other post-conflict settings. It would be valuable to examine how the legacies of violence are transmitted across generations, shaping gender norms and household dynamics over time. Another key direction is to evaluate how post-conflict recovery programs, such as those focused on education, labor market participation, or legal reforms, contribute to reducing gender inequality and protecting women from domestic violence.

Chapter 5

Conclusion

5.1 Summary

The thesis provides causal evidence on how large-scale shocks shape human capital formation, health, and inequality across generations. Bringing together three interconnected essays, it demonstrates that conflicts and state demographic interventions generate persistent effects that extend far beyond their immediate economic and social disruptions. These shocks leave enduring imprints on individuals, families, and communities, shaping life trajectories and influencing outcomes across generations.

Chapter 2 shows how the legacy of past destruction and unexploded ordnance (UXO) shapes the long-term recovery of communities. Leveraging the U.S. aerial bombing in Cambodia and a wide range of geocoded datasets, we provide evidence on two distinct channels through which conflict shapes long-term outcomes: a *risk* channel, where persistent UXO hazards continue to undermine health and development, and a *trigger* channel, where the absence of lingering threats creates opportunities for recovery and improved well-being. We find that more than three decades after the bombing, soft-soil areas with a high likelihood of UXO have worse health outcomes and weaker economic performance. In contrast, hard-soil areas with lower UXO risk exhibit better health outcomes and stronger economic performance. The chapter suggests that the legacy of conflict hinges on whether persistent hazards remain, and that failing to account for long-run health and development effects may underestimate the full benefits

of UXO clearance.

Chapter 3 examines the intergenerational effects of the One-Child Policy on the health outcomes of children whose parents were directly exposed to the policy. The policy reshaped mothers' family structures and improved their socioeconomic outcomes – lower fertility, higher education, and better health – which can carry over to the next generation. Our findings indicate that their children also exhibit better health outcomes, driven by better parental health, greater investment in children, and improved parenting practices. The chapter contributes to the literature on quantity-quality trade-offs and intergenerational transmission of health by demonstrating that fertility restrictions can generate positive spillovers to the next generation.

Chapter 4 investigates the long-term impact of the Cambodian Genocide on gender inequality, focusing on female empowerment and domestic violence. In areas exposed to higher levels of repression, women today are less empowered and more likely to experience domestic violence, particularly emotional abuse. These persistent effects are likely driven by skewed sex ratios, higher post-genocide fertility, lower household wealth, and shifts in gender-related characteristics, including earlier marriage, lower educational attainment, and multiple marriages for men. Overall, the chapter suggests that the demographic and economic disruptions of the genocide have had lasting consequences for gender norms and intra-household bargaining.

In conclusion, the thesis contributes to several strands of literature, including the economics of conflict, political economy, demographic economics, and health economics. It highlights the importance of adopting a long-term perspective when evaluating the costs of conflict and large-scale policy interventions.

5.2 Future Research

Future research should consider the long-term, multigenerational spillover effects of historical shocks and further clarify the mechanisms underlying their persistent impacts on health, human capital accumulation, and economic development. Combining historical and contemporary data and using richer longitudinal and administrative datasets that

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track families across multiple generations would help identify how these effects evolve over time. Importantly, future work should not only explore persistent effects but also examine the conditions that promote recovery, resilience, and reduce gender and health inequalities.

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Appendix A

Chapter 2 Appendix

A.1 Detailed description of the datasets

In this section, we provide detailed descriptions of the datasets used in this paper, including their sources, construction, and key variables.

A.1.1 Bombing and the identification of bombing areas

The bombing data used in this study was compiled by the Yale University Cambodian Genocide Program and provides information on 115,273 bombing sites targeted in Cambodia between October 1965 and May 1975. This dataset includes details such as the date of the bombing, precise locations, the number and type of aircraft involved in the sorties, bombing loads, and ordnance types.

We use the dataset to pinpoint regions heavily affected by bombing in the past, referred to as bombing areas. These designated areas must accurately capture the clustered patterns of bombing incidents, as areas beyond these boundaries are minimally impacted. To map out areas impacted by the bombing, we use a clustering analysis, which is utilized widely across various scientific disciplines, including geography, public health, and ecology (Aldstadt, 2009; Grubestic et al., 2014).

Spatial cluster detection integrates location attributes and events to detect meaningful patterns in geographical activities. In the fields of epidemiology and health-related sciences, clustering techniques help understand how location-specific features impact

health outcomes (Auchincloss et al., 2012; Beale et al., 2008; Elliott and Wartenberg, 2004; Rushton and Elliott, 2003). A common approach for identifying point clustering in the data space is to use grid cell densities (Ankerst et al., 1999), also known as quadrat analysis in the literature. This method creates a geographic histogram partitioning the data space into distinct, non-overlapping regions or cells. Cells with a significant number of objects signify cluster centers. Using this approach has several benefits (Boots and Getis, 1988). First, most of the points in the data space are used for the analysis. Second, it enables the identification of high-density regions in the data using square quadrats (or grid cells), which can be easily combined and merged into larger regions (Boots and Getis, 1988). However, the effectiveness of this method depends on the user-defined cell size, as small cells can lead to a noisy density estimate, while large cells may excessively smooth the density estimate (Ankerst et al., 1999; Cheng et al., 2018).

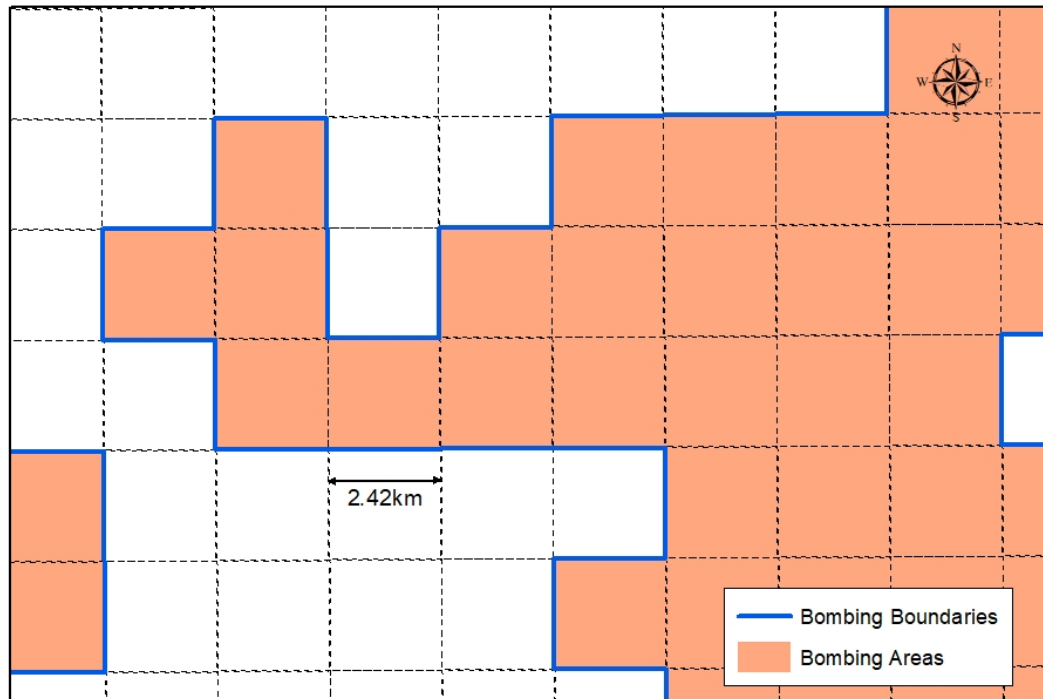
In our specific context, it is crucial that the designated bombing areas accurately capture the spatial patterns and distribution of bombing incidents. These areas must depict the geographical regions affected by the bombing, ensuring a precise representation of the impact zones. Based on this grid-based clustering technique, we divide the country map into geographic grid cells. We use an approach commonly employed by ecologists to identify the size of the grid cells. As outlined by Boots and Getis (1988), a suitable quadrat or grid size can be estimated as double the area per point, in particular: $I = \sqrt{2 \times A/n}$, where I denotes the calculated length of the side of a grid cell, A denotes the area of the focused region, and n denotes the number of features – airstrikes in our case – in the study area.

Our cell size equals 5.856 km^2 (2.42 km on each side), given Cambodia’s overall area being $337,561 \text{ km}^2$ and the number of airstrikes being 115,273.¹ In total, the country is divided into 31,799 grid cells.² After identifying the bombing loads in each cell,

¹This is similar to the cell size selected by Kohama et al. (2020) who examines how the economic characteristics of conflict zones influence the choice of military strategies.

²While this approach would produce about 57,600 cells based on the total national area, not all cells were suitable for analysis. Cells that fell outside the national boundary and overlapped permanent water bodies were removed. After applying these exclusions, 31,799 grid cells remained and were used in the analysis.

Figure A.1: Bombing boundaries and bombing areas



Notes: We exploit the spatial discontinuity around the bombing boundaries which separate areas exposed to past bombing from those that weren't. Map was drawn on ArcGIS.

only cells that have bombing loads greater than 0 are selected and defined as bombing areas. Figure A.1 demonstrates the way we construct bombing areas, zooming in on an actual portion of our data, and later bombing boundaries from the grid-cells. Figure 2.3 in Section 2.3 then shows the spatial distribution of identified bombing areas in Cambodia, which captures the clustering of strike locations, with areas outside these areas classified as unexposed.

A.1.2 Data on recovery

Health Index: Cambodia Demographic and Health Surveys

Our analysis builds upon four waves of the Cambodia Demographic and Health Surveys (DHS 2000, 2005, 2010, and 2014).³ We rely on the DHS individual women's data, as it offers extensive health information that is not available for the male sample. DHS surveys provide the geo-location of a cluster, which is a group of 25-30 households participating in the surveys. A household cluster in DHS can be considered an enumeration area, or a village in rural or urban areas. To keep respondents' confidentiality, GPS locations of clusters are displaced geospatially. Specifically, urban points are randomly displaced by up to 2 kilometers, while rural points are displaced by up to 10 kilometers. The randomness of this displacement ensures classical measurement error and thus unbiased estimates.

We follow Anderson (2008) to generate a standardized weighted summary *health index* of women comprising the following components:

1. *Height-for-age Z-scores (HAZ)*. HAZ is calculated by DHS using the WHO Growth Reference, reporting the difference between an individual's actual height and the median height of a reference population of the same age and sex.⁴ A below-median HAZ is an indication of stunting or malnutrition (Leroy and Frongillo, 2019).
2. *Weight-for-height Z-scores (WHZ)*. WHZ is also calculated by DHS using the WHO Growth Reference, measuring the number of standard deviations below or above the median normative weight for women of her age and weight. Very low weight-for-height z-scores indicate malnutrition, reflecting body weight substantially below the healthy reference level for a given height.
3. *Healthy Body Mass Index*. This variable is constructed based on women's Body

³Demographic and Health Surveys (DHS) Program has conducted six surveys in Cambodia, including CDHS 1998, 2000, 2005, 2010, 2014, and 2021-2022. Data on the exact locations of clusters or GPS data is not available in Cambodia DHS 1998. Meanwhile, DHS 2021-2022 does not provide information on anemia level, one of the outcomes analysed in this study.

⁴Height-for-age Z-score is calculated based on WHO Growth Reference tools, factoring in sex, age, and height data, with computations performed via WHO Anthro-based software. See https://dhsprogram.com/data/Guide-to-DHS-Statistics/Nutritional_Status.htm for more details.

Mass Index (BMI). BMI has been widely used in literature as an indicator of health status (Conti et al., 2024; Kountchou et al., 2019). Women are considered to have a healthy BMI if their BMI is between 18.5 and 25 kg/m² (Weir and Jan, 2019).

4. *Health Corpulence Index.* Corpulence Index (CI), also known as Ponderal Index (PI) or Rohrer’s Index, is a measure of body corpulence calculated as weight divided by height cubed, unlike BMI which normalizes by height squared. This adjustment makes the index more appropriate for very short or very tall individuals (Roth, 2018). We check the correlation between BMI and CI within our sample and find a moderate positive association ($r = 0.367$), indicating that while the two measures are related, they capture distinct aspects of body composition, which motivates us to use both indices. We then construct a variable called *Healthy CI*, which is defined as a CI between 11 and 15 (Mohajan, 2023).
5. *No Anemia.* Anemia is an indicator of inadequate nutrition and overall poor health, often associated with iron deficiency (WHO, 2008), that has been used as a health outcome in prior research (Aguilar and Vicarelli, 2011; Rosales-Rueda, 2018). Anemia status is classified by measuring hemoglobin levels, which are obtained through blood tests conducted by the DHS Program. Based on available DHS data, individuals are categorized into two groups: those with moderate or severe anemia and those with mild or no anemia.

By pooling information across multiple outcomes that capture the same latent dimension, a summary index reduces measurement error by averaging out idiosyncratic noise in individual outcome measures. We first recode each variable so that higher values indicate better health. We then standardize each variable and compute a weighted average. The weights are based on the inverse of the covariance matrix of the standardized variables, maximizing the information captured by the index (Anderson, 2008).

Post-bombing economic development data

We utilize the following datasets to evaluate post-bombing economic growth and infrastructure development. We begin by using data from the 2008 Population Census

and additional infrastructure datasets to construct an Economic Development Index that captures development at the census village level. We then draw on the Cambodia Socio-Economic Surveys from 2009 and 2014 to evaluate households' current levels of economic development.

Economic Development Index. Similar to the variable *Health Index*, we also use Anderson (2008)'s approach to construct a standardized weighted index of economic development based on the following components:

1. *School Locations in Cambodia (2012)*. We use this dataset to calculate the distance to the nearest school, which serves as a proxy for infrastructure development at the census village level. This dataset contains the geographic locations of schools as well as detailed information on preschools, primary schools, high schools, and colleges.⁵ We compute the distance from each census village to the nearest school. To ensure that higher values indicate better economic conditions, we reverse the sign of the distance variable so that larger values correspond to shorter distances to schools and, therefore, better economic development.
2. *Health facilities in Cambodia (2010)*. We use distance to hospitals as a proxy for health infrastructure development and healthcare accessibility. The dataset on health facilities provides comprehensive information on healthcare facilities in Cambodia, including national hospitals, referral hospitals, health centers, and health posts.⁶ We then calculate the distances from each census village to the nearest hospital. We reverse the sign of the distance measure so that higher values correspond to shorter distances and thus better economic development.
3. *Radio Station in Cambodia (2012)*. This dataset provides information on radio

⁵It was originally compiled by the Ministry of Education, Youth and Sports and later shared by the Office for the Coordination of Humanitarian Affairs (OCHA) through the Humanitarian Data Exchange (HDX). Open Development Cambodia (ODC) downloaded the data from HDX <https://data.humdata.org> in ESRI Shapefile format and converted it into other file formats. See <https://opendevelopmentcambodia.net> for more details.

⁶Cambodia's Ministry of Health (MoH) originally compiled the data, which was subsequently contributed to the Humanitarian Data Exchange (HDX) by the Office for the Coordination of Humanitarian Affairs (OCHA). Open Development Cambodia's team gathered data from Google Maps and utilized references from Cambodia's Ministry of Health. See <https://opendevelopmentcambodia.net> for more details.

stations across each province of Cambodia.⁷ Based on this dataset, we construct a dummy variable equal to 1 if census villages are located within a big radio network.

4. *Data from 2008 Population Census.* Using data from the 2008 Population Census data, we construct several indicators to measure development at the village level: (1) the percentage of households having access to improved water sources (including piped water, tube/pipe well, protected dug well and rain water); (2) the percentage of households having electricity as main source of light (including city power, generators, and both); (3) the percentage of households having toilet facility within premises; (4) dwelling tenure status (the proportion of households living in their own dwellings); (5) adult literacy rate (the percentage of literate individuals aged 15 and above among the total population aged 15 and above); (6) school attendance rate (the percentage of individuals (aged 6 and over) who are attending school); and (7) employment rate. Higher values of these indicators reflect higher levels of economic development.

Cambodia Socio-Economic Survey 2009 and 2014. To capture households' economic development at present, we use the Cambodia Socio-Economic Survey (CSES) 2009 and 2014, which are nationally representative surveys covering 12,000 households across 720 villages in 2009 and 12,096 households across 1,008 villages in 2014. The surveys contain information about households' economic activities, agricultural and non-agricultural incomes, expenditures, vulnerability to food shortages, field productivity, and risk-taking behaviors. Although CSES 2009 & 2014 do not provide geo-locations of households, we are able to geo-locate households using village coordinates provided by the 2008 Population Census. We selected the 2009 and 2014 waves because they use a roughly equal sampling scheme and size, and they come after the 2008 census.

⁷The data were collected from the fmscan.org website by Open Development Cambodia's mappers and were originally available in image format. See <https://opendevelopmentcambodia.net> for more details.

A.1.3 Pre-bombing data

We exploit several datasets from before the bombing or thereabouts for controls, heterogeneity dimensions, and balance testing.

Soil types in 1962. We use the data on the distribution of soil types in Cambodia provided by Crocker (1962) to identify pre-bombing soil fertility.⁸ Based on the discussion on soil type characteristics in White et al. (1997) and similar to the classification in Kohama et al. (2020), we create a dummy variable, Soil fertility in 1962, which equals 1 if a village is located in areas that are considered fertile (soft soil) and equals 0 otherwise.⁹ A detailed discussion on soil classification is provided in Appendix A.3.

1970 Indochina Atlas. We digitize three maps from the Indochina Atlas for location-specific measures of transportation links, agriculture, and population.¹⁰ The Indochina Transportation map depicts the major roads and railways of the country in 1970 (Appendix, Figure A.7). We geo-reference the map and measure the distance from a village to 1970 roads and railways. The Indochina Agriculture map outlines regions where agricultural activities took place (Appendix, Figure A.8). We then match this information with village locations to determine whether there were any agricultural activities at a village during the pre-bombing period. Finally, the Indochina Population map displays population density (persons per square kilometer) across Indochina (Appendix, Figure A.9), enabling us to identify population density at the village level in 1970. We argue these are “pre-bombing” characteristics, because although bombing started in the mid 1960s, the bombs escalated over a thousand times since 1970 (Owen and Kiernan, 2006), meaning that most of the strikes occurred after 1970 (see Figure 2.1).

⁸The data were provided to Open Development Cambodia in ESRI Shapefile format by Save Cambodia’s Wildlife’s 2013 Atlas Working Group. For more details, see <https://opendevelopmentcambodia.net>.

⁹Specifically, among sixteen different soil types given in Crocker (1962), the following six types of soils are classified as fertile: Latosols, Alluvial soils, Brown alluvial soils, Lacustrine alluvial soils, Regurs, and Brown hydromorphics (Kohama et al., 2020).

¹⁰These maps were released in October 1970 by the Directorate of Intelligence, Office of Basic and Geographic Intelligence, U.S. Central Intelligence Agency and are available at <https://maps.lib.utexas.edu>.

Global Agro-Ecological Zones (GAEZ). To assess the climate and potential crop productivity at the village level, we use the agro-ecological zones (AEZ) classification developed by the Food and Agriculture Organization of the United Nations (FAO) and the International Institute for Applied Systems Analysis (IIASA). This AEZ classification provides a comprehensive assessment of biophysical resources essential to agricultural production.¹¹ Based on the Dominant AEZ classification dataset, which is part of the GAEZ v4 Theme 1 Land and Water Resources, we match villages with their corresponding AEZ zone (Appendix, Figure A.6). Subsequently, we create a binary variable indicating whether a village is located in a grid cell characterized as tropics and lowland.

A.1.4 Data on unexploded ordnance

Mine/ERW casualties (2005-2013). In addition to the above datasets, we use data on Mine/ERW casualties (2005-2013) to test whether UXO is more prevalent in regions with pre-bombing softer soil. This dataset provides detailed locations of landmine and remnants of war (ERW) incidents. The data also reports the number of victims and casualties resulting from these incidents in Cambodia between 2005 and 2013.¹² In our analysis, we focus exclusively on incidents involving ERW likely originating from aerial bombing (CBU submunitions, fuses, and ammunition, other, and unknown) and exclude incidents related to landmines and other ground-delivered explosive weapons.¹³ We merge this dataset with 2008 Population Census data. We count the number of incidents within 3km of census village locations and construct a

¹¹The AEZ map incorporates thermal and moisture regimes, soil/terrain qualities, the presence of irrigated soils, and the identification of areas with significant bio-physical limitations such as extreme cold, arid deserts, steep terrains, and poor soil conditions. Geographical regions classified within the same AEZ category share similar climatic attributes, including rainfall and temperature patterns, which consequently contribute to comparable agricultural potentials. Several studies use GAEZ data to control for geographic characteristics or local agricultural suitability (e.g., Cagé and Rueda, 2016; Whatley and Gillezeau, 2011). Data can be accessed via <https://gaez.fao.org>.

¹²The data was compiled by The Cambodia Mine/ERW Victim Information System (CMVIS) of the Cambodian Mine Action and Victim Assistance Authority (CMAA) and shared via the Office for the Coordination of Humanitarian Affairs (OCHA) on the Humanitarian Data Exchange (HDX) platform. See <https://opendevelopmentcambodia.net> for more details.

¹³We note that UXO/ERW incidents can include those from separate causes than the US aerial bombing campaign (Martin et al., 2019; Roberts, 2011).

binary outcome equal to 1 if there are any incidents within this 3-km radius.

A.1.5 Descriptive Statistics

Table A.1 and A.2 report the summary statistics of variables used in this study. Table A.1 presents the variables measured at the DHS individual (DHS women) level. We see that health outcomes are, on average, comparable between individuals residing inside and outside bombing areas, with those living inside bombing areas exhibiting a slightly higher health index. Table A.2 reports the variables constructed at the census village level. Similarly, there are no significant differences in UXO likelihood or in the Economic Development Index between villages located inside and outside the bombing areas. Additionally, both tables show that the mean distance to Vietnam's borders is significantly lower for those inside the bombing areas, aligning with the historical narrative.

Table A.1: Descriptive statistics (*Unit: DHS women*)

	All observations				Within 2km distance			
	All	Outside	Inside	Diff	All	Outside	Inside	Diff
Panel A: Health outcomes								
Health Index	0.00 (0.42)	0.01 (0.42)	-0.01 (0.42)	0.02*** (4.73)	-0.01 (0.43)	-0.01 (0.42)	-0.02 (0.43)	0.01* (2.23)
Height-for-age Z-score	-1.81 (0.89)	-1.79 (0.88)	-1.84 (0.89)	0.05*** (4.94)	-1.83 (0.89)	-1.82 (0.88)	-1.85 (0.90)	0.03* (2.15)
Weight-for-height Z-score	-0.96 (0.96)	-0.93 (0.96)	-1.01 (0.97)	0.08*** (6.80)	-1.03 (0.96)	-1.00 (0.96)	-1.05 (0.96)	0.05** (3.15)
Healthy BMI ($18.5 \leq BMI < 25$)	0.71 (0.46)	0.71 (0.45)	0.70 (0.46)	0.01 (1.65)	0.70 (0.46)	0.71 (0.46)	0.70 (0.46)	0.00 (0.35)
Healthy Corpulence Index ($11 \leq CI < 15$)	0.69 (0.46)	0.69 (0.46)	0.70 (0.46)	-0.01 (-1.53)	0.71 (0.45)	0.71 (0.46)	0.71 (0.45)	-0.00 (-0.61)
No Anemia	0.91 (0.29)	0.90 (0.29)	0.91 (0.28)	-0.01* (-2.27)	0.91 (0.29)	0.90 (0.29)	0.91 (0.29)	-0.00 (-0.80)
Panel B: Pre-bombing characteristics								
Soil fertility (1962)	0.37 (0.48)	0.35 (0.48)	0.40 (0.49)	-0.05*** (-12.14)	0.36 (0.48)	0.33 (0.47)	0.39 (0.49)	-0.06*** (-11.62)
Elevation/Altitude (meters)	42.22 (76.74)	37.83 (59.67)	47.73 (93.53)	-9.90*** (-15.95)	40.63 (79.57)	36.57 (60.77)	44.72 (94.62)	-8.15*** (-9.26)
Tropics, lowland	0.55 (0.50)	0.49 (0.50)	0.62 (0.48)	-0.13*** (-33.75)	0.60 (0.49)	0.55 (0.50)	0.65 (0.48)	-0.10*** (-18.24)
Agricultural activities (1970)	0.93 (0.25)	0.92 (0.28)	0.96 (0.20)	-0.04*** (-23.56)	0.94 (0.23)	0.93 (0.25)	0.95 (0.22)	-0.02*** (-7.02)
Population density (1970)	0.75 (0.43)	0.68 (0.47)	0.83 (0.37)	-0.15*** (-46.80)	0.80 (0.40)	0.80 (0.40)	0.80 (0.40)	-0.00 (-0.07)
Distance (km) to roads/railways (1970)	6.51 (8.39)	7.42 (9.72)	5.36 (6.13)	2.05*** (33.56)	6.34 (6.91)	6.28 (7.04)	6.41 (6.77)	-0.14 (-1.78)
Panel C: Other characteristics								
Distance (km) to								
Vietnam borders	135.35 (116.53)	188.78 (124.39)	67.90 (55.09)	120.88*** (169.52)	89.28 (75.57)	105.18 (84.49)	73.22 (61.29)	31.96*** (39.21)
Capital	135.45 (101.41)	160.72 (103.31)	103.55 (89.28)	57.17*** (77.37)	103.11 (89.35)	104.84 (88.67)	101.37 (90.00)	3.46*** (3.51)
Thai borders	171.82 (83.50)	132.36 (81.45)	221.61 (54.34)	-89.25*** (-170.85)	202.37 (61.34)	189.33 (61.78)	215.54 (58.00)	-26.21*** (-39.59)
Observations	68506	38423	30083	68506	32750	16459	16291	32750

Note: The table provides the mean/standard deviation of the corresponding variables. "All" means the whole sample, "Outside" means the sample includes observations located outside bombing areas, and "Inside" means the sample includes observations located inside bombing areas. "Within 2 km distance" means the sample is restricted to observations located within 2 km of bombing boundaries. "Diff" shows the mean difference between treated (inside) and control (outside) groups. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix A. Chapter 2 Appendix

Table A.2: Descriptive statistics (*Unit: Census Villages*)

	All observations				Within 2km distance			
	All	Outside	Inside	Diff	All	Outside	Inside	Diff
Panel A: Unexploded Ordnance (UXO)								
UXO Likelihood	0.09 (0.29)	0.11 (0.32)	0.07 (0.25)	0.05*** (9.25)	0.07 (0.25)	0.07 (0.25)	0.06 (0.24)	0.00 (0.79)
Number of incidents	0.11 (0.41)	0.15 (0.49)	0.08 (0.30)	0.08*** (10.88)	0.08 (0.31)	0.08 (0.32)	0.07 (0.30)	0.01 (1.13)
Panel B: Post-bombing economic outcomes								
Economic Development Index	0.00 (0.36)	-0.03 (0.37)	0.03 (0.35)	-0.06*** (-9.17)	0.02 (0.32)	0.02 (0.33)	0.02 (0.31)	-0.00 (-0.52)
Distance (km) to School (2012)	0.91 (0.99)	0.92 (1.19)	0.89 (0.75)	0.03* (1.97)	0.92 (0.87)	0.91 (0.92)	0.92 (0.83)	-0.02 (-0.75)
Hospital (2010)	14.14 (11.87)	15.73 (13.68)	12.66 (9.64)	3.07*** (15.31)	13.81 (11.82)	14.16 (12.95)	13.53 (10.85)	0.63* (2.18)
Radio Station (big network)	0.89 (0.32)	0.86 (0.35)	0.91 (0.28)	-0.06*** (-10.53)	0.91 (0.28)	0.91 (0.28)	0.91 (0.28)	-0.00 (-0.37)
Percentage of households having Improved Water	49.67 (38.14)	46.68 (38.46)	52.46 (37.62)	-5.78*** (-9.00)	52.65 (38.90)	52.67 (39.26)	52.64 (38.62)	0.03 (0.03)
Electricity	16.25 (29.22)	19.62 (32.83)	13.09 (24.97)	6.53*** (13.21)	14.08 (27.64)	17.94 (32.32)	11.06 (22.90)	6.88*** (10.03)
Toilet within premises	25.57 (28.42)	27.62 (31.42)	23.65 (25.13)	3.96*** (8.23)	23.79 (27.84)	26.06 (31.01)	22.03 (24.96)	4.03*** (5.89)
Own dwellings	94.87 (8.98)	94.02 (8.67)	95.67 (9.20)	-1.65*** (-10.92)	95.55 (7.87)	94.81 (8.75)	96.12 (7.04)	-1.31*** (-6.77)
Literacy Rate	73.86 (17.43)	72.56 (18.29)	75.08 (16.50)	-2.52*** (-8.58)	74.46 (17.21)	74.11 (17.86)	74.74 (16.67)	-0.63 (-1.51)
School Attendance Rate	28.98 (7.05)	28.62 (7.24)	29.33 (6.84)	-0.71*** (-5.94)	29.59 (7.03)	29.36 (7.22)	29.77 (6.87)	-0.41* (-2.39)
Employment Rate	98.79 (3.09)	98.49 (3.53)	99.07 (2.58)	-0.58*** (-11.14)	98.93 (2.86)	98.65 (3.38)	99.14 (2.37)	-0.49*** (-6.90)
Panel C: Pre-bombing characteristics								
Soil fertility (1962)	0.39 (0.49)	0.39 (0.49)	0.39 (0.49)	0.00 (0.10)	0.36 (0.48)	0.36 (0.48)	0.36 (0.48)	-0.00 (-0.35)
Elevation/Altitude (meters)	34.01 (55.35)	36.66 (55.82)	31.53 (54.80)	5.12*** (5.49)	31.27 (56.02)	30.39 (48.48)	31.95 (61.27)	-1.57 (-1.20)
Tropics, lowland	0.52 (0.50)	0.46 (0.50)	0.57 (0.50)	-0.11*** (-13.00)	0.53 (0.50)	0.49 (0.50)	0.55 (0.50)	-0.06*** (-4.86)
Agricultural activities (1970)	0.95 (0.21)	0.94 (0.24)	0.97 (0.17)	-0.03*** (-9.52)	0.95 (0.21)	0.95 (0.22)	0.96 (0.20)	-0.01* (-2.16)
Population density (1970)	0.79 (0.40)	0.69 (0.46)	0.89 (0.31)	-0.21*** (-31.04)	0.86 (0.35)	0.85 (0.36)	0.87 (0.34)	-0.02* (-2.44)
Distance (km) to roads/railways (1970)	7.76 (8.90)	9.29 (10.63)	6.33 (6.60)	2.96*** (19.69)	7.47 (7.37)	7.62 (7.68)	7.36 (7.13)	0.26 (1.48)
Panel D: Other characteristics								
Distance (km) to Vietnam borders	123.87 (114.78)	190.26 (127.67)	61.80 (47.75)	128.46*** (78.03)	80.15 (66.99)	95.68 (77.60)	68.04 (54.38)	27.64*** (16.84)
Capital	113.19 (95.44)	153.75 (104.87)	75.26 (66.07)	78.49*** (52.71)	82.11 (71.51)	86.00 (74.85)	79.07 (68.64)	6.93*** (4.00)
Thai borders	184.52 (81.82)	138.06 (83.73)	227.96 (49.96)	-89.90*** (-76.69)	210.92 (57.29)	198.15 (59.11)	220.90 (53.77)	-22.75*** (-16.68)
Observations	14073	6800	7273	14073	7055	3092	3963	7055

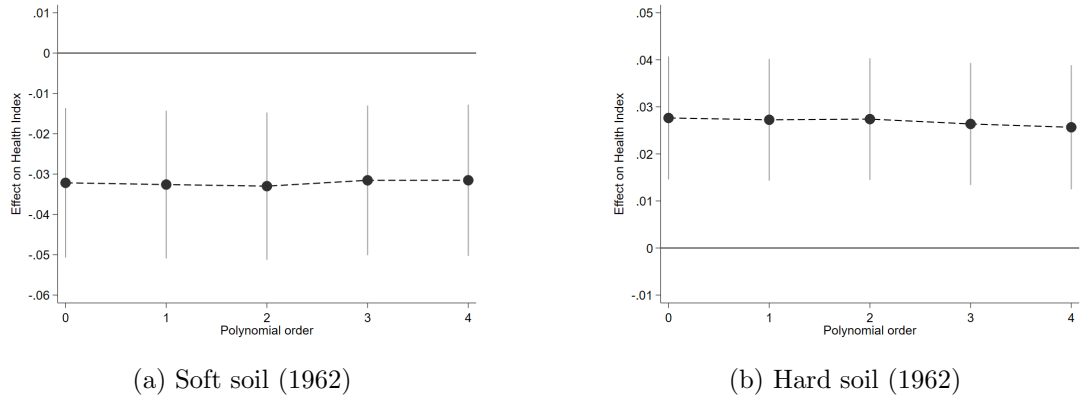
Note: The table provides the mean/standard deviation of the corresponding variables. "All" means the whole sample, "Outside" means the sample includes observations located outside bombing areas, and "Inside" means the sample includes observations located inside bombing areas. "Within 2 km distance" means the sample is restricted to observations located within 2 km of bombing boundaries. "Diff" shows the mean difference between treated (inside) and control (outside) groups. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.2 Robustness

We conduct multiple robustness checks to assess the sensitivity of the results and address potential concerns associated with our spatial RD design. We show that our results are robust to alternative bandwidths, different polynomial orders, different grid-cell fixed effects, a wide range of specifications including an uni-dimensional RDD, and placebo tests.

Choice of polynomial orders. Figure A.2 plots the main coefficients for different orders of RD polynomial in spatial coordinates. The results are consistent when higher orders of polynomials are used in the main regression with a substantial decline in the health index for soft-soil areas and consistently positive effects for hard-soil areas.

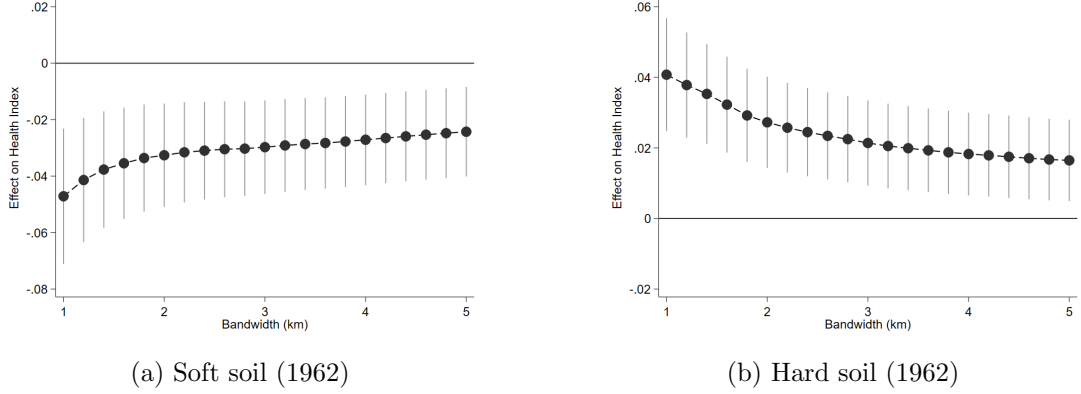
Figure A.2: Sensitivity of Results to Different Orders of Polynomial



Note: Dependent variable is Health Index. Each dot represents the RD estimate using the specified order of RD polynomial in spatial coordinates. Range spikes represent 90% confidence intervals of the estimates.

Bandwidth sensitivity. We conduct sensitivity checks to different choices of bandwidth ranging from 1 to 5 km with 0.2km intervals. Figure A.3 shows that our results are robust to any choice of bandwidths with negative health effects in soft soil regions and positive health effects in hard soil regions.

Figure A.3: Sensitivity of Results to Bandwidth Choice

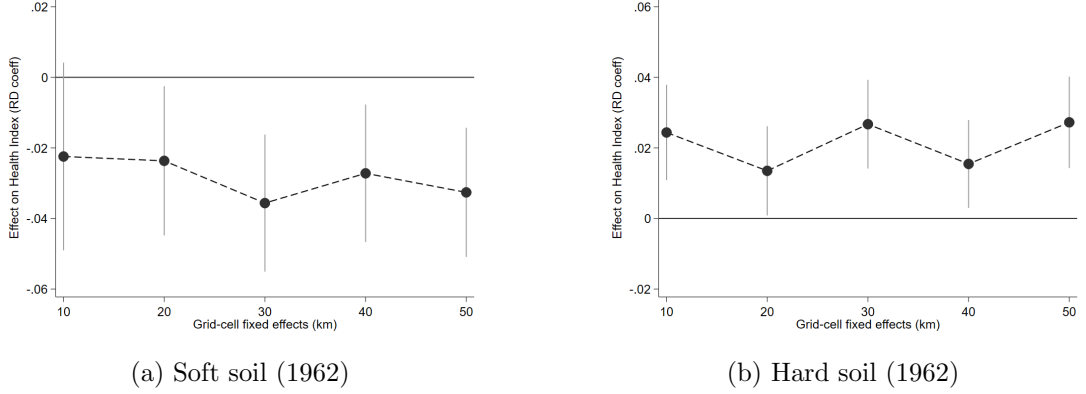


Note: Dependent variable is Health Index. Each sub-graph reports coefficient estimates and confidence intervals for different bandwidth levels ranging from 1 to 5 kilometers (horizontal axis) with 0.2km intervals. Each dot indicates the RD estimate using the specified bandwidth. Range spikes represent 90% confidence intervals of the estimates.

Grid-cell fixed effects. Spatial RD designs often include border-segment fixed effects to guarantee a comparison of observations in very close geographic proximity. Within our context, because there are numerous bombing borders widespread across the country, we employ 50x50km grid-fixed effects along with province fixed effects to ensure that we compare individuals within a confined area. However, we are also concerned that 50x50km grids may be too broad to account for between-area heterogeneity. Therefore, instead of using 50x50km grids to control for spatial variation, we divide the country into smaller grid-cells, ranging from 10x10km, 20x20km up to 50x50km, and control for grid-cell fixed effects in our main regressions. Figure A.4 presents our results. When smaller-grid fixed effects are employed, the results remain similar: living in bombing areas is associated with better health in hard-soil areas and worse health in soft-soil areas.

Uni-dimensional RD design. We use the uni-dimensional RD design to cross-verify our results. The regressions take the same form as our main specification. However, in this setting, RD polynomial $f(Geo_c)$ uses a uni-dimensional measure, in particular, distance to bombing boundaries as a running variable. The local linear polynomial has a function as $f(Geo_c) = \eta dist_c + \theta Bombing_c \times dist_c$ with the forcing variable

Figure A.4: Sensitivity of Results to Different Grid Fixed Effects



Note: Dependent variable is Health Index. Each dot represents the RD estimate controlling for a specified grid-fixed-effects. Range spikes represent 90% confidence intervals of the estimates.

$dist_c$ denoting the Euclidean distance between a community and the closest point on bombing boundaries. Higher-order polynomials will take the following form: $f(Geo_c) = \sum_{k=1}^a \eta_k dist_c^k + \theta_k Bombing_c \times dist_c^k$. The interaction term of the treatment variable with the distance to the bombing areas is of great importance because it allows for different slopes of the functions on two sides of bombing boundaries. Table A.3 shows that a unidimensional RDD yields similar results, with coefficients even larger in magnitude.

Alternative specifications. In Table A.4, we investigate robustness to a wide range of specifications. The first column controls for 50x50km grid fixed effects and district fixed effects instead of province fixed effects.¹⁴ Column (2) shows the results when observations are clustered at bombing grids instead of DHS villages. Column (3) includes the distance to Thai border among the control variables. The following column presents outcomes without a triangular kernel weight.¹⁵ Columns (5) (6) exclude the distance to the capital and the distance to Vietnam's borders in the main regressions. The last column runs a *donut* exercise by removing all observations close to bombing boundaries (within 0.1km) and keeping the remaining data to fit the current spatial RD model. The objective of the *donut* exercise is to address the potential issue of

¹⁴In our dataset, households are located in 176 districts.

¹⁵A triangular kernel involves decreasing the weight assigned to each observation as the distance from the boundaries increases.

Table A.3: Uni-dimensional RDD

	All areas		Soft soil (1962)		Hard soil (1962)	
	Linear (1)	Quad. (2)	Linear (3)	Quad. (4)	Linear (5)	Quad. (6)
Bombing	0.057*** (0.017)	0.076*** (0.026)	-0.045* (0.024)	-0.072** (0.035)	0.059*** (0.017)	0.079*** (0.027)
Bombing $\times$ Soft Soil	-0.090*** (0.030)	-0.142*** (0.044)				
Mean	-0.013	-0.013	-0.010	-0.010	-0.015	-0.015
Observations	17443	17443	6260	6260	11140	11140
Clusters	1060	1060	382	382	676	676

Note: The unit of analysis is DHS survey respondents. The sample is restricted to those living within 2km of bombing boundaries. All regressions control for 50x50km grid fixed effects, province fixed effects, distance to the capital, distance to Vietnam borders and other pre-bombing characteristics. Regressions (1) (3) (5) control for a linear polynomial in distance to the bombing boundaries. Regressions (2) (4) (6) control for a quadratic polynomial in distance to the bombing boundaries. Standard errors reported in parentheses are clustered at the DHS village level. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

systematic disparities between border populations and populations further away from bombing boundaries. Table A.4 shows that our estimates are robust in terms of magnitude and significance level across different specification choices. Even in the more restrictive *donut* model, the effects remain strong and significant.

Table A.4: Robustness checks: different specifications with x- and y- coordinates as running variables

	Dependent variable is Health Index						
	Grid-&District-FE (1)	Cluster(Bombing Grid) (2)	With Dist. Thai (3)	No weight (4)	No Dist. Capital (5)	No Dist. Vietnam (6)	Donut 0.1km (7)
Panel A: All observations							
Bombing	0.027*** (0.010)	0.025** (0.010)	0.024** (0.010)	0.016 (0.010)	0.025** (0.010)	0.025** (0.010)	0.018* (0.011)
Bombing $\times$ Soft Soil	-0.050*** (0.018)	-0.054*** (0.018)	-0.053*** (0.017)	-0.045*** (0.015)	-0.054*** (0.017)	-0.054*** (0.017)	-0.053*** (0.018)
Mean	0.0944	0.0944	0.0944	0.0944	0.0944	0.0944	0.0947
Observations	17453	17400	17400	17400	17400	17400	16181
Clusters	1062	848	1058	1058	1058	1058	981
Panel B: Soft soil (1962)							
Bombing	-0.028* (0.015)	-0.033** (0.014)	-0.033** (0.014)	-0.030** (0.013)	-0.032** (0.014)	-0.033** (0.014)	-0.034** (0.015)
Mean	0.0802	0.0802	0.0802	0.0802	0.0802	0.0802	0.0801
Observations	6260	6260	6260	6260	6260	6260	5824
Clusters	382	291	382	382	382	382	357
Panel C: Hard soil (1962)							
Bombing	0.026** (0.010)	0.027*** (0.010)	0.025** (0.010)	0.016* (0.010)	0.027*** (0.010)	0.027*** (0.010)	0.020* (0.010)
Mean	0.102	0.102	0.102	0.102	0.102	0.102	0.103
Observations	11193	11140	11140	11140	11140	11140	10357
Clusters	680	572	676	676	676	676	624

Note: The unit of analysis is DHS survey respondents. Regressions (1) control for grid-fixed effects and district-fixed effects (instead of province fixed effects). Regressions (2) show the estimates when observations are clustered at bombing grids instead of DHS villages. Regressions (3) additionally control for distance to Thai borders. Regressions (4) exclude the triangular kernel weight. Regressions (5) (6) exclude the distance to the capital and the distance to Vietnam's borders, correspondingly. Regressions (7) conduct a donut exercise that excludes observations within 0.1 km of bombing boundaries. Standard errors reported in parentheses are clustered at the DHS village level. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

Placebo tests. We conduct placebo tests to confirm that the treatment effect does not come from other factors such as random variation or bias. Placebo boundaries are created by shifting bombing areas by 5km in all directions (north/ east/ west/ south). Then, we re-assign treatment and estimate the treatment effects in placebo situations both for the aggregate effects and again split by 1962 soil conditions. As illustrated in Table A.5, there are no significant placebo-boundary effects on *health index* for both soft soil and hard soil areas.

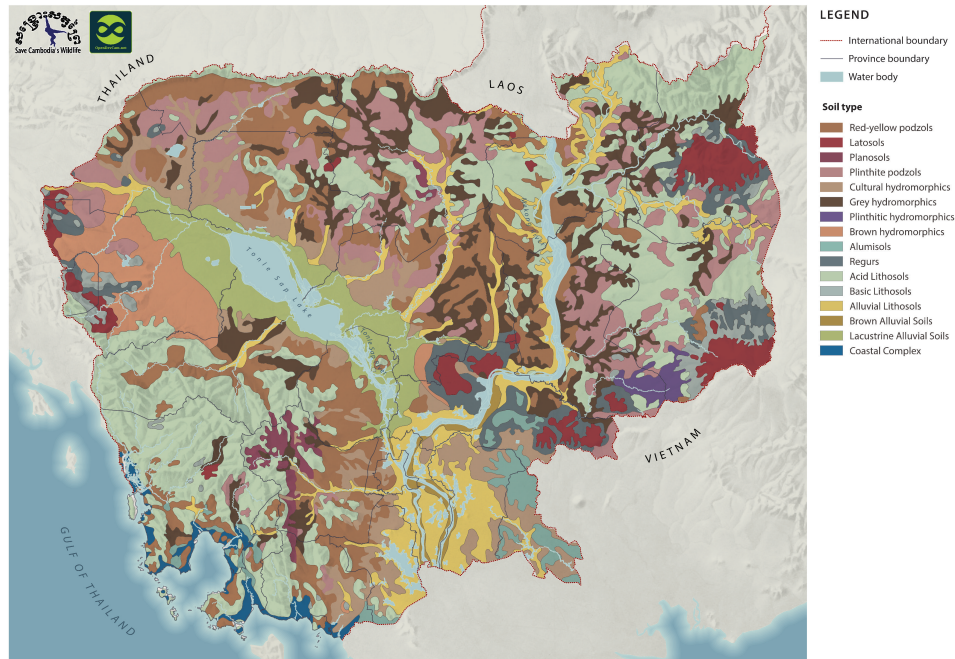
Table A.5: Robustness checks: Shifting borders

	Shift east (1)	Shift west (2)	Shift north (3)	Shift south (4)
<i>Panel A: All observations</i>				
Bombing	0.002 (0.010)	-0.010 (0.010)	-0.012 (0.010)	0.007 (0.010)
Bombing $\times$ Soft Soil (1962)	0.014 (0.017)	0.002 (0.016)	0.017 (0.016)	-0.012 (0.016)
Mean	0.401	0.389	0.405	0.398
Observations	18323	18327	18857	18339
Clusters	1113	1107	1144	1111
<i>Panel B: Soft soil (1962)</i>				
Bombing	0.011 (0.014)	-0.013 (0.015)	0.007 (0.013)	-0.004 (0.014)
Mean	0.439	0.410	0.415	0.382
Observations	7418	7004	7214	6548
Clusters	448	415	433	395
<i>Panel C: Hard soil (1962)</i>				
Bombing	-0.004 (0.010)	-0.011 (0.010)	-0.016 (0.010)	0.007 (0.010)
Mean	0.379	0.378	0.400	0.407
Observations	10905	11323	11643	11791
Clusters	665	692	711	716

Note: The table shows the results of placebo tests which shift bombing borders by 5 kilometers to four different directions: east-west-north-south. The unit of analysis is survey respondents. All regressions control for 50x50km grid fixed effects, province fixed effects, distance to the capital, distance to Vietnam borders and other pre-bombing characteristics. Standard errors reported in parentheses are clustered at the DHS village level. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

A.3 Soil classification in Cambodia

Figure A.5: Soil types in Cambodia (Crocker, 1962)



Notes: The map displays the distribution of soil types in Cambodia, as documented by Crocker (1962). The data, in vector format, were provided to Open Development Cambodia by Save Cambodia's Wildlife's Atlas Working Group. See <https://opendevelopmentcambodia.net> for more details.

Crocker (1962) carried out an exploratory soil survey in Cambodia, followed by the publication of a general soil map at a 1:1,000,000 scale in 1963 (Kyuma and Kawaguchi, 1966). The map classifies soil in Cambodia into 16 different soil types: red-yellow podzols, latosols, planosols, plinthite podzols, cultural hydromorphics, grey hydromorphics, plinthitic hydromorphics, brown hydromorphics, alumisols, regurs, acid lithosols, basic lithosols, alluvial lithosols, brown alluvial soils, lacustrine alluvial soils, and coastal complex (Figure A.5).

White et al. (1997) discuss the characteristics of each soil group in Cambodia and its potential for rice production. For example, Prey Khmer soils correspond to Red-Yellow Podzols and occasionally to Planosols, as identified by Crocker (1962). These

soils have low water-holding capacity and limited potential for high rice yields. On the contrary, Kompong Siem Soils which are Regurs in Crocker (1962) are considered fertile and well-suited for rice cultivation, with top soil having good water holding capacity.

Based on the characteristics of each soil type described in White et al. (1997) and similar to the classification in Kohama et al. (2020), we define these six soil types in Crocker (1962) as fertile (soft) soils: Latosols, Alluvial soils, Brown alluvial soils, Lacustrine alluvial soils, Regurs, and Brown hydromorphics. These soils often have good water-holding capacity and are suitable for rice production. Most importantly, since areas with soft or fertile soil are more likely to contain UXO (Lin, 2022; Moyes et al., 2002),¹⁶ we use this classification of pre-bombing soil fertility in our analysis to disentangle the long-term effects of local area exposure to bombing in areas with fertile (soft) soil - a high likelihood of UXO versus areas with infertile (hard) soil - lower UXO occurrences.

¹⁶See Section 2.2.

A.4 Additional Tables and Figures

Table A.6: Effects on health: Component Outcomes

	Dependent variable is:				
	(1) Height-for-Age	(2) Weight-for-Height	(3) Healthy BMI	(4) Healthy CI	(5) No Anemia
Panel A: All areas					
Bombing	0.082*** (0.025)	-0.008 (0.029)	0.013 (0.010)	0.005 (0.011)	0.017** (0.008)
Bombing $\times$ Soft Soil	-0.117*** (0.042)	-0.075 (0.049)	-0.015 (0.017)	-0.002 (0.019)	-0.021* (0.012)
Mean	-1.834	-1.026	0.705	0.708	0.906
$ \beta /\text{Mean}$	0.045	0.008	0.018	0.006	0.019
Observations	17369	13613	17368	17368	15307
Clusters	1058	948	1058	1058	1058
Panel B: Soft soil (1962)					
Bombing	-0.045 (0.034)	-0.081* (0.042)	0.003 (0.013)	-0.001 (0.016)	-0.008 (0.008)
Mean	-1.834	-1.001	0.697	0.691	0.920
$ \beta /\text{Mean}$	0.025	0.081	0.004	0.001	0.008
Observations	6249	4882	6249	6249	5476
Clusters	382	344	382	382	382
Panel C: Hard soil (1962)					
Bombing	0.074*** (0.026)	-0.010 (0.030)	0.015 (0.011)	0.007 (0.011)	0.018** (0.008)
Mean	-1.834	-1.039	0.709	0.717	0.898
$ \beta /\text{Mean}$	0.041	0.010	0.021	0.010	0.020
Observations	11120	8730	11119	11119	9831
Clusters	676	603	676	676	676

Note: The unit of analysis is DHS survey respondents. The sample is restricted to those living within a 2km bandwidth from bombing boundaries. All regressions use a local linear polynomial in spatial coordinates with a triangular kernel weight and control for 50x50km grid fixed effects, province fixed effects, distance to the capital, distance to Vietnam borders and other pre-bombing characteristics. *Healthy BMI* is defined as a BMI value between 18.5 and 25. *Healthy Corpulence Index (CI)* or Rohrer's Index is defined as a CI between 11 and 15. Standard errors reported in parentheses are clustered at the DHS village level. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

Table A.7: Economic Index (2008 Population Census Villages): Individual Outcomes

	Dependent variable is:									
	Dist. School (1)	Dist. Hospital (2)	Radio Station (3)	Improved Water (4)	Electricity (5)	Toilet (6)	Own Dwellings (7)	Literacy (8)	School Attendance (9)	Employment (10)
Panel A: All observations										
Bombing	-0.006 (0.026)	-0.224 (0.225)	0.000 (0.005)	-0.952 (1.114)	-0.851 (0.752)	-0.204 (0.781)	0.312 (0.225)	0.863* (0.473)	0.489** (0.207)	0.190** (0.087)
Bombing × Soft Soil	0.015 (0.044)	0.710* (0.375)	-0.011 (0.007)	-1.340 (1.911)	-1.219 (1.584)	-0.411 (1.449)	-0.144 (0.534)	-0.145 (0.771)	-0.273 (0.381)	-0.366* (0.199)
Mean	0.917	13.811	0.913	52.650	14.075	23.793	95.548	74.463	29.591	98.928
Observations	7054	7054	7054	7054	7054	7054	7054	7054	7054	7054
Clusters	1117	1117	1117	1117	1117	1117	1117	1117	1117	1117
Panel B: Soft soil (1962)										
Bombing	0.008 (0.031)	0.729*** (0.270)	-0.007 (0.004)	-2.159 (1.558)	-1.979 (1.366)	-0.878 (1.198)	-0.205 (0.419)	0.786 (0.611)	0.188 (0.298)	-0.162 (0.172)
Mean	0.794	11.754	0.918	57.454	20.100	29.134	93.688	76.099	29.388	98.490
Observations	2532	2532	2532	2532	2532	2532	2532	2532	2532	2532
Clusters	563	563	563	563	563	563	563	563	563	563
Panel C: Hard soil (1962)										
Bombing	-0.009 (0.026)	-0.248 (0.224)	0.001 (0.005)	-0.498 (1.093)	-0.597 (0.732)	-0.065 (0.769)	0.292 (0.205)	0.833* (0.470)	0.454** (0.208)	0.192** (0.087)
Mean	0.985	14.964	0.910	49.955	10.696	20.797	96.590	73.545	29.705	99.174
Observations	4517	4517	4517	4517	4517	4517	4517	4517	4517	4517
Clusters	836	836	836	836	836	836	836	836	836	836

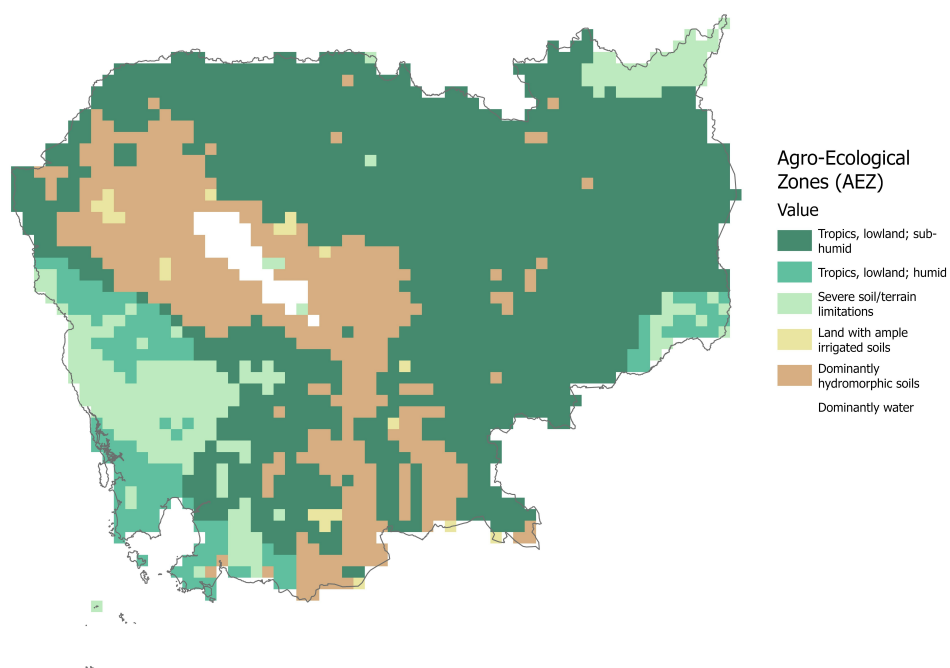
Note: The unit of analysis is census villages. All regressions use a local linear polynomial in spatial coordinates with a triangular kernel weight and control for 50x50km grid fixed effects, province fixed effects, distance to the capital, distance to Vietnam borders and other pre-bombing characteristics. *Economic Development Index* is the standardized weighted summary index variable created from multiple outcomes in this table. *Dist. School* indicates the distance from each village to the nearest school (in km). *Dist. Hospital* indicates the distance from each village to the nearest hospital (in km). *Radio Station* indicates location within big radio networks. The subsequent columns report the percentage of households in the village having access to improved water (column 4), electricity (column 5), and toilets (column 6); the percentage living in owner-occupied dwellings (column 7); literacy rate (column 8); school attendance rate (column 9); and employment rate (column 10). Standard errors reported in parentheses are clustered at the commune level. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

Table A.8: Conley Standard Errors

	Dependent variable is:		
	UXO Likelihood (1)	Health Index (2)	Economic Index (3)
Bombing	-0.015** (0.007)	0.025*** (0.004)	0.015* (0.008)
Bombing $\times$ Soft Soil	0.052** (0.024)	-0.054*** (0.019)	-0.045*** (0.017)
Mean	0.066	-0.013	0.023
Observations	7054	17400	7054

Note: All regressions use a triangular kernel weight and control for 50x50km grid fixed effects, province fixed effects, distance to the capital, distance to Vietnam borders and other pre-bombing characteristics. Standard errors are computed using Conley (1999)'s spatial correction. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

Figure A.6: Agro-ecological Zones classes



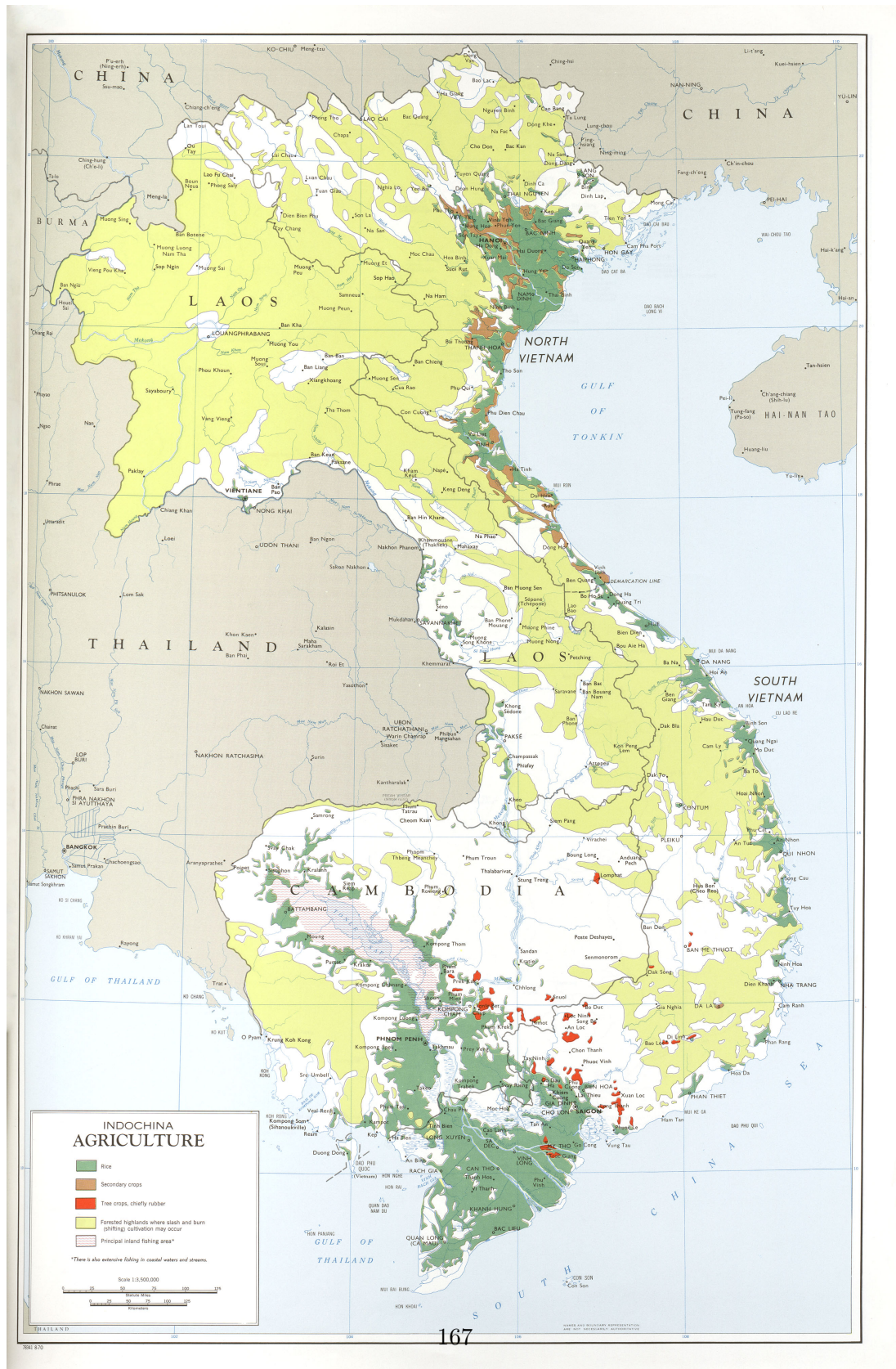
Notes: The map overlays Cambodia to the agro-ecological zones (AEZs) as classified by The Food and Agriculture Organization of the United Nations (FAO) and the International Institute for Applied Systems Analysis (IIASA). Geographic areas belonging to the same AEZ category exhibit analogous climatic characteristics, encompassing rainfall and temperature patterns, and thus possess equivalent agricultural capabilities. Map is drawn on ArcGIS.

Figure A.7: Indochina Transportation in 1970



Notes: Indochina Transportation map, which was in “Indochina Atlas”, published in October 1970 by the Directorate of Intelligence, Office of Basic and Geographic Intelligence, U.S. Central Intelligence Agency.

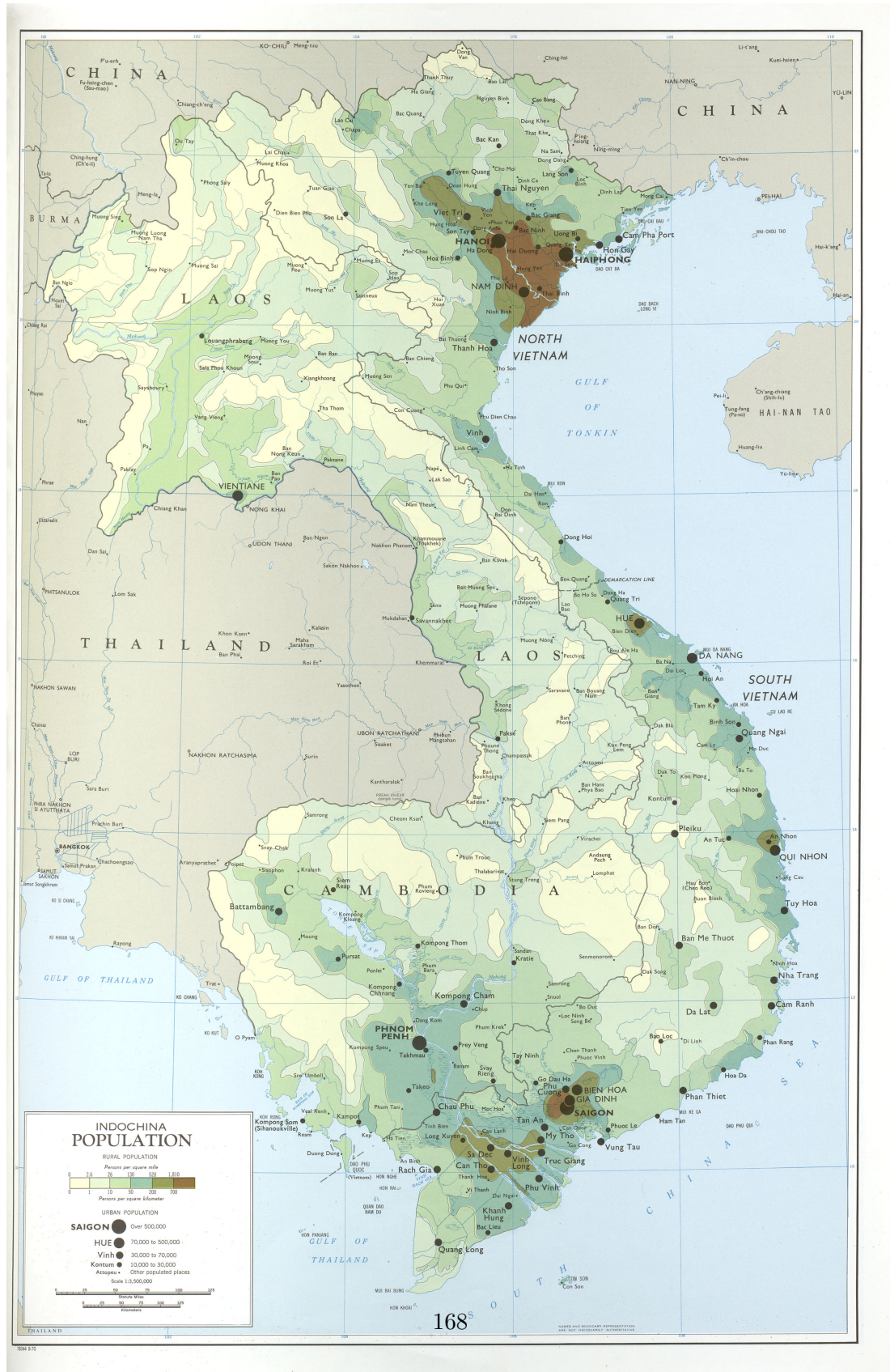
Figure A.8: Indochina Agriculture in 1970



Notes: Indochina Agriculture map, which was in "Indochina Atlas", published in October 1970 by the Directorate of Intelligence, Office of Basic and Geographic Intelligence, U.S. Central Intelligence Agency.

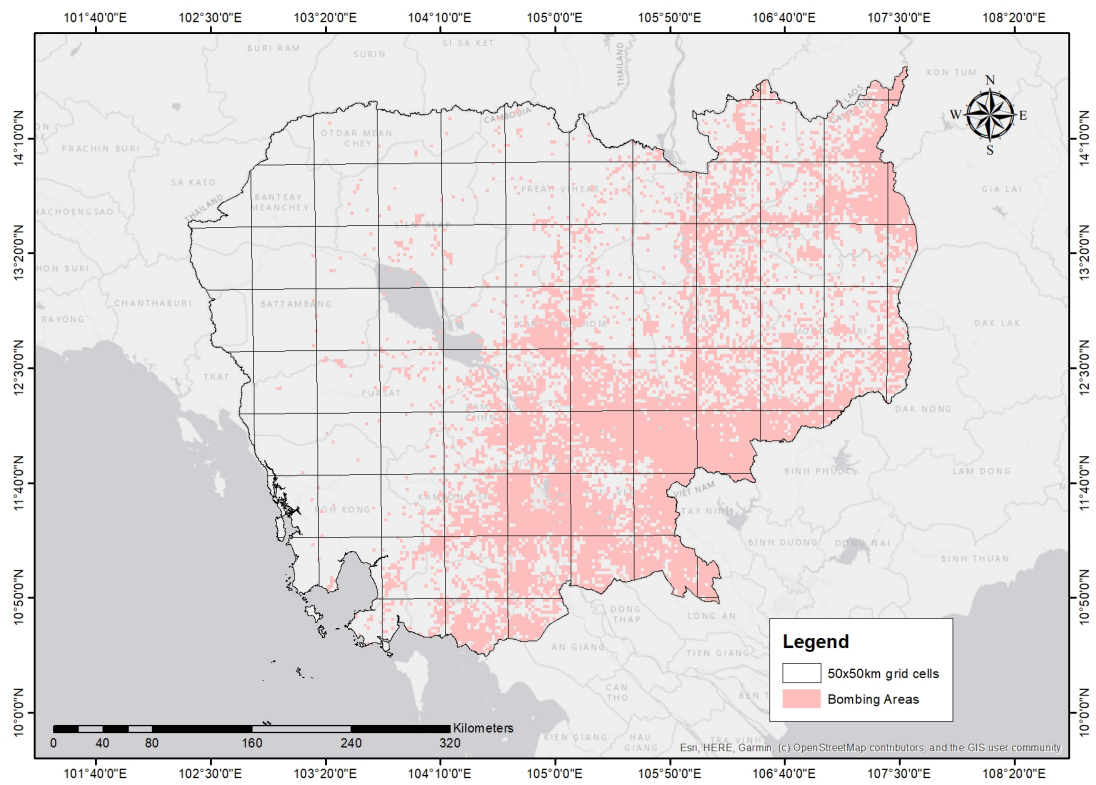
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Figure A.9: Indochina Population in 1970



Notes: Indochina Population map, which was in "Indochina Atlas", published in October 1970 by the Directorate of Intelligence, Office of Basic and Geographic Intelligence, U.S. Central Intelligence Agency.

Figure A.10: Generating 50x50km grid cells



Notes: The map illustrates how the country was divided into 50x50km grid-cells. Map overlaid on OpenStreetMap base map and drawn on ArcGIS.

Appendix B

Chapter 3 Appendix

B.1 Description of Psychological Scales

CFPS uses different scales of mental health distress in different survey waves. One indicator is the Kessler Psychological Distress Scale (K6), developed by Kessler et al. (2002), which was asked in the 2010 and 2014 surveys. Respondents reported their experiences in the past month on items in Table B.1. We reverse code each item to score as 0 (never), 1 (once a month), 2 (2-3 times a month), 3 (2-3 times a week) and 4 (Almost every day) and aggregate them to a final score ranging from 0 to 24, with higher scores indicating greater depressive symptoms. While a score of 13 usually defines serious mental illness (Kessler et al., 2003), we use a lower threshold of $K6 \geq 5$ to indicate moderate mental distress (Prochaska et al., 2012).

Another mental health indicator used in CFPS is the Center for Epidemiologic Studies Depression Scale (CES-D) (Radloff, 1977). The full 20-item CES-D was included in the 2012 and 2016 surveys, while an 8-item version was asked in the 2018 and 2020 surveys. Respondents rated their past-week status on items in Table B.1. Each item was scored as 1 (never(less than one day)), 2 (sometimes (1-2 days)), 3 (often(3-4 days)), and 4 (most of the time (5-7 days)). We reverse code items 4, 8, 12, 16 and aggregate those items, with the 20-item version ranging from 0 to 60 and the 8-item version from 0 to 24. Higher scores indicate more severe depression. The CES-D20 categorizes scores as follows: ≤ 16 indicates no to mild depression, 17-23 indicates moderate depression,

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and ≥ 24 indicates severe depression (Bi et al., 2023). In this paper, we use a CES-D20 cut-off of 16, corresponding to a CES-D8 cut-off score of 7, as these scores effectively identify individuals at risk of clinical depression in the Chinese context (Bi et al., 2023).

Both K6 and CES-D are frequently used to evaluate psychological distress and serious mental illness (Kessler et al., 2003; Kim et al., 2016; Weissman et al., 1977). Because these scales do not appear in all survey waves, we construct a consistent variable called “*distress*”. A child is coded as 1 (distressed) if their K6 score is ≥ 5 (Prochaska et al., 2012), CES-D8 score is ≥ 7 , or CES-D20 score is ≥ 16 (Bi et al., 2023).

Table B.1: Items of psychological scales

	2010	2012	2014	2016	2018	2020
<i>K6: Please select according to your statuses in the past month.</i>						
(1) Feel depressed and cannot cheer up.	X		X			
(2) Feel nervous.	X		X			
(3) Feel agitated or upset and cannot remain calm.	X		X			
(4) Feel hopeless about the future.	X		X			
(5) Feel that everything is difficult.	X		X			
(6) Think life is meaningless.	X		X			
<i>CES-D: Please select according to your statuses in the past week.</i>						
(1) I am worried about some trivial things.		X		X		
(2) I have a poor appetite and do not want to eat.		X		X		
(3) I feel depressed despite the help from relatives and friends.		X		X		
(4) I find myself not worse than others.		X		X		
(5) I cannot concentrate on things.		X		X		
(6) I am in a low spirit.		X		X	X	X
(7) I find it difficult to do anything.		X		X	X	X
(8) I find the future promising.		X		X		
(9) I feel that I have been a loser for a long time.		X		X		
(10) I feel scared.		X		X		
(11) I cannot sleep well.		X		X	X	X
(12) I feel happy.		X		X	X	X
(13) I talk less than usual.		X		X		
(14) I feel lonely.		X		X	X	X
(15) I find that people are not friendly to me.		X		X		
(16) I have a happy life.		X		X	X	X
(17) I cried or I want to cry.		X		X		
(18) I feel sad.		X		X	X	X
(19) I find that others do not like me.		X		X		
(20) I feel that I cannot continue with my life.		X		X	X	X
Number of items	6	20	6	20	8	8

Notes: This table presents detailed items of the K6 scale and the CES-D scale and in which wave they were elicited.

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In Section 3.5.2, we show that children born to mothers affected by the policy are less likely to experience mental distress. Since constructing a single mental health indicator (*distress*) may raise concerns about comparability across different scales, we now focus on examining comparable items from the K6, CESD-20, and CESD-8 scales. Table B.2 shows how we map K6 items with CSED items and construct corresponding variables. Table B.3 reports the results on our constructed variables. Across all specifications, we can see consistent positive impacts of the policy on mental health. Although the estimated effect on *Depressed* is positive, it is small in magnitude and statistically insignificant. For all other mental health measures, the estimates indicate consistent beneficial effects of the policy, as children are less likely to experience mental health issues.

Table B.2: Mapping of K6 and CES-D Items

Name of Variable	K6 Items	CES-D Items
Depressed	(1) Feel depressed and cannot cheer up.	(3) I feel depressed despite the help from relatives and friends; (6) I am in a low spirit;
Anxiety	(2) Feel nervous; (3) Feel agitated or upset and cannot remain calm.	(1) I am worried about some trivial things; (10) I feel scared.
Hopelessness	(4) Feel hopeless about the future.	(8) I find the future promising (reverse);
Difficulty	(5) Feel that everything is difficult.	(5) I cannot concentrate on things; (7) I find it difficult to do anything.
Worthlessness	(6) Think life is meaningless.	(20) I feel that I cannot continue with my life.

Notes: This table presents the corresponding items of psychological scales in K6 and CES-D. Item groupings are based on conceptual similarity rather than identical wording. CES-D items marked as reverse are positive statements, so their scores are reversed to match the rest of the scale.

Table B.3: Mental Distress: Similar items of K6 and CES-D

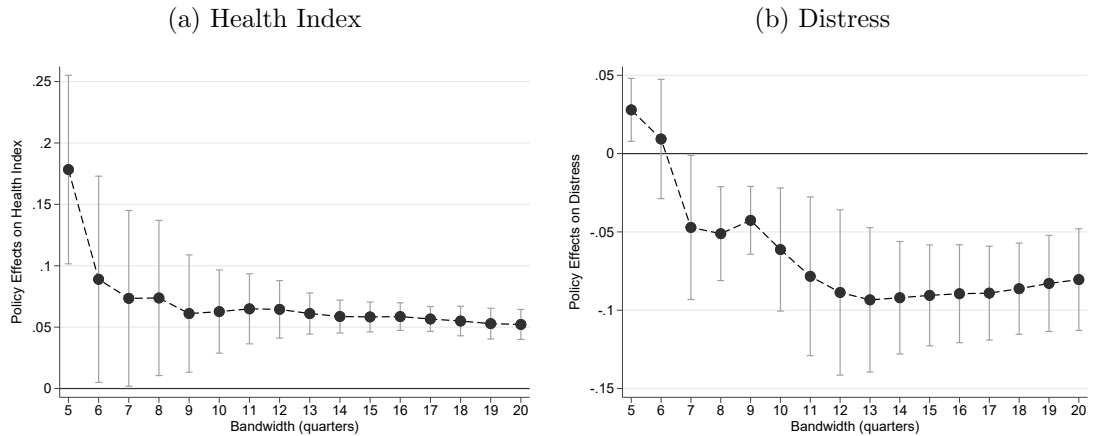
	(1) Depressed	(2) Anxiety	(3) Hopeless	(4) Difficulty	(5) Worthless
Policy	0.003 (0.017)	-0.098 (0.051)	-0.012 (0.049)	-0.045* (0.019)	-0.006 (0.005)
Mean	0.070	0.101	0.102	0.067	0.015
Observations	1,310	513	512	1,309	1,067
R^2	0.066	0.218	0.216	0.068	0.078

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. The policy cut-off is 1980Q1. All regressions include children of mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. All regressions include the interaction between treatment and running variables.

B.2 Robustness Checks

Bandwidth sensitivity. We show bandwidth sensitivity in Appendix Figure B.1. Each sub-graph reports coefficient estimates and 95% confidence intervals of our main results for bandwidths ranging from 5 to 20 quarters. Our baseline results show robustness to changes in bandwidth. For *Health Index*, as the bandwidth increases, the positive effects of the policy become increasingly statistically significant. Similarly, for children’s mental health *Distress*, larger bandwidths yield more significant policy effects, reflected in consistent reductions in distress, likely due to increased statistical power from a larger sample size.

Figure B.1: Sensitivity of results to bandwidth choices

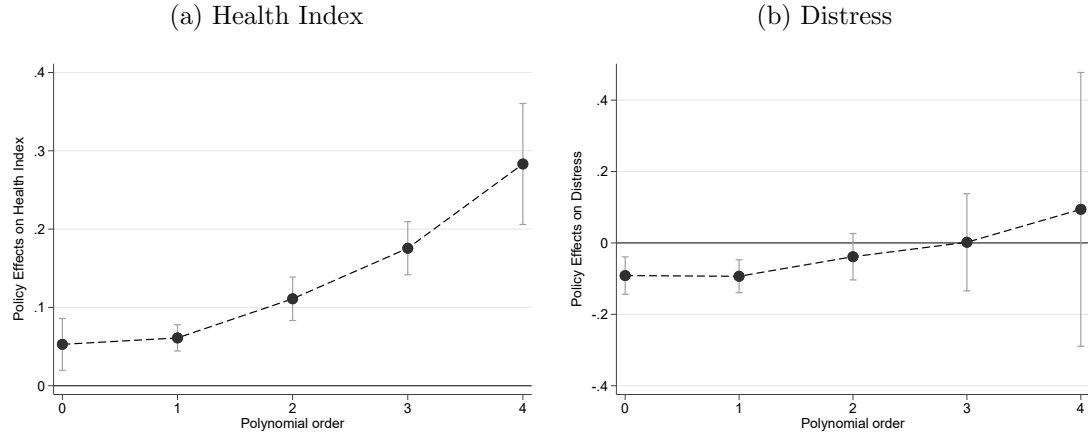


Notes: Each sub-graph reports coefficient estimates and confidence intervals for different bandwidths from 5 to 20 quarters. Each dot indicates the RD estimate using the specified bandwidth. Capped spikes represent 95% confidence intervals of the estimates.

Choice of polynomial orders. In our main analysis, we use a linear polynomial of our running variable – mothers’ birth quarters, which is the most common choice in RD designs. We show the sensitivity of our results to RD polynomials up to the fourth order in Appendix Figure B.2. The results suggest that our estimated effects on *Health Index* are not sensitive to the choice of polynomial order. However, higher-order polynomials produce implausibly large coefficients, suggesting potential overfitting. For *Distress*, policy effects disappear when we use third- or fourth-order polynomials, yielding null results. We do not view this as a concern, as high-order polynomials in regression

discontinuity designs are known to be unstable and may absorb local variation, reducing precision rather than reflecting genuine effects.

Figure B.2: Sensitivity of results to different orders of polynomial



Notes: Each dot represents the RD estimate using the specified order of RD polynomial. Capped spikes represent 95% confidence intervals of the estimates.

Different specifications. We test the robustness of our main results using various specifications, as shown in Appendix Table B.4. In column (1), we keep our main specification but use months rather than quarters as the running variable. Column (2) further applies a quadratic polynomial in birth months. In column (3), we remove the triangle kernel weights to see if our results are sensitive to the weighting schemes. Column (4) alters the clustering approach by clustering at the mother's birth year and province instead of birth year alone. In column (5), we conduct a donut exercise by removing all observations within 1 quarter of the policy cut-off and keeping the rest of the sample to fit our main specification.

Across all specifications, we can see consistent positive results of the policy on children's health: children born to mothers affected by the policy exhibit better physical health index, and they are less likely to experience mental distress. The estimates for both *Health Index* and *Distress* remain stable in magnitude and statistically significant. Especially, the donut exercise (column 5) yields similar and statistically significant coefficients, indicating that the estimated policy effects are not driven by observations near the cutoff.

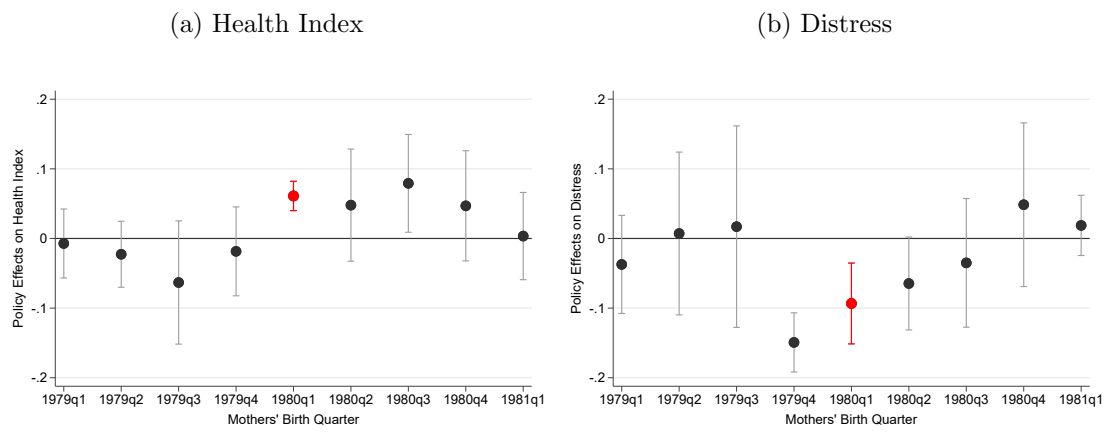
Table B.4: Robustness to different specifications

	Monthly Running (Linear) (1)	Monthly Running (Quadratic) (2)	No weights (3)	Double Clusters (4)	Donut 1Q (5)
<i>Panel A. Dependent variable is: Health Index</i>					
Policy	0.055*** (0.006)	0.089*** (0.017)	0.047** (0.016)	0.061** (0.026)	0.055*** (0.014)
Mean	0.007	0.007	0.007	0.007	0.007
Observations	3,947	3,947	3,857	3,546	3,426
R^2	0.060	0.061	0.053	0.062	0.070
<i>Panel B. Dependent variable is: Distress</i>					
Policy	-0.094*** (0.016)	-0.074* (0.035)	-0.089*** (0.015)	-0.093*** (0.031)	-0.090*** (0.019)
Mean	0.170	0.170	0.170	0.170	0.173
Observations	1,501	1,501	1,460	1,310	1,269
R^2	0.136	0.137	0.108	0.146	0.147

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. The policy cut-off is 1980Q1. All regressions include children of mothers born within 13 quarters around the policy cut-off. All regressions control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. All regressions include the interaction between treatment and running variables. Standard errors are in parentheses and clustered by mothers' birth years in (1) (2) (3) and (5). In column (4), standard errors are clustered by mothers' birth year and province.

Placebo tests. Our identification strategy assumes that there is a discontinuity in the treatment at the policy implementation date. One way to test against failures of this assumption is to use placebo tests with different policy cut-offs. In Appendix Figure B.3, we reproduce our main specification results with alternative policy cut-offs up to 4 quarters prior and post the policy cut-off employed – 1980 Q1. We find no significant effects from the policy at the placebo cut-offs for *Health Index*, indicating the validity of our identification strategy. For children's mental distress, we observe a significant effect at the 1979 Q4 placebo cut-off. While this effect is noteworthy, it does not persist across other outcomes, suggesting it may be due to random variation rather than a systematic issue with our identification strategy.

Figure B.3: Placebo 1980 Q1 cut-offs



Notes: This figure tests different policy cut-offs up to 4 quarters prior and post the policy cut-off employed in this paper. The policy cut-off we choose for this paper is 1980Q1, marked in red.

B.3 Heterogeneity on children's outcomes

In this section, we examine the heterogeneous effects of the policy by children's gender and age groups, as well as by mothers' birth order status (first-born versus later-born).

Table B.5 presents the effects of the policy passed onto boys and girls separately. We run our main specification on separate subsamples of boys and girls. Boys born to a mother born after the policy have a significantly higher health index with an increase of 0.072 index points or 18% of a standard deviation.¹ They are also less likely to experience distress, with a drop of 10.9 percentage points or about 57% in the likelihood of distress. For girls, the estimates indicate similar patterns, though the effects are not statistically significant. This difference may be due to adolescent girls' tendency to rate themselves more conservatively in self-assessments, whether in self-rated health or distress levels (Boerma et al., 2016; Van Droogenbroeck et al., 2018). Research has shown that girls often report lower self-rated health and higher levels of psychological distress than boys, which may explain the observed discrepancies between health outcomes (Breidablik et al., 2009; Jerdén et al., 2011).

Table B.5: Heterogeneous effects by gender

Gender groups	Health Index		Distress	
	Boy (1)	Girl (2)	Boy (3)	Girl (4)
Policy	0.072** (0.029)	0.056 (0.029)	-0.109** (0.037)	-0.052 (0.046)
Mean	0.04	0.04	0.19	0.17
Observations	1,816	1,730	656	654
R^2	0.074	0.113	0.175	0.228

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. The policy cut-off is 1980Q1. All regressions include children of mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. All regressions include the interaction between treatment and running variables.

Table B.6 presents the impact of the policy on mothers passed on to children across

¹This standard deviation is 0.39 in Table 3.1.

different age groups (0-12 and 13-15). We group children under 12 as those who are in primary school or less than primary school age, and those aged 13-15 as those in middle school, according to the Chinese education system. Overall, the results indicate a generally better health status for both age groups. The improvement in *health index* is larger in magnitude and more statistically significant for children aged 13-15, with an increase of 0.105 or 26.9% of a standard deviation compared to 0.048, about 12% of a standard deviation for the younger age group. However, the reduction in *distress* is larger among younger children, with a drop of 10.7 percentage points, equivalent to 70% of the mean, while the decline is only 6.9 percentage points (around 30%) for the older group.

Table B.6: Heterogeneous effects by age groups

Age groups	Health Index		Distress	
	≤ 12 (1)	> 12 (2)	≤ 12 (3)	> 12 (4)
Policy	0.048* (0.021)	0.105** (0.036)	-0.107** (0.034)	-0.069** (0.023)
Mean	0.03	0.07	0.15	0.21
Observations	2,932	614	727	583
R^2	0.080	0.140	0.174	0.235

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. The policy cut-off is 1980Q1. All regressions include children of mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. All regressions include the interaction between treatment and running variables.

Table B.7 demonstrates the impact of the policy with two different control groups: first-born mothers vs later-born mothers. First, we restrict the sample to mothers who are either the only child or the first-born (columns 1 and 3). This subsample analysis allows us to assess the policy's effects as driven specifically by the absence of maternal siblings, helping to isolate the sibling effect from other potential influences. We can see that the effects on physical and mental health are not statistically significant. Second, we restrict the control group to mothers who are not first-borns (columns 2 and 4). For

this subgroup, we see that children born to policy-affected mothers have a significantly higher physical health index. They are also less likely to be distressed. Collectively, the findings suggest that these positive health effects of the policy are not driven solely by the absence of a sibling but also by other factors the policy might affect, such as sibling composition.

Table B.7: Heterogeneous effects by birth order

Birth order	Health Index		Distress	
	First (1)	Later (2)	First (3)	Later (4)
Policy	-0.013 (0.015)	0.117*** (0.019)	-0.030 (0.032)	-0.137*** (0.027)
Mean	0.009	0.001	0.157	0.167
Observations	2,592	2,793	869	959
R^2	0.052	0.069	0.179	0.145

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. The policy cut-off is 1980Q1. All regressions include children of mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. All regressions include the interaction between treatment and running variables. Columns (1) (3) restrict the control group to first-born mothers only. Columns (2) (4) restrict the control group to mothers who are not first borns.

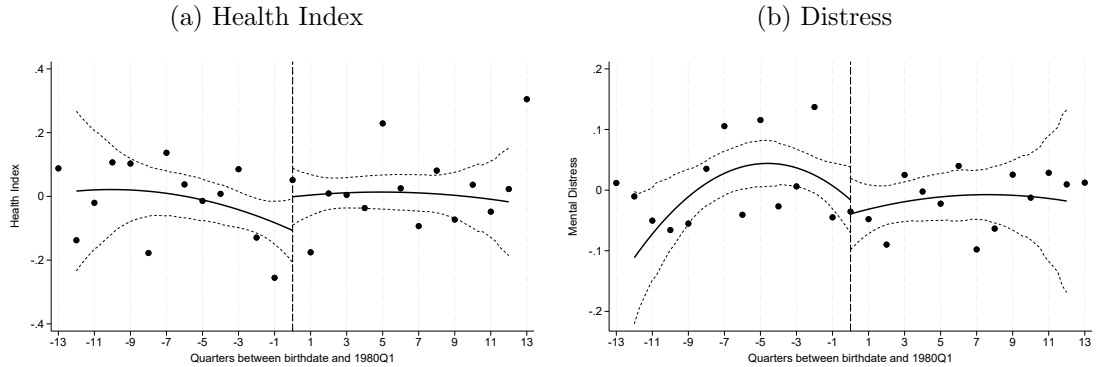
B.4 Additional Tables and Figures

Table B.8: Mothers' health: component outcomes of Health Index

	(1) No Discomfort	(2) No Chronic Disease	(3) No Hospital	(4) Self-rated Health	(5) Observed Health
Policy	0.058** (0.019)	0.061*** (0.007)	0.081*** (0.017)	-0.103** (0.028)	-0.001 (0.003)
Mean	0.752	0.925	0.922	0.216	0.980
Observations	3,473	3,473	2,107	3,550	2,983
R^2	0.042	0.041	0.071	0.218	0.037

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. All regressions include mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, mothers' age, and mothers' province-birthyear fixed effects. All regressions include the interaction between treatment and running variables. *Health Index* is the standardized weighted summary index variable created from multiple health outcomes in columns 2 to 6. *No Discomfort* takes 1 if a mother reported no physical discomfort in the last two weeks. *No Chronic Disease* is a dummy variable indicating whether a mother was not diagnosed with a chronic disease in the past six months. *No Hospital* is also a dummy variable which is equal to 1 if a mother was not hospitalized last year due to illness or injury. *Self-rated Health* takes 1 if they rated themselves as healthy. *Interviewer-observed Health* is a binary variable, with 1 indicating good health.

Figure B.4: Quadratic polynomial: RD plots for children's health outcomes



Notes: The points depict binned residuals from a main regression of the outcome variable on a quadratic polynomial in birth quarter, along with other control variables. Solid lines display quadratic polynomial regressions, separately estimated on each side of the cut-off, with dashed lines indicating 95% confidence intervals.

Table B.9: Children's health: component outcomes of Health Index (using mothers' birth information)

	(1)	(2)	(3)	(4)
	No Sick	No Hospital	Self-rated Health	Observed Health
Policy	0.035** (0.010)	0.027** (0.010)	0.090*** (0.023)	0.013** (0.005)
Mean	0.732	0.794	0.389	0.978
Observations	3,463	3,461	1,312	1,759
R^2	0.098	0.101	0.126	0.062

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The policy cut-off is 1980Q1. Standard errors are in parentheses and are clustered by mothers' birth years. All regressions include children of mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. All regressions include the interaction between treatment and running variables. *Health Index* is the standardized weighted summary index variable created from multiple health outcomes in this table. Only those aged 10 and over responded to the self-rated health question, and *self-rated health* is equal to 1 if children rate themselves as healthy. The scale for interviewer-observed child health ranges from 1 (worst) to 7 (best). The *observed health* variable equals 1 if the rating is greater than or equal to 4. The interviewers only assessed the health of those children who were present at the interview.

Table B.10: Children's cognitive outcomes

	(1)	(2)	(3)
	Key Class	Above Average Chinese	Above Average Math
Policy	0.046* (0.020)	-0.014 (0.036)	-0.066 (0.035)
Mean	0.093	0.665	0.683
Observations	2,051	2,094	2,096
R^2	0.078	0.139	0.144

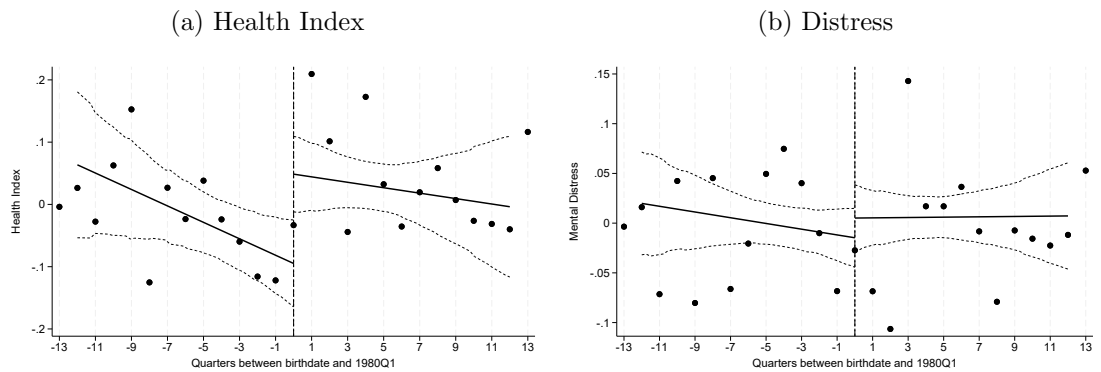
Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered at the mothers' birth years. The policy cut-off is 1980Q1. All regressions include children of mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's age and gender. The interaction between treatment and running variables is included in all regressions. These outcomes are for children of school age, i.e., 6 to 15 years old in the dataset.

Table B.11: Effects from mothers' side: interaction between parents and children, restricted to children above 9 years old

	Interviewers' observations		Children's Responses		
	(1) Active Communication	(2) Care about Education	(3) Quarrel	(4) Heart-to-heart Talk	(5) Parents Quarrel
Policy	0.053*** (0.011)	0.041 (0.022)	0.501** (0.148)	0.441 (0.284)	0.007 (0.186)
Mean	0.820	0.820	1.273	2.383	0.844
Observations	1,216	1,246	1,226	1,139	1,199
R^2	0.590	0.413	0.078	0.108	0.057

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by mothers' birth years. The policy cut-off is 1980Q1. All regressions include children of mothers born within 13 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. All regressions include the interaction between treatment and running variables. The first two variables are dummy variables, showing interviewers' observations on whether parents communicate with their child actively and on whether home environment indicates parents care about their child's education. The next three variables are only reported by children aged 9-15. Quarrel refers to the number of times children quarrelled with their parents last month (column 3). Heart-to-heart talk refers to the number of times children had a heart-to-heart talk with parents last month (column 4). The last variable (column 5) refers to the number of times the parents quarrelled with each other in the last month.

Figure B.5: RD plots for mothers' health outcomes



Notes: The points depict binned residuals from a main regression of the outcome variable on a linear polynomial in birth quarter, along with other control variables. Solid lines display local linear polynomial regressions, separately estimated on each side of the cut-off, with dashed lines indicating 95% confidence intervals.

B.5 The policy's effects on fathers and children born to these fathers

Summary Statistics. Table B.13 reports descriptive statistics for the fathers sample, including characteristics of the fathers and their children. Most statistically significant differences between the control and treatment groups are concentrated in age-related variables. This pattern is expected given the policy design, as individuals born later (and therefore younger at the time of the survey) are more likely to be exposed to the treatment.

Balance check. A balance check for the fathers' dataset supports the continuity assumption of the design. Across specifications, we find no discontinuities in grandparents' predetermined characteristics at the cutoff, with the exception of *Communist* which is significant only at the 10% level.

Table B.12: Pre-determined characteristics of fathers' parents

	Dependent variable is:						
	Paternal grandfather			Paternal grandmother			Either
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Age	Literacy	No Work	Age	Literacy	No Work	Communist
Policy	0.000 (0.000)	0.007 (0.053)	0.027 (0.023)	0.000 (0.000)	-0.016 (0.083)	-0.015 (0.018)	-0.111* (0.049)
Mean	58.620	0.769	0.078	61.055	0.494	0.173	0.267
Observations	2,357	2,357	2,357	2,357	2,357	2,357	2,357
R^2	1.000	0.348	0.141	1.000	0.311	0.146	0.259

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered by fathers' birth years. The policy cut-off is 1980Q1. Regressions include fathers born within 10 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' and fathers' province-birthyear fixed effects. All regressions include the interaction between treatment and running variables. Columns (1) (2) (3) show the characteristics of fathers' fathers, and columns (4) (5) (6) show the characteristics of fathers' mothers.

Appendix B. Chapter 3 Appendix

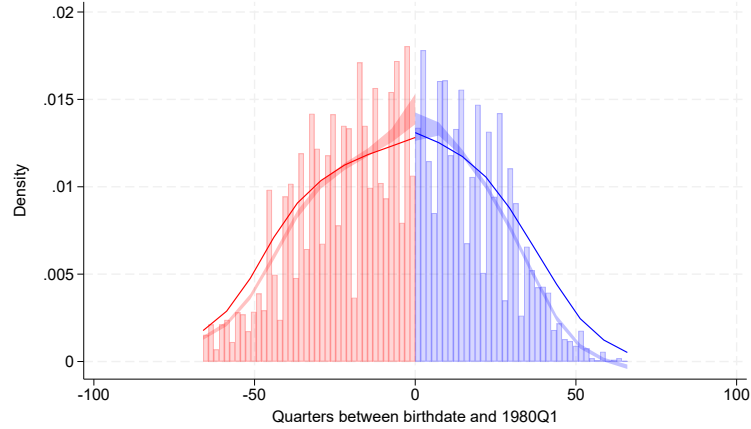
Table B.13: Descriptive statistics

	All observations				Within 10-quarter bandwidth			
	All	Control	Treated	Diff	All	Control	Treated	Diff
A. Health Outcomes								
Health Index	0.01 (0.39)	0.04 (0.45)	-0.01 (0.31)	0.05*** (8.74)	0.00 (0.37)	0.01 (0.38)	-0.00 (0.36)	0.01 (0.93)
No sickness last month	0.72 (0.45)	0.76 (0.43)	0.67 (0.47)	0.09*** (12.48)	0.71 (0.45)	0.72 (0.45)	0.70 (0.46)	0.02 (1.50)
Not hospitalized last month	0.78 (0.42)	0.82 (0.39)	0.73 (0.44)	0.09*** (12.87)	0.78 (0.42)	0.78 (0.41)	0.77 (0.42)	0.01 (1.17)
Self-rated health (healthy = 1)	0.43 (0.49)	0.43 (0.50)	0.42 (0.49)	0.02 (0.93)	0.40 (0.49)	0.39 (0.49)	0.41 (0.49)	-0.02 (-0.75)
Interviewer-observed health (≥ 4 on 1-7 scale)	0.98 (0.15)	0.98 (0.14)	0.97 (0.16)	0.01* (2.03)	0.97 (0.16)	0.98 (0.14)	0.97 (0.18)	0.01 (1.70)
Distress indicator based on K6 and CESD	0.18 (0.38)	0.18 (0.39)	0.17 (0.37)	0.01 (1.15)	0.17 (0.37)	0.16 (0.37)	0.18 (0.38)	-0.02 (-0.77)
B. Demographics								
Child's age	7.92 (4.49)	9.74 (4.20)	5.66 (3.77)	4.08*** (64.55)	7.60 (4.12)	8.11 (4.23)	7.10 (3.94)	1.02*** (8.22)
Child's gender (female = 1)	0.46 (0.50)	0.45 (0.50)	0.46 (0.50)	-0.01 (-0.94)	0.48 (0.50)	0.47 (0.50)	0.48 (0.50)	-0.01 (-0.66)
Child's birthyear	2007 (5.23)	2004 (4.56)	2010 (3.94)	-5.99*** (-88.80)	2007 (3.74)	2007 (3.95)	2008 (3.41)	-1.30*** (-11.64)
Mother's age	34.85 (6.22)	38.28 (5.26)	30.54 (4.38)	7.74*** (97.60)	33.49 (4.23)	34.61 (4.11)	32.38 (4.07)	2.23*** (17.37)
Mother's birthyear	1980 (6.65)	1976 (5.01)	1985 (4.07)	-9.64*** (-131.08)	1981 (3.13)	1980 (2.95)	1983 (2.81)	-2.47*** (-27.71)
Father's age	36.79 (6.39)	40.60 (5.10)	31.86 (4.09)	8.75*** (117.27)	35.30 (3.51)	36.47 (3.31)	34.16 (3.32)	2.31*** (22.55)
Father's birthyear	1978 (6.79)	1973 (4.66)	1984 (3.59)	-10.71*** (-163.58)	1980 (1.53)	1978 (0.73)	1981 (0.81)	-2.63*** (-113.26)
Family size	5.08 (1.89)	4.72 (1.64)	5.52 (2.07)	-0.80*** (-26.34)	5.30 (1.95)	5.06 (1.75)	5.53 (2.11)	-0.46*** (-7.95)
C. Father's characteristics								
Health Index	0.02 (0.42)	0.02 (0.45)	0.01 (0.37)	0.01* (2.15)	0.02 (0.40)	0.02 (0.40)	0.02 (0.40)	0.00 (0.15)
Distress indicator based on K6 and CESD	0.18 (0.38)	0.18 (0.39)	0.17 (0.37)	0.01 (1.15)	0.17 (0.37)	0.16 (0.37)	0.18 (0.38)	-0.02 (-0.77)
No siblings	0.07 (0.26)	0.06 (0.25)	0.09 (0.28)	-0.02*** (-4.18)	0.10 (0.30)	0.09 (0.29)	0.11 (0.31)	-0.02 (-1.36)
Number of children	1.76 (0.78)	1.78 (0.82)	1.74 (0.74)	0.04*** (3.37)	1.77 (0.82)	1.72 (0.80)	1.81 (0.83)	-0.09*** (-3.77)
College+	0.17 (0.37)	0.14 (0.35)	0.19 (0.40)	-0.05*** (-8.17)	0.20 (0.40)	0.21 (0.41)	0.19 (0.39)	0.03* (2.11)
Ln(Yearly Income)	5.40 (4.89)	5.17 (4.83)	5.73 (4.95)	-0.56*** (-6.25)	5.58 (4.94)	5.67 (4.92)	5.50 (4.97)	0.17 (1.00)
Social Position (Leading Cadres)	0.01 (0.08)	0.01 (0.10)	0.00 (0.06)	0.01*** (4.30)	0.01 (0.09)	0.01 (0.09)	0.01 (0.09)	0.00 (0.04)
D. Grandparents' characteristics								
Grandfather's age	61.26 (9.39)	64.58 (8.50)	53.86 (6.67)	10.71*** (66.41)	57.24 (6.26)	58.92 (6.06)	55.36 (5.94)	3.56*** (15.67)
Grandmother's age	62.74 (8.74)	65.55 (8.15)	56.44 (6.44)	9.11*** (58.57)	59.49 (6.57)	61.15 (6.58)	57.65 (6.05)	3.50*** (14.58)
Literacy (grandfather)	0.74 (0.44)	0.70 (0.46)	0.82 (0.38)	-0.12*** (-13.02)	0.78 (0.41)	0.77 (0.42)	0.80 (0.40)	-0.03 (-1.64)
Literacy (grandmother)	0.54 (0.50)	0.50 (0.50)	0.62 (0.49)	-0.12*** (-10.97)	0.60 (0.49)	0.60 (0.49)	0.61 (0.49)	-0.01 (-0.52)
Unemployment (grandfather)	0.13 (0.34)	0.14 (0.35)	0.11 (0.32)	0.03*** (4.06)	0.11 (0.31)	0.11 (0.31)	0.10 (0.31)	0.01 (0.47)
Unemployment (grandmother)	0.23 (0.42)	0.24 (0.43)	0.20 (0.40)	0.04*** (4.40)	0.20 (0.40)	0.19 (0.40)	0.20 (0.40)	-0.01 (-0.49)
Either of grandparents is communist	0.10 (0.30)	0.14 (0.35)	0.05 (0.22)	0.09*** (19.75)	0.11 (0.32)	0.15 (0.35)	0.08 (0.27)	0.07*** (6.87)
Observations	15922	8813	7109	15922	4367	2151	2216	4367

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses. "All" means the whole sample, "Treated" means fathers have no siblings, and "Control" means mothers have siblings. "Diff" shows the mean difference between treated and control groups. The scale for interviewer-observed child health ranges from 1 (worst) to 7 (best).

Manipulation test. The manipulation test (Figure B.6) using the fathers' dataset shows no evidence of a density discontinuity at the cutoff, indicating no manipulation of the running variable.

Figure B.6: Manipulation tests: fathers' birth quarters



Notes: The figures depict manipulation tests based on density discontinuity using Stata command `rddensity` (Cattaneo et al., 2018). Solid lines display point estimates, separately estimated on each side of the cut-off, with shaded areas indicating 95% confidence intervals. Confidence intervals are not centered at the density point estimates because they have been bias-corrected (Cattaneo et al., 2022).

The policy's effects on fathers. Table B.14 presents the policy effects on fathers' demographic characteristics. In contrast to the results for mothers, we observe no significant change in fathers' sibling size. This finding aligns with our hypothesis that the policy primarily constrained families whose first child was female, as those families faced the strongest pressure to have a second child (typically to have a son) before the policy's strict enforcement.

In terms of health (Table B.15), for fathers exposed to the policy, we observe a significant improvement in physical health, with their health index increasing by 0.134 or 33.5% of a standard deviation.² As shown in Table B.16, these improvements are consistent across various individual health components. In contrast, the policy has a significant adverse effect on mental health. Policy-affected fathers are approximately 17 percentage points more likely to report distress, corresponding to about 67% of the

²This standard deviation is 0.40 in Table B.13.

Table B.14: Policy effects on fathers' sibling composition

	(1) No siblings	(2) Older Brother	(3) Younger Brother	(4) Older Sister	(5) Younger Sister
Policy	-0.023 (0.049)	-0.027 (0.043)	-0.046 (0.027)	-0.108* (0.045)	0.305*** (0.035)
Mean	0.218	0.220	0.117	0.334	0.206
Observations	2,346	2,346	2,346	2,346	2,346
R^2	0.357	0.323	0.187	0.361	0.221

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are reported in parentheses and are clustered by fathers' birth years. The policy cut-off is 1980Q1. Regressions include fathers born within 10 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, fathers' age, and fathers' province-birthyear fixed effects. All regressions include the interaction between treatment and running variables.

mean.

Table B.15: Policy effects on fathers' health status

	Health Index		Mental Distress	
	(1)	(2)	(3)	(4)
Policy	0.134*** (0.029)	0.128* (0.050)	0.172*** (0.009)	0.167*** (0.018)
Policy $\times$ Quarter	Yes	No	Yes	No
Mean	0.013	0.013	0.257	0.257
Observations	2,354	2,354	2,253	2,253
R^2	0.092	0.089	0.126	0.124

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by fathers' birth years. The policy cut-off is 1980Q1. Regressions include fathers born within 10 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, fathers' age, and fathers' province-birthyear fixed effects. *Health Index* is the standardized weighted summary index variable created from multiple health outcomes in Table B.16. *Distress* is a binary variable where 1 indicates psychological distress.

Table B.17 reports the effects on fathers' socioeconomic outcomes. In contrast to the effects observed for mothers, fathers exposed to the policy tend to have more children. We also find no significant change in college attendance rates, suggesting no improvement in their educational attainment. Moreover, the policy does not increase fathers' income or enhance their social status.

Table B.16: Fathers' health status: component outcomes of Health Index

	(1) No Discomfort	(2) No Chronic Disease	(3) No Hospital	(4) Self-rated Health	(5) Observed Health
Policy	0.096** (0.026)	0.068* (0.027)	0.038** (0.013)	0.108*** (0.023)	-0.002 (0.009)
Mean	0.807	0.911	0.963	0.225	0.981
Observations	2,253	2,250	1,315	2,354	1,898
R^2	0.061	0.092	0.075	0.227	0.046

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by fathers' birth years. The policy cut-off is 1980Q1. Regressions include fathers born within 10 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, fathers' age, and fathers' province-birthyear fixed effects. All regressions include the interaction between treatment and running variables. *Health Index* is the standardized weighted summary index variable created from multiple health outcomes in this table. *No Discomfort* takes 1 if a father reported no physical discomfort in the last two weeks. *No Chronic Disease* is a dummy variable indicating whether a father was not diagnosed with a chronic disease in the past six months. *No Hospital* is also a dummy variable which is equal to 1 if a father was not hospitalized last year due to illness or injury. *Self-rated Health* takes 1 if they rated themselves as healthy. *Interviewer-observed Health* is a binary variable, with 1 indicating good health.

Table B.17: Policy effects on fathers' demographic and social characteristics

	(1) Number of children	(2) College+	(3) Ln(Yearly Income)	(4) Social Position
Policy	0.120** (0.045)	0.002 (0.046)	-0.502* (0.215)	-0.029*** (0.006)
Mean	1.765	0.209	7.430	0.023
Observations	2,357	2,357	2,165	2,357
R^2	0.325	0.262	0.358	0.074

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered at the fathers' birth years. The policy cut-off is 1980Q1. Regressions include fathers born within 10 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, fathers' age, and fathers' province-birthyear fixed effects. All regressions include the interaction between treatment and running variables. *College+* takes 1 if a father has a college degree or higher.

The intergenerational health effects on children born to policy-affected fathers. Table B.18 presents the intergenerational health effects of the policy on the next generation – children. Children of fathers exposed to the policy exhibit better physical health, with their health index improving by about 0.05 or 13.5% of a stan-

dard deviation.³ However, we find no statistically significant effects on mental health, although the estimates are negative, suggesting lower levels of distress.

Table B.18: Children's results using fathers' birth information

	Health Index		Mental Distress	
	(1)	(2)	(3)	(4)
Policy	0.050*** (0.005)	0.051*** (0.008)	-0.042 (0.055)	-0.042 (0.060)
Policy $\times$ Quarter	Yes	No	Yes	No
Mean	0.000	0.000	0.168	0.168
Observations	2,346	2,346	735	735
R^2	0.060	0.060	0.160	0.160

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by fathers' birth years. The policy cut-off is 1980Q1. All regressions include children of fathers born within 10 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. *Health Index* is the standardized weighted summary index variable created from multiple health outcomes in Table B.19. *Distress* in column (4) equals 1 if Kessler Psychological Distress Scale (K6) is larger than or equal to 5, or CES-D8 is larger than or equal to 7, or CES-D20 is larger than or equal to 16.

³This standard deviation is 0.37 in Table B.13.

Table B.19: Children's Health: component outcomes (using fathers' birth information)

	(1)	(2)	(3)	(4)
	No Sick	No Hospital	Self-rated Health	Observed Health
Policy	0.081*** (0.016)	0.039* (0.016)	-0.062 (0.042)	0.050*** (0.010)
Mean	0.709	0.775	0.400	0.975
Observations	2,298	2,295	736	1,072
R^2	0.088	0.101	0.123	0.094

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by fathers' birth years. The policy cut-off is 1980Q1. All regressions include children of fathers born within 10 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. All regressions include the interaction between treatment and running variables. *Health Index* is the standardized weighted summary index variable created from multiple health outcomes in this table. Only those aged 10 and over responded to the self-rated health question, and *self-rated health* is equal to 1 if children rate themselves as healthy. The scale for interviewer-observed child health ranges from 1 (worst) to 7 (best). The *observed health* variable equals 1 if the rating is greater than or equal to 4. The interviewers only assessed the health of those children who were present at the interview.

Table B.20: Effects from fathers' side: family income and expenditure

	(1)	(2)	(3)	(4)	(5)
	Total Income	Total Exp.	Public Ins.	Commercial Ins.	Commercial Ins. Spending
Policy	-0.215** (0.079)	0.055 (0.081)	-0.013 (0.022)	-0.111** (0.029)	-0.753** (0.197)
Mean	10.755	10.916	0.717	0.183	1.165
Observations	2,293	2,241	2,292	2,286	2,285
R^2	0.321	0.340	0.169	0.098	0.107

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered by fathers' birth years. The policy cut-off is 1980Q1. All regressions include children of fathers born within 10 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. All regressions include the interaction between treatment and running variables. The first two are taken from family level expenditure and the rest are directly on children. Total family income comprises five components: wage income, total/net business income, property income, transfer income, and other income. We take natural logs of total income and expenditure (columns 1 and 2), medical expenditure (column 3), and commercial insurance spending (column 6). Public and commercial insurance in columns (4) and (5) are binary variables.

Table B.21: Effects from fathers' side: interaction between parents and children

	Interviewers' observations		Children's Responses		
	(1) Active Communication	(2) Care about Education	(3) Quarrel	(4) Heart-to-heart Talk	(5) Parents Quarrel
Policy	-0.001 (0.013)	0.008 (0.009)	-0.159 (0.194)	-0.372 (0.452)	0.112 (0.247)
Mean	0.844	0.849	1.093	2.480	0.681
Observations	1,911	1,965	693	669	679
R^2	0.541	0.390	0.133	0.128	0.098

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by fathers' birth years. The policy cut-off is 1980Q1. All regressions include children of fathers born within 10 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. All regressions include the interaction between treatment and running variables. The first two variables are dummy variables, showing interviewers' observations on whether parents communicate with their child actively and on whether home environment indicates parents care about their child's education. The next three variables are only reported by children aged 9-15. Quarrel refers to the number of times children quarrelled with their parents last month (column 3). Heart-to-heart talk refers to the number of times children had a heart-to-heart talk with parents last month (column 4). The last variable (column 5) refers to the number of times the parents quarrelled with each other in the last month.

Table B.22: Effects from fathers' side: parental care

	(1) Give up watching TV	(2) Discuss School	(3) Homework Check	(4) TV Restriction	(5) Save for Education
Policy	-0.115** (0.032)	-0.032* (0.015)	-0.024 (0.031)	0.045 (0.037)	-0.130*** (0.030)
Mean	0.240	0.107	0.308	0.199	0.231
Observations	1,361	1,407	1,373	1,398	2,290
R^2	0.133	0.103	0.167	0.074	0.114

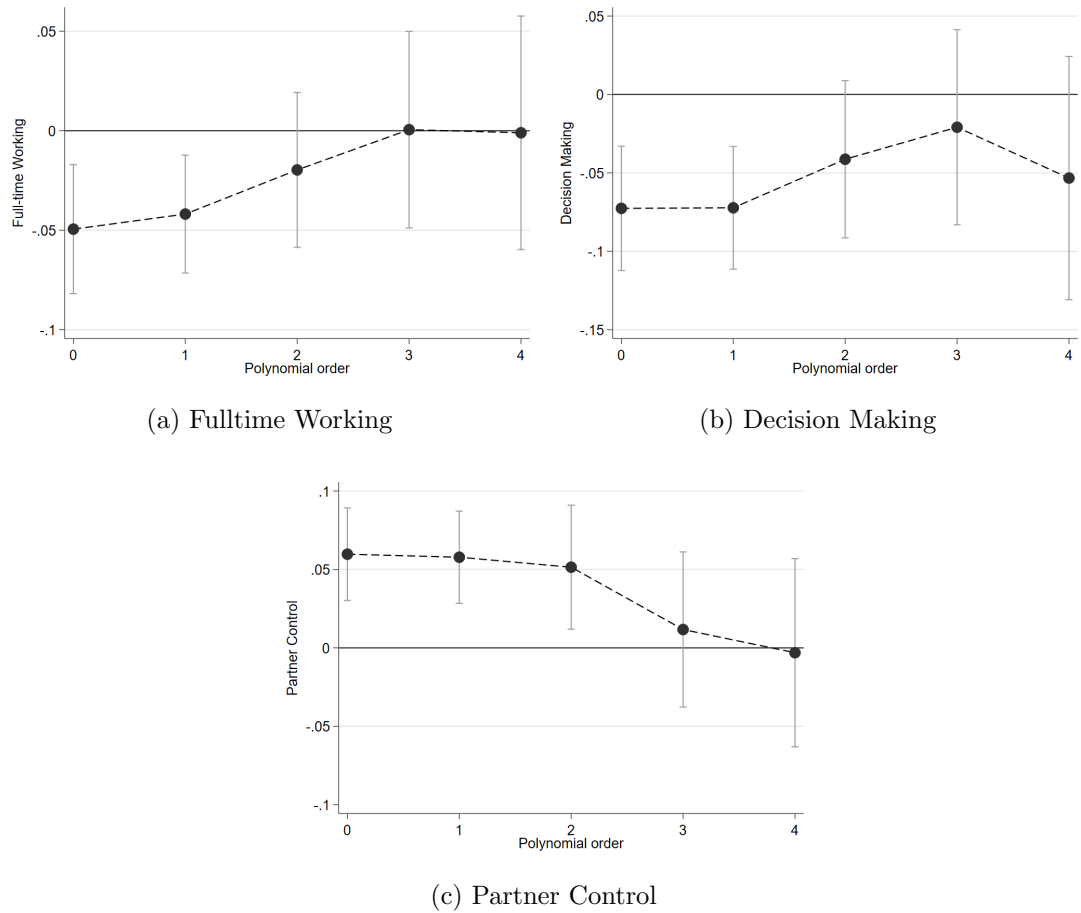
Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and are clustered by fathers' birth years. The policy cut-off is 1980Q1. All regressions include children of fathers born within 10 quarters around the policy cut-off. All regressions use a local linear polynomial of birth quarters with a triangular kernel weight and control for grandparents' characteristics, parents' age, parents' province-birthyear fixed effects, children's province-birthyear fixed effects, children's age and gender. All regressions include the interaction between treatment and running variables. Outcomes in columns (1) to (4) are dummy variables constructed based on parents' responses: whether parents very often (6-7 times a week) give up watching TV to avoid disturbing their child (column 1), whether parents very often discuss happenings at school with their child this semester (column 2), whether parents very often ask their child to finish homework or check their child's homework (column 3), and whether parents restrict their child from watching TV or restrict the type of TV programs their child could watch (column 4). Column (5) is a dummy variable equal to 1 if the parent answered "yes" to the question of having started saving money for the child's education.

Appendix C

Chapter 4 Appendix

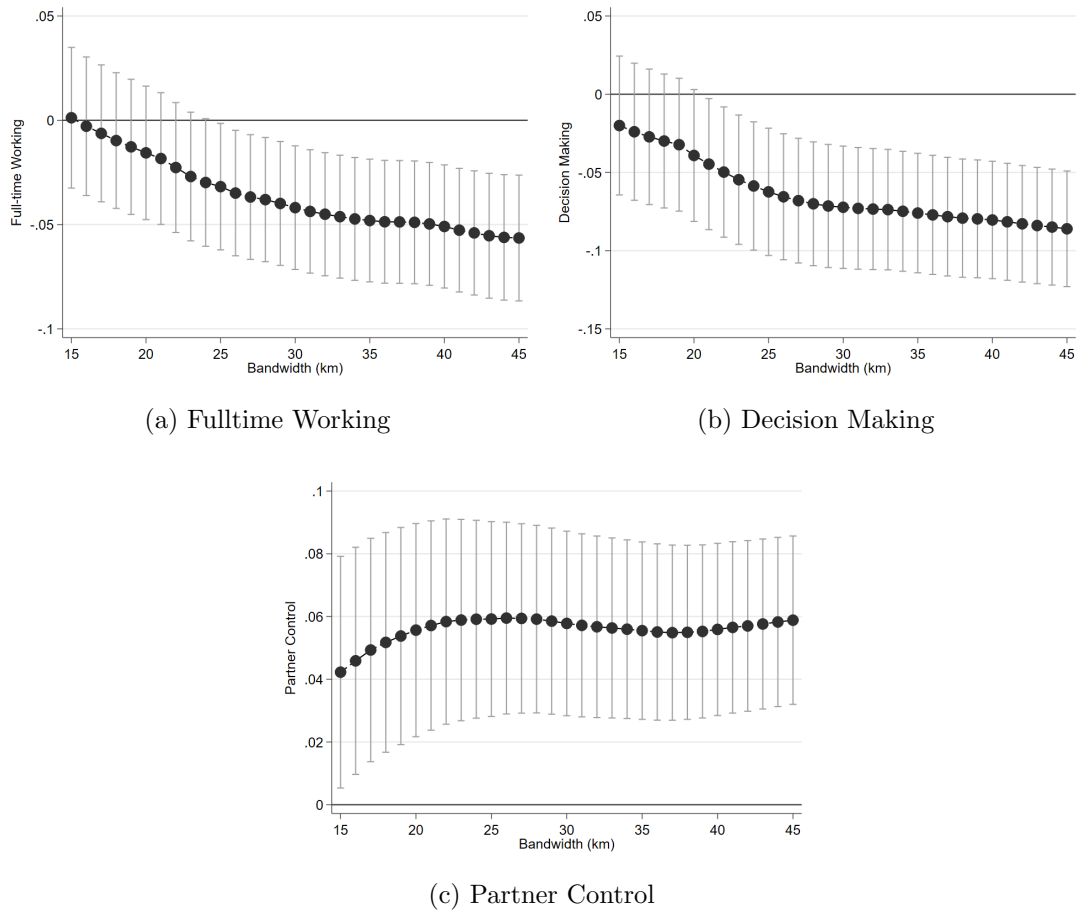
C.1 Robustness

Figure C.1: Female Empowerment: Sensitivity of results to different orders of polynomial



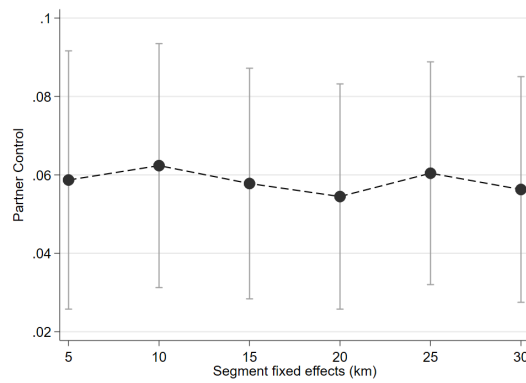
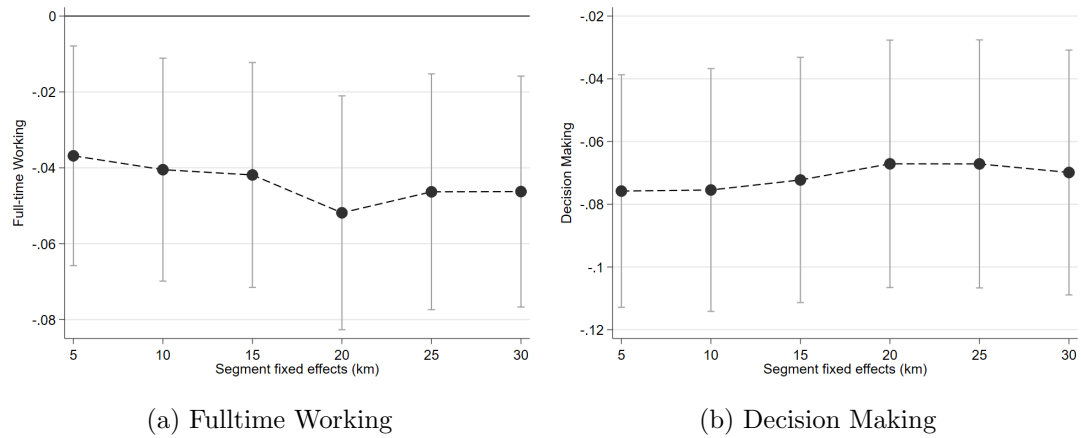
Note: Each dot represents the RD estimate using the specified order of RD polynomial in distance to the border. Capped spikes represent 90% confidence intervals of the estimates.

Figure C.2: Female Empowerment: Sensitivity of results to bandwidth choices



Note: Each sub-graph reports coefficient estimates and confidence intervals for different bandwidth levels ranging from 15 to 45 kilometers (horizontal axis) with 1km intervals. Each dot indicates the RD estimate using the specified bandwidth. Capped spikes represent 90% confidence intervals of the estimates.

Figure C.3: Female Empowerment: Sensitivity of results to different segment fixed effects



(c) Partner Control

Note: Each dot represents the RD estimate using the specified border segment fixed effects. Capped spikes represent 90% confidence intervals of the estimates.

Table C.1: Female Empowerment: Robustness to different specifications

	No Interaction (1)	No Dist. Capital (2)	No weights (3)	Province FE (4)	1km Donut (5)	3km Donut (6)
<i>Panel A. Dependent variable is: Fulltime Working</i>						
Genocide	-0.050** (0.025)	-0.048** (0.024)	-0.054** (0.025)	-0.051* (0.026)	-0.072*** (0.027)	-0.105** (0.046)
Mean	0.709	0.709	0.709	0.709	0.689	0.600
Observations	11723	11723	11723	11710	10301	7694
Clusters	459	459	459	458	401	297
<i>Panel B. Dependent variable is: Decision Making</i>						
Genocide	-0.075** (0.031)	-0.069** (0.031)	-0.081*** (0.031)	-0.059* (0.030)	-0.056 (0.037)	-0.118** (0.053)
Mean	0.563	0.563	0.563	0.563	0.559	0.535
Observations	7023	7023	7023	7009	6176	4585
Clusters	459	459	459	458	401	297
<i>Panel C. Dependent variable is: Partner Control</i>						
Genocide	0.058** (0.023)	0.060*** (0.023)	0.051** (0.022)	0.055** (0.022)	0.067** (0.026)	0.083** (0.036)
Mean	0.126	0.126	0.126	0.126	0.124	0.119
Observations	2173	2173	2173	2173	1955	1518
Clusters	366	366	366	366	322	243

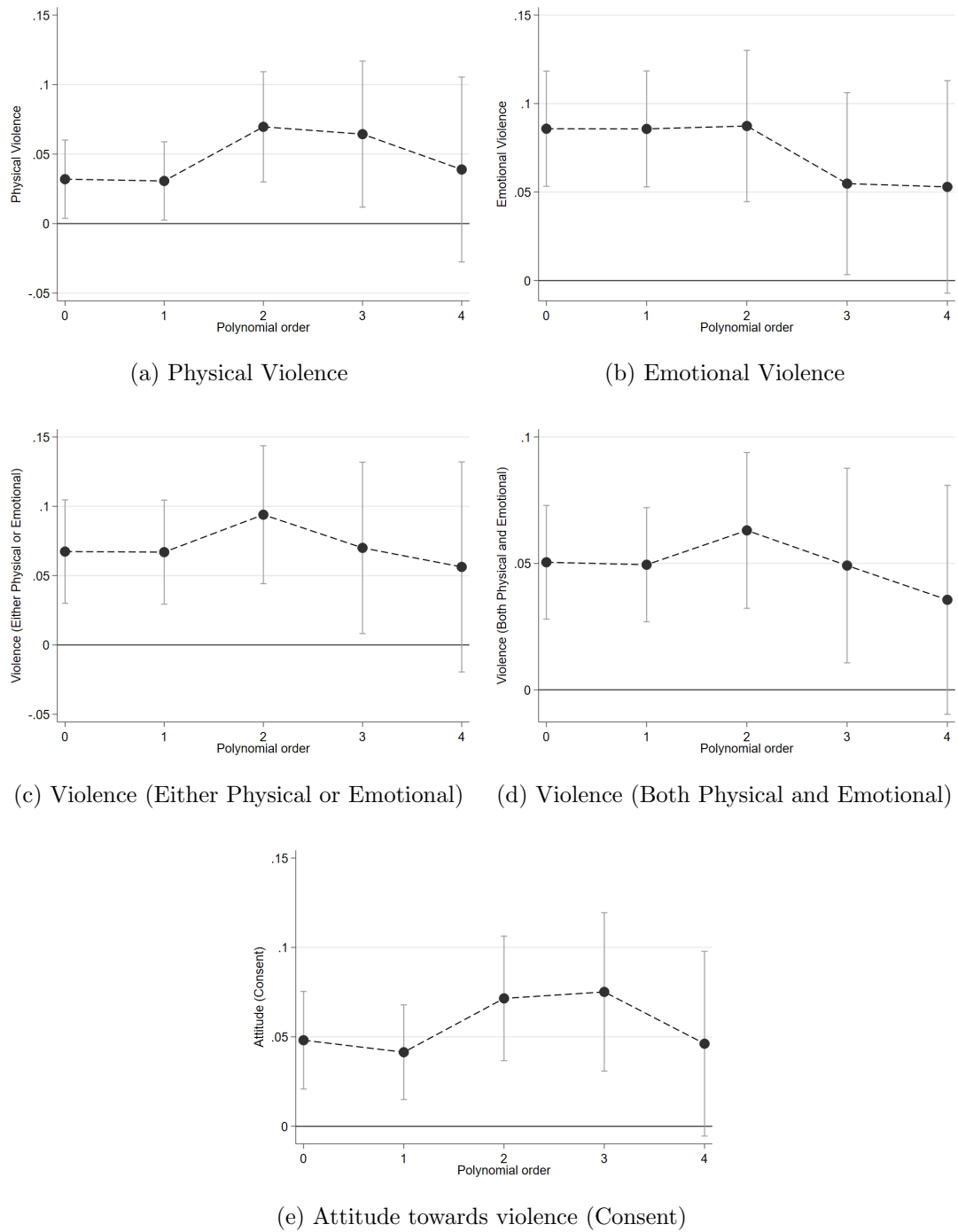
Note: The unit of analysis is DHS survey respondents. The sample is restricted to those within a 30km bandwidth from the Southwest-West boundary. Column (1) excludes the interaction between the treatment variable and the running variable – distance to the border. Column (2) excludes the distance to the capital in the main regression. Column (3) presents estimates without a triangular kernel weight. Column (4) replaces segment fixed effects with province fixed effects. The last two columns conduct a *donut* exercise by excluding all observations within 1 km and 3 km of the border, respectively, and estimate the model using the remaining observations. Standard errors reported in parentheses are clustered at the DHS village level. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

Table C.2: Female Empowerment: Shifting border

	Shift Northwest		Shift Southeast	
	10km (1)	30km (2)	10km (3)	30km (4)
<i>Panel A. Dependent variable is: Fulltime Working</i>				
Genocide	0.125** (0.058)	-0.199*** (0.075)	-0.130*** (0.042)	0.090* (0.047)
Mean	0.727	0.707	0.706	0.648
Observations	10284	7459	12186	12442
Clusters	400	288	478	502
<i>Panel B. Dependent variable is: Decision Making</i>				
Genocide	-0.007 (0.064)	-0.074 (0.087)	-0.064 (0.046)	0.080 (0.053)
Mean	0.547	0.522	0.564	0.597
Observations	6080	4458	7347	7644
Clusters	400	288	478	502
<i>Panel C. Dependent variable is: Partner Control</i>				
Genocide	-0.060* (0.034)	0.083* (0.045)	-0.067* (0.035)	0.068** (0.030)
Mean	0.124	0.115	0.128	0.126
Observations	1826	1353	2260	2287
Clusters	313	225	379	393

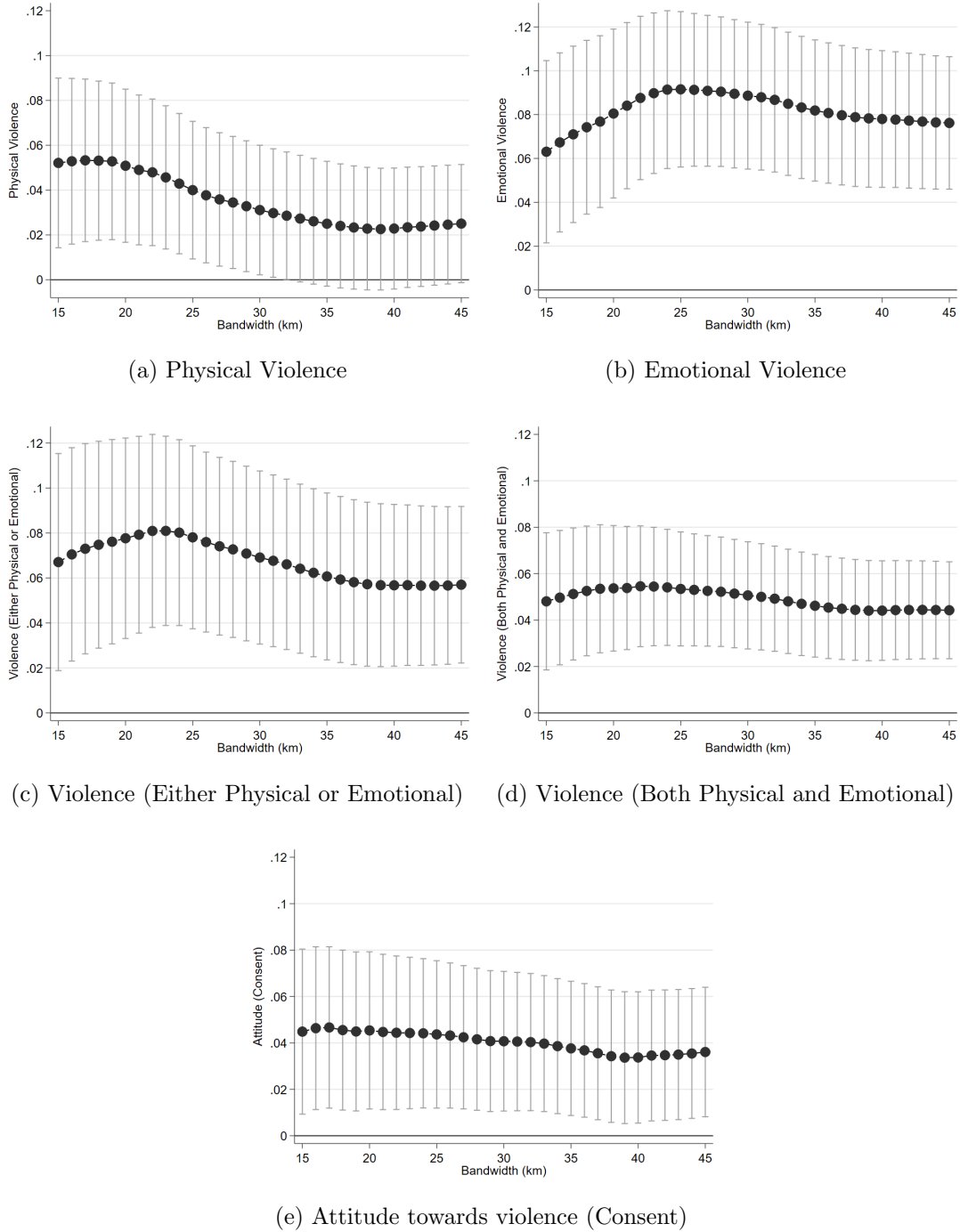
Note: The table shows the results of placebo tests which shift the border by 10 and 30 kilometers to two directions: northwest or southeast. The unit of analysis is DHS survey respondents. The sample is restricted to those within a 30km bandwidth from the placebo border. All regressions control for pre-genocide characteristics, distance to the capital, distance to Vietnam and Thailand borders, 15-km segment fixed effects and survey year fixed effects. Standard errors reported in parentheses are clustered at the DHS village level. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

Figure C.4: Domestic Violence: Sensitivity of results to different orders of polynomial



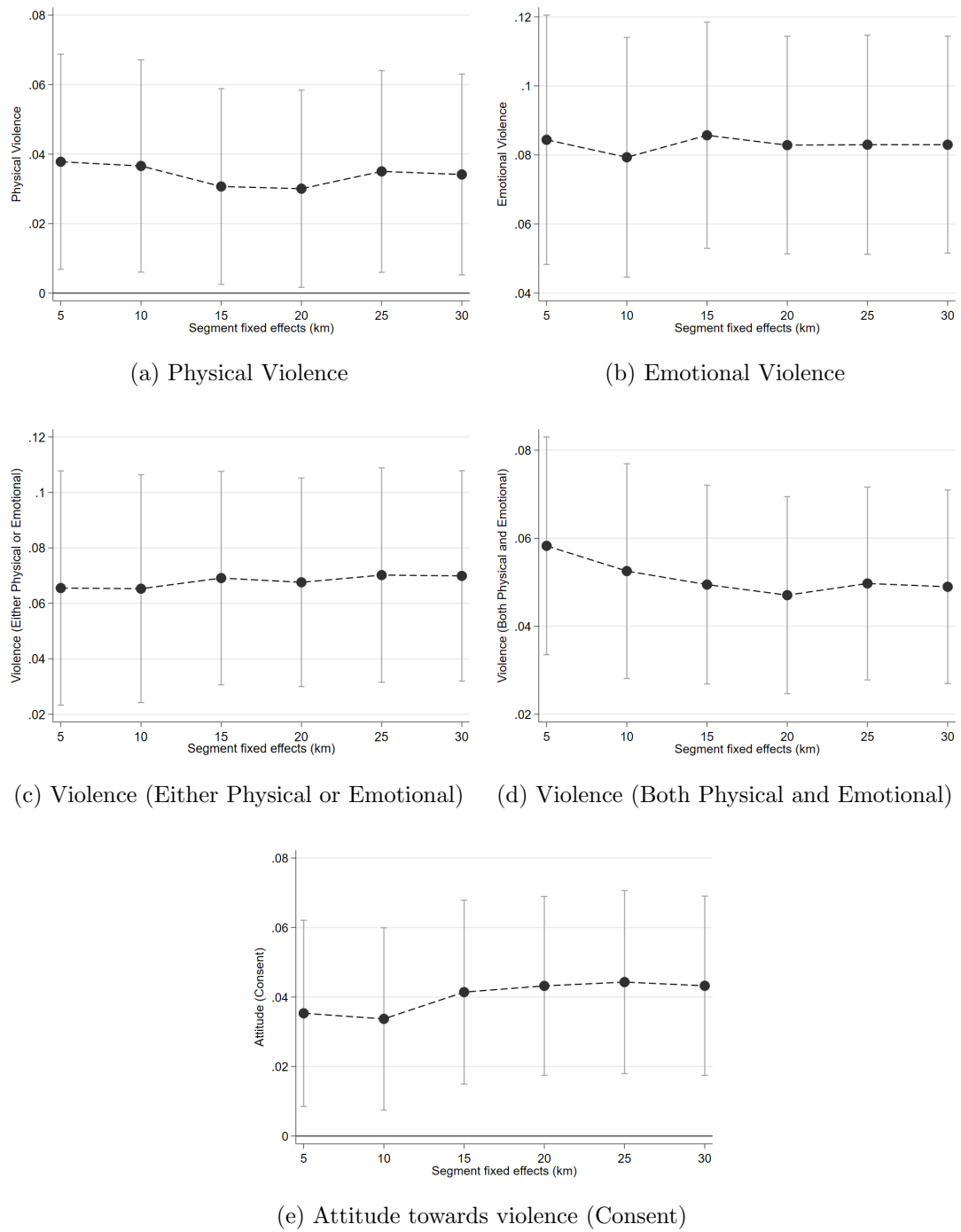
Note: Each dot represents the RD estimate using the specified order of RD polynomial in distance to the border. Capped spikes represent 90% confidence intervals of the estimates.

Figure C.5: Domestic Violence: Sensitivity of results to bandwidth choices



Note: Each sub-graph reports coefficient estimates and confidence intervals for different bandwidth levels ranging from 15 to 45 kilometers (horizontal axis) with 1km intervals. Each dot indicates the RD estimate using the specified bandwidth. Capped spikes represent 90% confidence intervals of the estimates.

Figure C.6: Domestic Violence: Sensitivity of results to different segment fixed effects



Note: Each dot represents the RD estimate using the specified border segment fixed effects. Capped spikes represent 90% confidence intervals of the estimates.

Table C.3: Domestic Violence: Robustness to different specifications

	No Interaction (1)	No Dist. Capital (2)	No weights (3)	Province FE (4)	1km Donut (5)	3km Donut (6)
<i>Panel A. Dependent variable is: Physical Violence</i>						
Genocide	0.031 (0.022)	0.032 (0.022)	0.013 (0.022)	0.040* (0.023)	0.024 (0.025)	0.043 (0.033)
Mean	0.115	0.115	0.115	0.115	0.116	0.126
Observations	2173	2173	2173	2173	1955	1518
Clusters	366	366	366	366	322	243
<i>Panel B. Dependent variable is: Emotional Violence</i>						
Genocide	0.086*** (0.025)	0.086*** (0.025)	0.077*** (0.025)	0.085*** (0.025)	0.087*** (0.032)	0.111*** (0.043)
Mean	0.162	0.162	0.162	0.162	0.169	0.179
Observations	2173	2173	2173	2173	1955	1518
Clusters	366	366	366	366	322	243
<i>Panel C. Dependent variable is: Violence (Either)</i>						
Genocide	0.067** (0.029)	0.068** (0.029)	0.049* (0.029)	0.071** (0.029)	0.058 (0.036)	0.083* (0.046)
Mean	0.197	0.197	0.197	0.197	0.204	0.216
Observations	2173	2173	2173	2173	1955	1518
Clusters	366	366	366	366	322	243
<i>Panel D. Dependent variable is: Violence (Both)</i>						
Genocide	0.050*** (0.018)	0.050*** (0.018)	0.040** (0.017)	0.054*** (0.017)	0.052** (0.021)	0.070** (0.027)
Mean	0.080	0.080	0.080	0.080	0.082	0.089
Observations	2173	2173	2173	2173	1955	1518
Clusters	366	366	366	366	322	243
<i>Panel E. Dependent variable is: Attitude (Consent)</i>						
Genocide	0.048** (0.021)	0.045** (0.021)	0.032 (0.021)	0.044** (0.020)	0.018 (0.025)	0.001 (0.044)
Mean	0.322	0.322	0.322	0.322	0.333	0.365
Observations	10597	10597	10597	10573	9118	6658
Clusters	459	459	459	458	401	297

Note: The unit of analysis is DHS survey respondents. The sample is restricted to those within a 30km bandwidth from the Southwest-West boundary. Column (1) excludes the interaction between the treatment variable and the running variable – distance to the border. Column (2) excludes the distance to the capital in the main regression. Column (3) presents estimates without a triangular kernel weight. Column (4) replaces segment fixed effects with province fixed effects. The last two columns conduct a *donut* exercise by excluding all observations within 1 km and 3 km of the border, respectively, and estimate the model using the remaining observations. Standard errors reported in parentheses are clustered at the DHS village level. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

Table C.4: Domestic Violence: Shifting border

	Shift Northwest		Shift Southeast	
	10km (1)	30km (2)	10km (3)	30km (4)
<i>Panel A. Dependent variable is: Physical Violence</i>				
Genocide	-0.043 (0.041)	0.044 (0.057)	-0.021 (0.030)	0.000 (0.038)
Mean	0.109	0.103	0.122	0.125
Observations	1826	1353	2261	2289
Clusters	313	225	379	393
<i>Panel B. Dependent variable is: Emotional Violence</i>				
Genocide	-0.040 (0.042)	0.244*** (0.050)	-0.045 (0.041)	-0.017 (0.041)
Mean	0.156	0.142	0.170	0.180
Observations	1826	1353	2261	2288
Clusters	313	225	379	393
<i>Panel C. Dependent variable is: Violence (Either)</i>				
Genocide	-0.070 (0.050)	0.238*** (0.061)	-0.024 (0.044)	-0.013 (0.048)
Mean	0.187	0.178	0.203	0.210
Observations	1826	1353	2261	2289
Clusters	313	225	379	393
<i>Panel D. Dependent variable is: Violence (Both)</i>				
Genocide	-0.012 (0.032)	0.050 (0.040)	-0.042* (0.025)	-0.004 (0.034)
Mean	0.0785	0.0671	0.0895	0.0949
Observations	1826	1353	2261	2289
Clusters	313	225	379	393
<i>Panel E. Dependent variable is: Attitude (Consent)</i>				
Genocide	-0.080** (0.040)	0.080 (0.053)	0.006 (0.034)	0.013 (0.053)
Mean	0.322	0.315	0.344	0.369
Observations	9341	6827	10992	11412
Clusters	400	288	478	502

Note: The table shows the results of placebo tests which shift the border by 10 and 30 kilometers to two directions: northwest or southeast. The unit of analysis is DHS survey respondents. The sample is restricted to those within a 30km bandwidth from the placebo border. All regressions control for pre-genocide characteristics, distance to the capital, distance to Vietnam and Thailand borders, 15-km segment fixed effects and survey year fixed effects. Standard errors reported in parentheses are clustered at the DHS village level. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

C.2 Multi-dimensional RD design

This section employs a multi-dimensional RDD to validate the main results. I retain the baseline specification, but the RD polynomial uses a multi-dimensional measure, particularly, demeaned x- and y-coordinates of household locations $f(Geo_c) = x + y$. The results of the multidimensional RDD, which are reported in Table C.5 and Table C.6, are consistent with those obtained in the main analysis.

Table C.5: Multidimensional RDD: Impacts on female empowerment

	Dependent variable is:		
	(1) Fulltime Working	(2) Decision Making	(3) Partner Control
Genocide	-0.054** (0.025)	-0.071** (0.031)	0.058** (0.023)
Mean	0.709	0.563	0.126
Observations	11723	7023	2173
Clusters	459	459	366

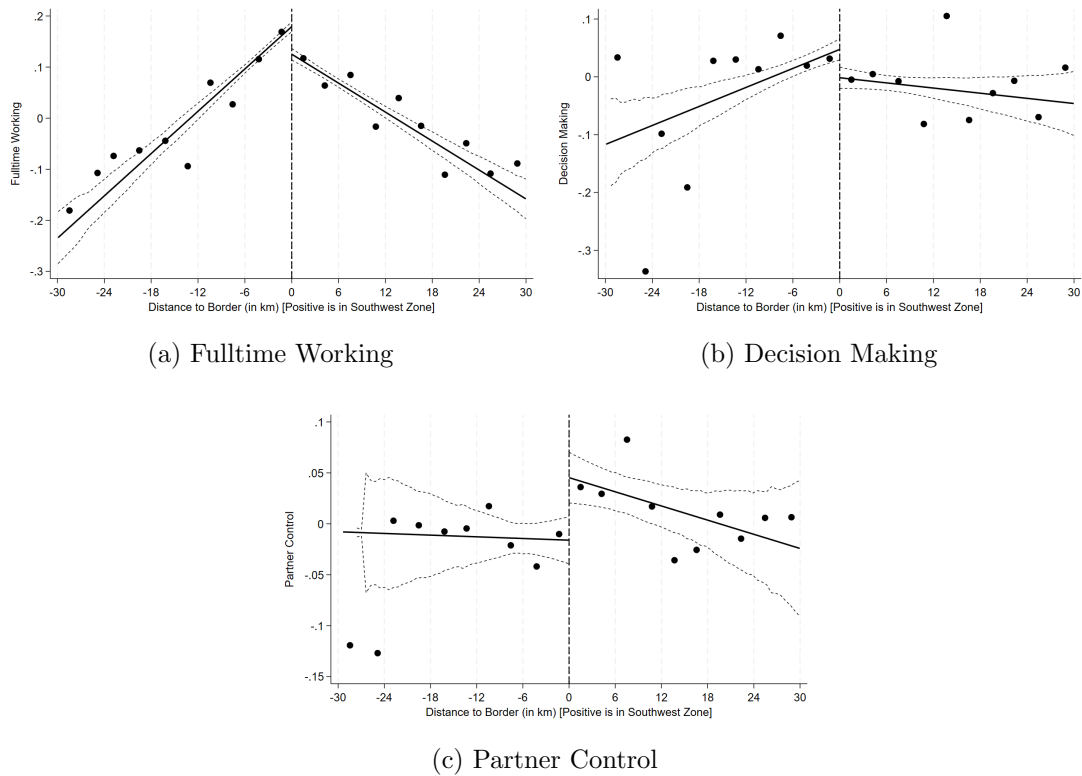
Note: The unit of analysis is DHS survey respondents. The sample is restricted to those within a 30km bandwidth from the Southwest-West boundary. All regressions use a local linear polynomial in spatial coordinates with a triangular kernel. All regressions control for pre-genocide characteristics, distance to the capital, distance to Vietnam and Thailand borders, 15-km segment fixed effects and survey year fixed effects. Standard errors reported in parentheses are clustered at the DHS village level. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

Table C.6: Multidimensional RDD: Impacts on domestic violence

	Dependent variable is:				
	(1) Physical Violence	(2) Emotional Violence	(3) Violence (Either)	(4) Violence (Both)	(5) Attitudes (Consent)
Genocide	0.033 (0.022)	0.087*** (0.025)	0.069** (0.029)	0.051*** (0.018)	0.049** (0.021)
Mean	0.115	0.162	0.197	0.0802	0.322
Observations	2173	2173	2173	2173	10597
Clusters	366	366	366	366	459

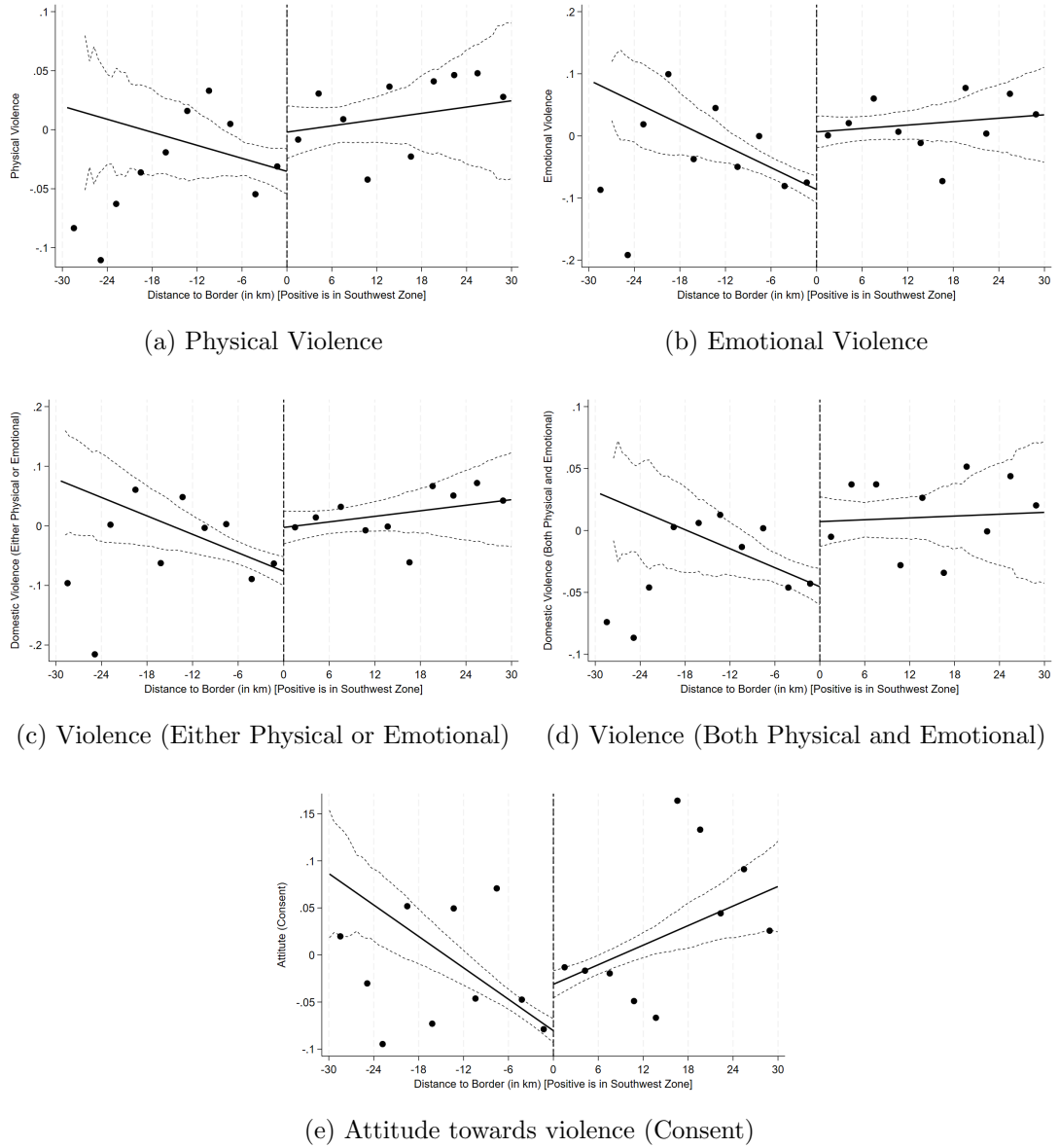
Note: The unit of analysis is DHS survey respondents. The sample is restricted to those within a 30km bandwidth from the Southwest-West boundary. All regressions use a local linear polynomial in spatial coordinates with a triangular kernel. All regressions control for pre-genocide characteristics, distance to the capital, distance to Vietnam and Thailand borders, 15-km segment fixed effects and survey year fixed effects. Standard errors reported in parentheses are clustered at the DHS village level. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

Figure C.7: Female Empowerment: Multidimensional RDD plots



Note: The points represent binned residuals derived from a main regression of the outcome variable on a linear polynomial in x- and y-coordinates of household locations and other control variables. Solid lines depict local linear regressions, separately estimated on each side of the threshold, while dashed lines represent 95% confidence intervals. “Positive” values of distance indicate locations in the Southwest Zone.

Figure C.8: Domestic Violence: Multidimensional RDD plots



Note: The points represent binned residuals derived from a main regression of the outcome variable on a linear polynomial in x- and y-coordinates of household locations and other control variables. Solid lines depict local linear regressions, separately estimated on each side of the threshold, while dashed lines represent 95% confidence intervals. “Positive” values of distance indicate locations in the Southwest Zone.

C.3 Forced marriage during the genocide

Between 1975 and 1979, the Khmer Rouge used forced marriages to promote regime loyalty and dismantle traditional family structures, with about one in three marriages during this period being forced (Heuveline and Poch, 2006; Jacobs, 2022). To investigate forced marriage during this period, I use a basic difference-in-differences (DID) design and examine the following three outcomes: (1) whether women knew their husbands on the day of marriage, (2) whether they chose their husbands, and (3) whether their names are absent from their husbands' household book. However, data on these variables are only available in the 2000 DHS. The regression takes the following form:

$$Y_{icd} = \alpha + \beta \times Genocide_c + \eta Marriage_c + \theta Genocide_c \times Marriage_c + f(Geo_c) + \lambda \mathbf{X}_c + \gamma \mathbf{D}_c + \zeta_d + \epsilon_{icgt} \quad (C.1)$$

where Y_{icgt} is the outcome variable of interest for a woman i in DHS cluster c , district d , and $Genocide_c$ is an indicator equal to 1 if a woman is currently located in an area that was part of the extremist Southwest Zone during the Khmer Rouge era and equal to zero if she resides in an area formerly in the moderate West Zone. $Marriage_c$ is a binary equal to 1 if her first marriage is between April 1975 and January 1979. The function $f(Geo_c)$ controls for smooth functions of the geographic locations of village c , particularly the distance to the Southwest-West zone border. $\mathbf{X}_c$ is a vector of covariates, control for all pre-genocide characteristics.¹ $\mathbf{D}_c$ is a vector of geographic covariates, controlling for distance to the capital of Cambodia - the largest urban city, distance to Vietnam borders and distance to Thailand borders. ζ_d is district fixed effects. Standard errors are clustered at the DHS survey cluster level.

Table C.7 presents the results. Women getting married during the genocide are 16.1 percentage points more likely to know their husband on their wedding day and 20.7 percentage points less likely to choose their husband. They are also more likely to be unregistered in their husbands' household book. No statistically significant effects

¹Pre-genocide characteristics include geographic characteristics (elevation, tropics/lowland), demographic characteristics (population density in 1970) and economic characteristics (agricultural activities and distance to main roads/railways in 1970)

Table C.7: Mechanism: Marriage during Genocide

	Dependent variable is:		
	(1) Knew Husband	(2) Chose Husband	(3) No Register
Genocide	-0.057 (0.051)	-0.012 (0.043)	-0.142** (0.062)
Marriage	0.161*** (0.060)	-0.207*** (0.057)	0.137* (0.071)
Genocide $\times$ Marriage	0.058 (0.084)	0.066 (0.082)	-0.142 (0.089)
Mean	0.296	0.759	0.185
Observations	1932	1931	1006

Note: The unit of analysis is DHS survey respondents. All regressions control for pre-genocide characteristics, distance to the capital, distance to Vietnam and Thailand borders, and district fixed effects. *Knew Husband* is a binary variable indicating whether a woman knew her husband on their wedding day. *Chose Husband* is a binary variable indicating whether the woman chose or participated in choosing her husband. *No Register* refers to whether a woman is not registered in husband's household book. Standard errors reported in parentheses are at the DHS village level. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

are observed for the interaction between marriage during the genocide and residence in the extremist Southwest Zone. This suggests that while forced marriage practices were widespread during the Khmer Rouge regime, their intensity or impact may not have varied substantially between the Southwest and West Zones. It is possible that the regime's marriage policies were centrally coordinated and consistently applied across different zones, regardless of their level of repression. Additionally, the lack of significant findings may be partly attributed to limitations in sample size, which could reduce the statistical power to detect regional variation.

C.4 Additional Tables and Figures

Table C.8: Descriptive statistics

	All observations				Within 30km distance			
	All	West	Southwest	Diff	All	West	Southwest	Diff
Panel A: Female Empowerment								
Fulltime Working	0.57 (0.49)	0.60 (0.49)	0.55 (0.50)	0.05*** (8.35)	0.71 (0.45)	0.72 (0.45)	0.69 (0.46)	0.03*** (3.47)
Decision Making	0.55 (0.50)	0.47 (0.50)	0.61 (0.49)	-0.14*** (-17.56)	0.56 (0.50)	0.57 (0.50)	0.56 (0.50)	0.01 (0.99)
Partner Control	0.13 (0.33)	0.13 (0.33)	0.13 (0.33)	-0.00 (-0.01)	0.13 (0.33)	0.10 (0.31)	0.14 (0.35)	-0.04*** (-2.80)
Panel B: Domestic Violence								
Physical Violence	0.12 (0.33)	0.11 (0.31)	0.13 (0.34)	-0.02** (-2.06)	0.11 (0.32)	0.10 (0.30)	0.13 (0.34)	-0.03** (-2.32)
Emotional Violence	0.17 (0.38)	0.15 (0.36)	0.19 (0.39)	-0.04*** (-3.50)	0.16 (0.37)	0.14 (0.34)	0.19 (0.39)	-0.05*** (-3.23)
Domestic Violence (Either)	0.21 (0.41)	0.19 (0.39)	0.22 (0.42)	-0.03*** (-2.89)	0.20 (0.40)	0.17 (0.38)	0.22 (0.41)	-0.04*** (-2.65)
Domestic Violence (Both)	0.09 (0.28)	0.07 (0.26)	0.10 (0.30)	-0.02*** (-2.93)	0.08 (0.27)	0.06 (0.24)	0.10 (0.30)	-0.04*** (-3.25)
Attitude (Consent)	0.38 (0.48)	0.32 (0.47)	0.42 (0.49)	-0.10*** (-16.20)	0.32 (0.47)	0.29 (0.46)	0.35 (0.48)	-0.05*** (-6.09)
Panel C: Other characteristics								
Elevation/Altitude (meters)	21.60 (25.23)	23.90 (27.49)	19.82 (23.17)	4.07*** (14.01)	21.18 (18.15)	21.02 (17.22)	21.32 (18.95)	-0.30 (-1.02)
Tropics, lowland	0.49 (0.50)	0.54 (0.50)	0.44 (0.50)	0.10*** (17.50)	0.53 (0.50)	0.51 (0.50)	0.55 (0.50)	-0.04*** (-4.93)
Population density (1970)	0.71 (0.46)	0.56 (0.50)	0.81 (0.39)	-0.25*** (-48.49)	0.83 (0.38)	0.75 (0.43)	0.89 (0.31)	-0.14*** (-22.43)
Agricultural activities (1970)	0.64 (0.48)	0.50 (0.50)	0.74 (0.44)	-0.23*** (-43.50)	0.68 (0.47)	0.58 (0.49)	0.78 (0.42)	-0.20*** (-26.41)
Distance (km) to								
Main roads/railways (1970)	4.49 (6.27)	5.51 (8.08)	3.69 (4.20)	1.82*** (24.10)	3.03 (4.10)	3.26 (4.38)	2.83 (3.82)	0.43*** (6.38)
Capital	52.82 (49.17)	56.28 (57.24)	50.13 (41.64)	6.15*** (10.65)	26.69 (42.15)	30.89 (45.76)	22.90 (38.22)	7.99*** (11.58)
Vietnam borders	74.09 (45.54)	112.82 (38.23)	43.97 (21.84)	68.85*** (189.43)	73.32 (15.12)	83.46 (13.21)	64.19 (10.08)	19.27*** (99.98)
Thai borders	192.17 (44.65)	172.93 (56.41)	207.12 (23.61)	-34.19*** (-66.97)	195.71 (33.23)	187.32 (37.65)	203.28 (26.47)	-15.96*** (-29.85)
Observations	31720	13874	17846	31720	15113	7166	7947	15113

Note: The table provides the mean/standard deviation of the corresponding variables. "All" means the whole sample, "West" means the control group includes observations located in the moderate West Zone, and "Southwest" means the treated group includes observations located in the extremist Southwest Zone. "Within 30 km distance" means the sample is restricted to observations located within 30km of the border. "Diff" shows the mean difference between treated and control groups. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

Table C.9: Post-genocide Population Density

(1) Post-genocide Population Density	
Genocide	-2.678 (2.419)
Mean	5.518
Observations	9618
Clusters	199

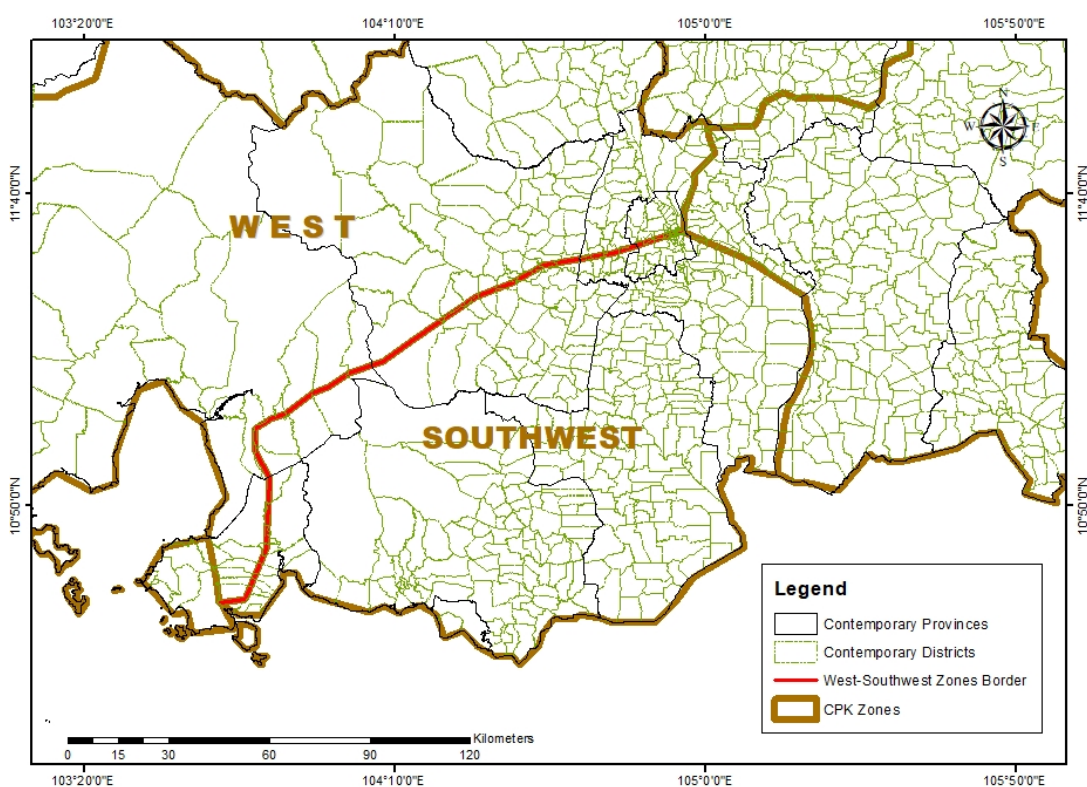
Note: The unit of analysis is DHS villages. The sample is restricted to villages within a 30km bandwidth from the Southwest-West boundary. All regressions use a local linear polynomial of distance to the Southwest/West border with a triangular kernel weight. All regressions control for pre-genocide characteristics, distance to the capital, distance to Vietnam and Thailand borders, 15-km segment fixed effects and year fixed effects. Heteroskedasticity-robust standard errors are reported in parentheses. ***(**)(*) indicates significance at the 1%(5%)(10%) level.

Figure C.9: Southwest-West Zones Border and 1970 Administrative Divisions



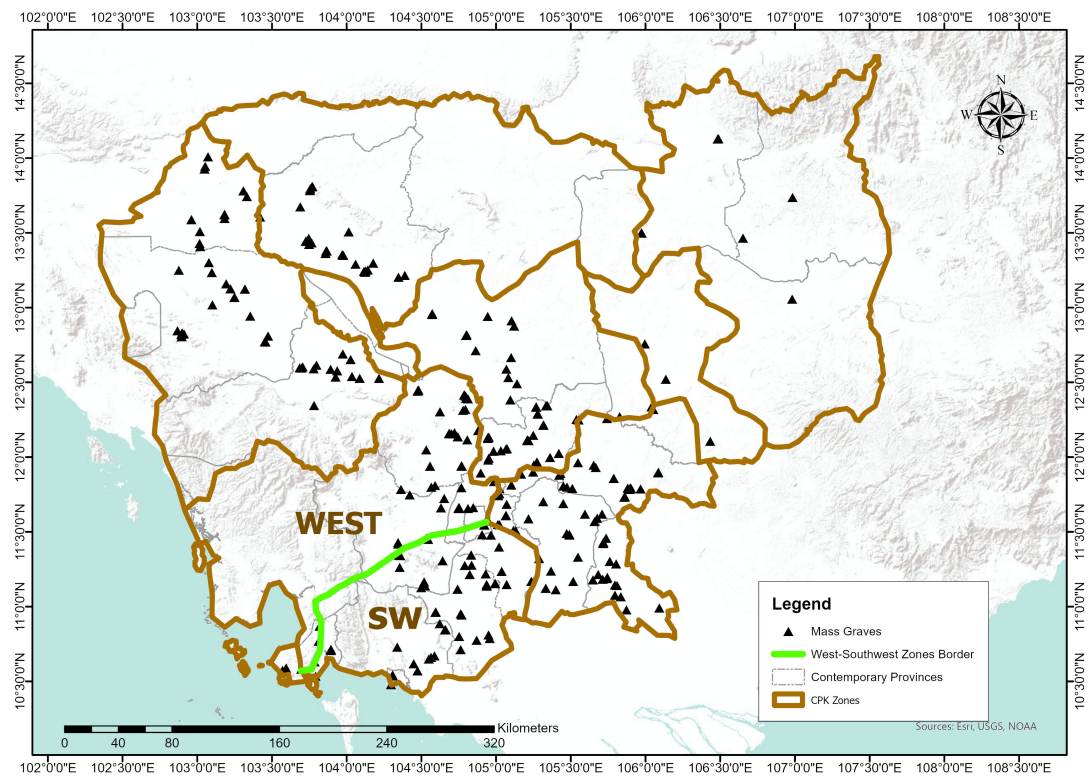
Notes: The map displays pre-genocide administrative divisions of Cambodia, digitized from the 1970 Indochina Atlas. It also shows that the Border between the Southwest and West Zones does not align with any pre-genocide administrative divisions.

Figure C.10: Southwest-West Zones Border and Contemporary Administrative Divisions



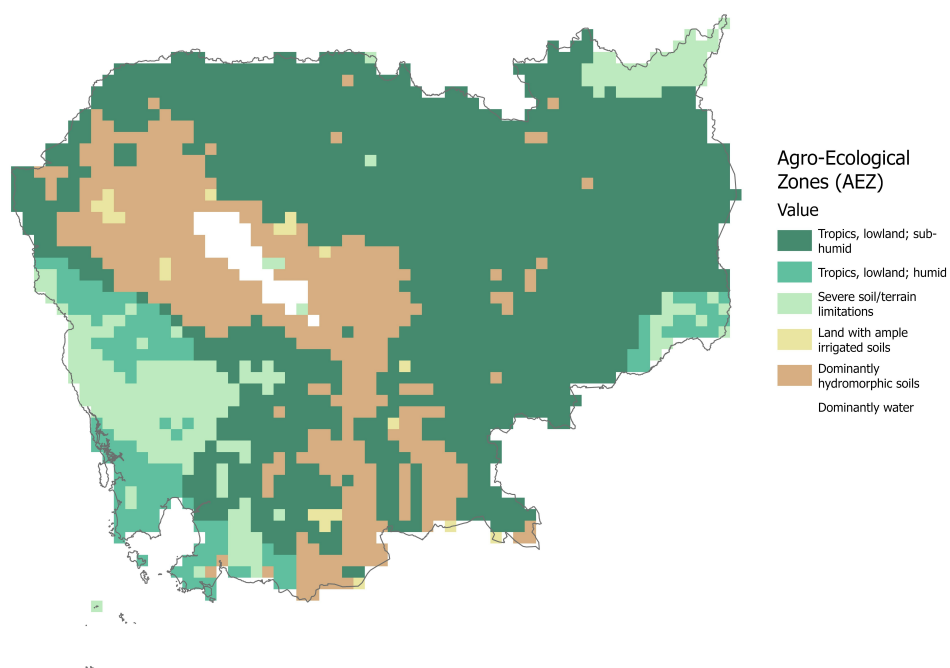
Notes: This map shows contemporary administrative divisions in Cambodia. The border between the Southwest and West Zones does not correspond to any contemporary administrative boundaries.

Figure C.11: 309 Democratic Kampuchea mass grave sites



Notes: This map displays the locations of 309 sites containing a total of 18,953 Democratic Kampuchea (DK) mass graves in Cambodia. Data is from Yale University's Cambodian Genocide Databases.

Figure C.12: Agro-ecological Zones classes



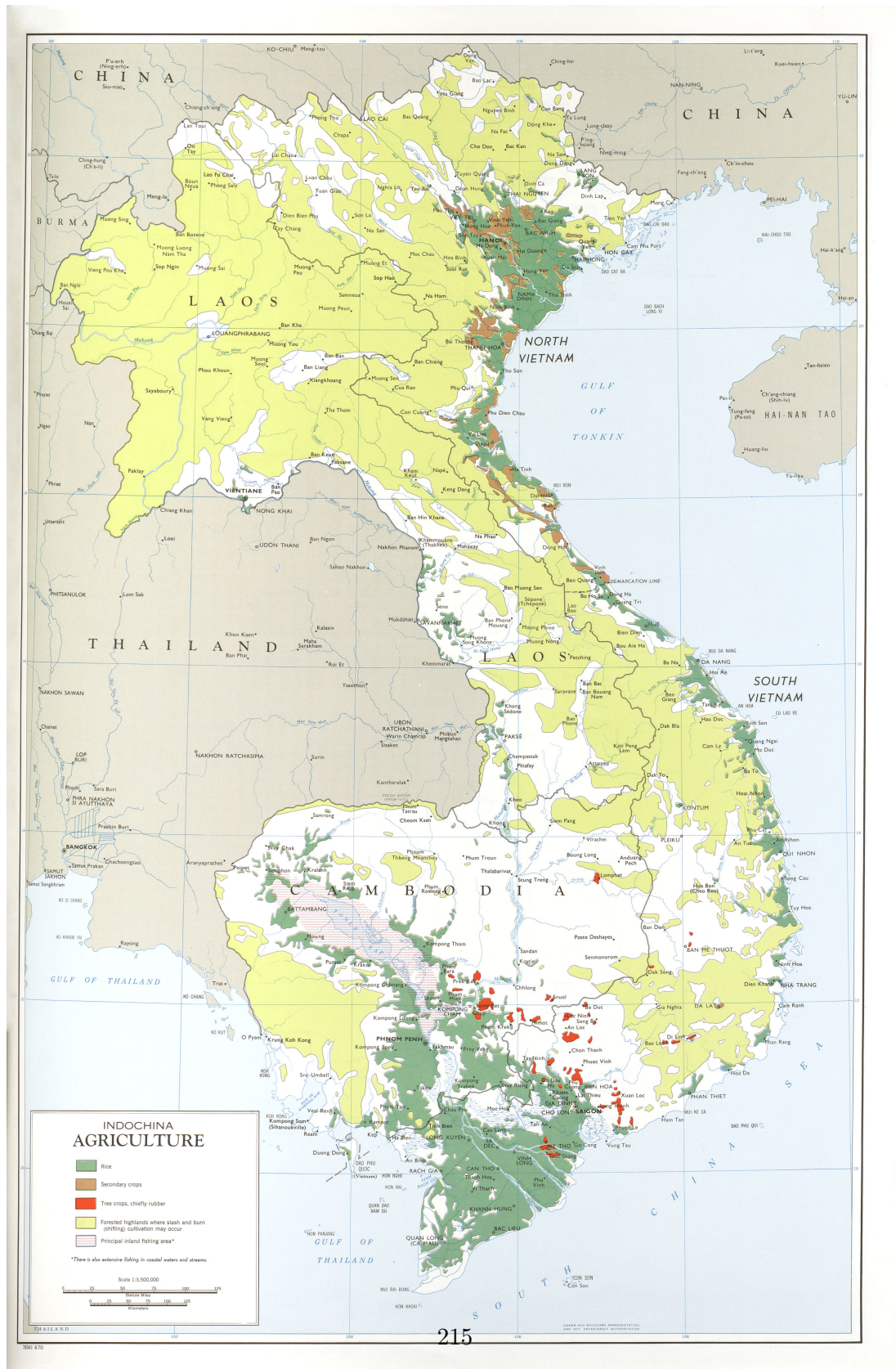
Note: The map overlays Cambodia to the agro-ecological zones (AEZs) as classified by The Food and Agriculture Organization of the United Nations (FAO) and the International Institute for Applied Systems Analysis (IIASA). Geographic areas belonging to the same AEZ category exhibit analogous climatic characteristics, encompassing rainfall and temperature patterns, and thus possess equivalent agricultural capabilities. Map is drawn on ArcGIS.

Figure C.13: Indochina Transportation in 1970



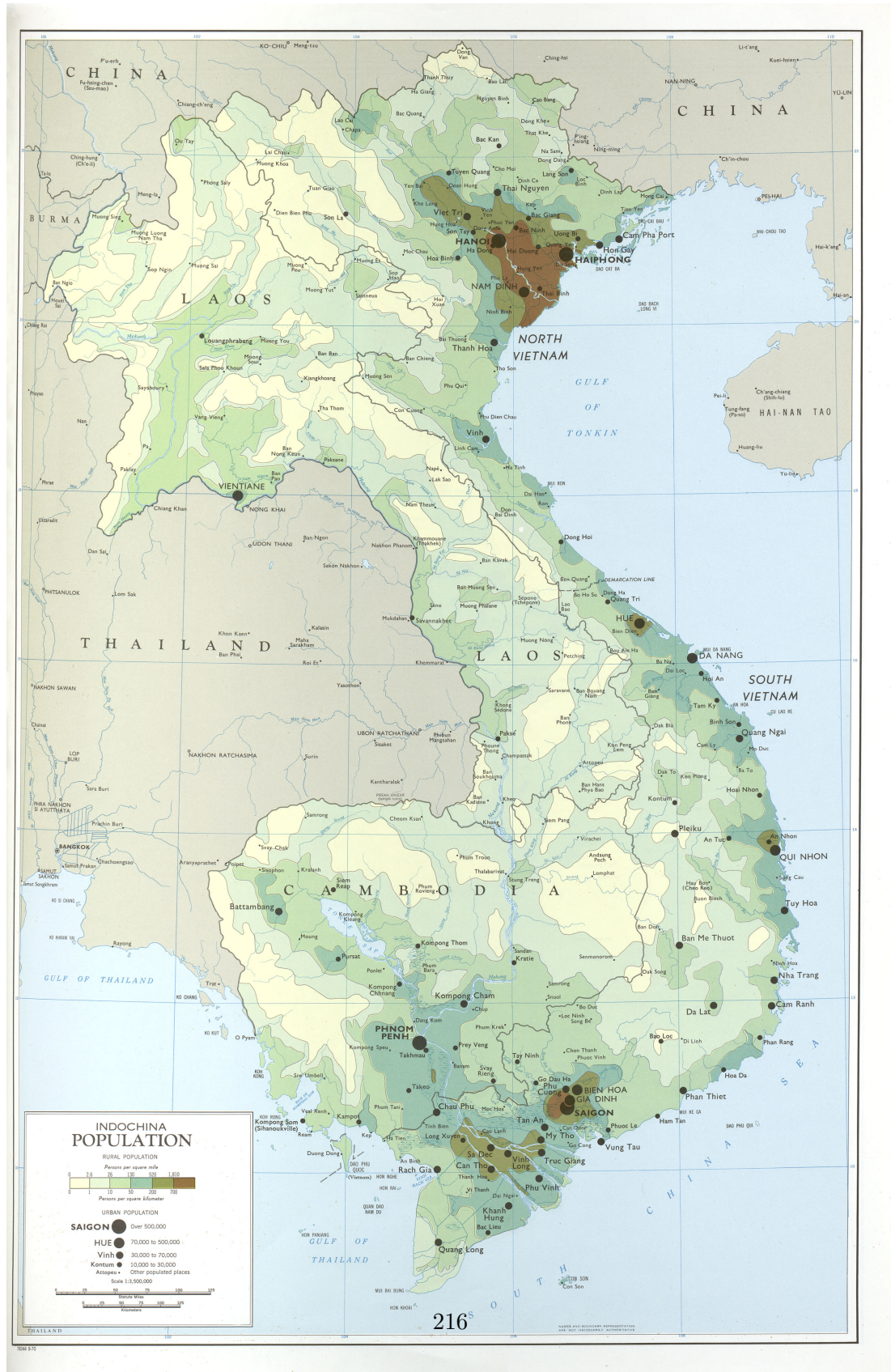
Note: Indochina Transportation map, which was in "Indochina Atlas", published in October 1970 by the Directorate of Intelligence, Office of Basic and Geographic Intelligence, U.S. Central Intelligence Agency.

Figure C.14: Indochina Agriculture in 1970



Note: Indochina Agriculture map, which was in "Indochina Atlas", published in October 1970 by the Directorate of Intelligence, Office of Basic and Geographic Intelligence, U.S. Central Intelligence Agency.

Figure C.15: Indochina Population in 1970



Note: Indochina Population map, which was in "Indochina Atlas", published in October 1970 by the Directorate of Intelligence, Office of Basic and Geographic Intelligence, U.S. Central Intelligence Agency.