NBER WORKING PAPER SERIES

WORLD WAR II BLUES:
THE LONG–LASTING MENTAL HEALTH EFFECT OF CHILDHOOD TRAUMA

Mevlude Akbulut-Yuksel
Erdal Tekin
Belgi Turan

Working Paper 30284
http://www.nber.org/papers/w30284

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
July 2022, Revised November 2024

The authors thank Kathryn Johnson for excellent research assistance. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

World War II Blues: The Long–lasting Mental Health Effect of Childhood Trauma
Mevlude Akbulut-Yuksel, Erdal Tekin, and Belgi Turan
NBER Working Paper No. 30284
July 2022, Revised November 2024
JEL No. H56, I10, I12, I18

ABSTRACT

There has been a revival of warfare and threats of interstate war in recent years as the number of countries engaged in armed conflict surged dramatically, reaching levels unprecedented since the end of the Cold War. This is happening at a time when the global burden of mental health illness is also on the rise. We examine the causal impact of early life exposure to warfare on long–term mental health, using novel data on the amount of bombs dropped in German cities by Allied Air Forces during World War II (WWII) and the German Socio-economic Panel. Our identification strategy leverages a generalized difference-in-differences design, exploiting the plausibly exogenous variation in the bombing intensity experienced by the former West German cities during the war as a quasi-experiment. We find that cohorts younger than age five at the onset of WWII or those born during the war are in significantly worse mental health later in life when they are between the ages of the late 50s and 70s. Specifically, an increase of one standard deviation in the bombing intensity experienced during WWII is associated with about a 10 percent decline in an individual's long–term standardized mental health score. This effect is equivalent to a 16.2 percent increase in the likelihood of being diagnosed with clinical depression. Our investigation suggests that mechanisms such as the destruction of healthcare infrastructure, the increased burden on the healthcare system, and wealth losses during WWII exacerbate the adverse impact of bombing exposure on long–term mental health. Conversely, war relief funds transferred to municipalities following the war have a mitigating impact. Our findings are robust to numerous empirical checks and specifications. With the mental health impact of childhood exposure to warfare persisting well into the late stages of life, the global burden of mental illness may be aggravated for many years to come. Our findings imply that prioritizing children and a long–term horizon in public health planning and response may be critical to mitigating the adverse mental health consequences of exposure to armed conflict.

Mevlude Akbulut-Yuksel
Department of Economics
Dalhousie University
PO Box 15000
6214 University Avenue Halifax,
Nova Scotia
B3H 4R2 CANADA
and IZA
mevlude@dal.ca

Erdal Tekin
School of Public Affairs
American University
4400 Massachusetts Avenue NW
Washington, DC 20016-8070 and
IZA
and also NBER
tekin@american.edu

Belgi Turan
Department of Economics
TOBB University of Economics and Technology
Sogutozu, Ankara
TURKEY
belgituran@etu.edu.tr

1 Introduction

Mental health disorders impose an enormous societal burden globally, accounting for one in three years lived with disability, and costing the world economy 2.5 trillion dollars annually–a figure that is projected to rise to six trillion dollars by 2030 (de Menil and Glassman, 2015; Mnookin, 2016). Furthermore, the majority of people with mental illness receive no treatment, even in economically advantaged societies (Alonso et al., 2018; Thornicroft et al., 2017). Accordingly, there is an increased acknowledgment among governments and international organizations of the important role that mental health plays in achieving global development goals and an urgent need to scale up quality mental health services. As a sign of this recognition, mental health was included in the United Nations Sustainable Development Goals (SDGs) in September 2015, and the World Health Organization (WHO) identified mental health as an area of accelerated action in 2018. A critical step toward formulating effective treatments and preventive strategies to diminish the global burden of mental illness requires a thorough understanding of the factors and disturbances that trigger these disorders throughout the life cycle.

In this paper, we provide causal evidence on the long–term mental health effects of childhood trauma induced by exposure to war, leveraging a unique historical dataset on the bombing intensity of Germany during World War II (WWII) and individual data from 2002–2010 waves of German Socio-Economic Panel (GSOEP). Specifically, we exploit plausibly exogenous city–by–cohort variation in the intensity of childhood exposure to the aerial attacks carried out by Allied Air Forces ("AAF") using a generalized difference–in–differences strategy. Our measure of bombing intensity is defined as the total number of bombs dropped per square kilometer in each city over the course of WWII. Our focus is on individuals who were younger than age five at the onset of WWII or those born at some point during the war. Specifically, our treatment cohort consists of individuals born between 1934 and 1945 at the onset of the war.

Our decision to focus on this group of individuals is supported by research in medical and child psychology, which shows that early-life traumatic experiences can result in long-term changes in brain structure with significant implications for mental health in later years. Some of these studies specifically examine the enduring impact of childhood war trauma (Catani et al., 2008;

Betancourt et al., 2013; Barenbaum et al., 2004; Shonkoff and Garner, 2012; Heim and Nemeroff, 2001). Additionally, a considerable body of research underscores the profound and lasting effects of trauma during the early years of life, particularly the first five years, on brain development, mental health, and cognitive function. These studies consistently show that this early period is highly sensitive to adverse experiences, making it crucial for understanding the long-term consequences of childhood trauma (e.g., Gunnar and Herrera, 2015; Lupien et al., 2009; Shonkoff and Garner, 2012; De Bellis et al., 2009).[1]

One of the main factors motivating our focus on WWII is the fact that it is a momentous event in modern history. It is therefore important to understand the long-lasting effects on the mental health of populations who survived this tragedy in its own right. Studying the context of Germany is particularly important because of the tremendous physical and human toll that the war took on the German population. Over one and a half million tons of bombs were dropped on German soil, which caused, in addition to a significant number of casualties, significant distress and disruptions in daily life among survivors via uncertainty of the aerial attacks as well as the destruction of the dwellings, hospitals, roads, schools and other public spheres (United States Strategic Bombing Survey (USSBS), 1945; Davis, 2006). Interestingly, mental health problems among the elderly appear to be severely overlooked in Germany, where 35 percent of suicide deaths occur among people over the age of 65 despite the fact that this age group accounts for only 21 percent of the population (Pladson, 2019). In general, the specific causes of mental illness among older populations are often poorly diagnosed, and psychological ailments detected in these persons are misperceived as normal manifestations of aging (Corcoran et al., 2013).

Our emphasis on warfare is also motivated by the troubling observation that exposure to combat during childhood, or armed conflict in general, is a particularly extreme form of a traumatic experience, and yet it is common worldwide, affecting as many as 426 million children (Ostby et al., 2020).[2] According to the United Nations Children's Fund (UNICEF), the number of countries experiencing conflict, involving weapons or violence in 2021 was the highest in 30 years.[3] Parallel

[1]In the economics literature, Almond and Currie (2011) lay out the mechanisms that illustrate the presence of a causal relationship between shocks in the first five years of life and future outcomes.

[2]This number refers to the estimated number of children under age 18 who lived within 50 km of a conflict zone where actual fighting took place. Of these, more than 71 million are 0–5–year–olds who lived in areas of armed conflict during their entire lifetime (Ostby et al., 2020).

[3]See https://https://www.unicef.org/children-under-attack.

to this trend, the number of children living in territories or countries in armed conflict or emerging from war has been escalating over the last few decades (Ostby et al., 2020). The nature of war has also changed with combat zones becoming increasingly widespread with the destruction caused on a larger scale than before (Levy and Sidel, 2008).

The significance of our analysis is further predicated by recent armed conflicts, including the ongoing war in Ukraine, the violence in Gaza, and conflicts in regions such as Syria and Sudan. These events have forced millions of children to abandon their homes and schools, seeking refuge in bomb shelters, underground metro stations, parking lots, and overcrowded refugee camps. The trauma, fear, and uncertainty these children are enduring may have profound and long-lasting effects on their mental health, shaping their well-being throughout their lives. As a physician from Doctors Without Borders noted, "Every child in these war-torn regions is now experiencing multiple adverse childhood events, and that is one of the uncounted casualties that will ripple throughout generations" (Kondeleon, 2022).

Beyond armed–conflict and warfare, children are also faced with an alarming increase in exposure to terrorism, gun violence, and mass public and school shootings as a common occurrence in their lives (Chrisman and Dougherty, 2014; Kadir et al., 2018; Cabral et al., 2021; Soni and Tekin, In press). At a time when the incidences of wars, terrorism, and mass gun violence are on the rise worldwide, it is important to understand whether or not exposure to armed conflict has a deleterious impact on mental health and the extent to which it contributes to the global burden of mental illness.

Broadly, our paper contributes to the literature on the impact of adverse childhood experiences on long–term mental health. While there is an accumulating body of research on the relationship between early childhood experiences and the development of mental health ailments later in life, causal evidence obtained from credible research designs is relatively rare (Adhvaryu et al., 2019; Persson and Rossin–Slater, 2018). Moreover, there is a limited understanding of how specific forms of trauma, such as warfare, uniquely influence long-term outcomes, especially long-term mental health, despite the growing vulnerability of children to combat and warfare globally. One exception is Singhal (2019), who shows that early–life exposure to bombing during the American War in Vietnam has a deleterious effect on the mental health status of the Vietnamese population in adulthood. As another example, Kim (2017) documents that exposure to Korean War in late childhood to early teenage years has a long–run negative impact on indicators of mental health, including depression, fear, insomnia, and loneliness. Finally, Bratti et al. (2015) find that trauma experienced during the war in Bosnia and Herzegovina between 1992 and 1995 war significantly increased the likelihood of depression six years after the conflict.[4] The present study builds on such findings by focusing on the specific context of WWII bombing in Germany, which offers a distinct context for examining these effects.

Our paper offers complementary evidence to these studies, but it also breaks new ground by incorporating information on the role of potential pathways, such as the loss of saving and housing stock during the war, damage to healthcare infrastructure, loss of healthcare personnel, the number of child patients, out–of–wedlock births, changes in the infant mortality rate, and the amount of war relief payments paid to municipalities in 1948 to help households profoundly affected by the consequences of war. Furthermore, our treatment measure defined as bombing intensity allows us to explore a response–dose relationship, in which the impact of exposure to warfare on mental health may be progressively increasing in the severity of bombing. Finally, the wars that occurred in Vietnam, Bosnia, and Herzegovina, and Korea are relatively recent incidents compared to WWII. As a result, we are able to study potential effects that might persist into much later in life than could be examined by those more recent wars. In fact, the need to study the "long–term" consequences of exposure to war trauma has been acknowledged (Chrisman and Dougherty, 2014).

Aside from these contributions, our paper is closely related to two strands of literature. The first is the "fetal origins" literature in economics that relates early life pre– and post–natal environment and conditions to health and well–being in adult life. This research documents that early life exposure to conditions like malnutrition, extreme weather, disease, income shocks, maltreatment, and maternal stress has long–lasting effects on a variety of outcomes, including educational attainment, labor market productivity, and physical and mental health.[5] While many early-life

[4]There is a related and larger literature on the impact of warfare on mental health that relates to the conditions of individuals directly involved in armed conflicts, like military personnel or child soldiers or direct victims of wars like wounded civilians (e.g., Annan et al., 2011; Blattman and Annan, 2010; Cesur Sabia, and Tekin, 2013; Gade and Wenger, 2011; Lyk-Jensen et al., 2016). The findings from this literature consistently point to a negative effect of combat exposure on mental health among affected populations.

[5]Examples include Almond and Mazumder (2011) for malnutrition, Almond (2006) and Bleakley (2007; 2010) for disease environment, Adhvaryu et al. (2016), Bozzoli and Quintana-Domeque (2014), Currie (2009), and Hoynes et al. (2016) for income, Currie, and Tekin (2012) for maltreatment, and Persson and Rossin-Slater (2018) for maternal bereavement and stress. See Almond and Curie (2011) for an extended review.

traumas share common mechanisms of long-term impact, such as stress and deprivation, warfare, particularly sustained bombing campaigns, differs from other early-life traumas through its unpredictability, severity, and widespread disruption of social and physical environments. These factors likely interact with early childhood development in unique ways, beyond what is typically seen with poverty, malnutrition, or maternal stress, necessitating a focused examination of its long-term effects. Therefore, this paper adds to the fetal origins literature by quantifying the long–term effect of exposure to warfare following conception and during the early years of life on mental health in the late adult years. Second, our paper also contributes to the accumulating literature on the immediate and long–term consequences of combat exposure on long–term outcomes in general. The studies in this literature show that exposure to warfare as a child has detrimental effects on later life outcomes including physical health, human capital, labor market productivity and earnings, risk–aversion, and trust and social engagement.[6]

Our research design pays careful attention to accounting for confounding factors as well as ensuring that our results are not simply an artifact of the pre–war or post-war cohort-specific trends in long–term mental health. For example, our empirical analysis controls for city and year fixed effects, linear state and city trends, and the interaction of prewar city-level indicators with linear trends. Furthermore, we perform several placebo experiments in which we demonstrate that the cohort-specific effects of exposure to aerial bombing on long–term mental health are only significant for the cohorts who were five and younger at the time of the war, while there is no discernible effect for the older or younger birth cohorts.

We find that exposure to warfare within the very early years of life increases the likelihood of poor mental health much later in life. According to our analysis, a one-standard-deviation (82.7 bombs per square kilometer) increase in bombing intensity results in an approximately 10 percent

[6] See Akbulut-Yuksel (2014; 2017), Akresh et al. (2012), Akresh et al. (2021), Conzo and Salustri (2019), Kim and Lee (2014), Mansour and Rees (2012), Kesternich et al. (2014), Minoiu and Shemyakina (2014), and Ramirez and Haas (2021) as examples. Kesternich et al. (2014), who examine the long-run effects of WWII on the socioeconomic status and health of older individuals in thirteen European countries, consider the likelihood of depressive symptoms among other outcomes. Using data from the Survey of Health, Aging, and Retirement in Europe (SHARE), the authors show that living in a war country during the period of WWII is associated with a higher likelihood of expressing depressive symptoms at older ages. While we too focus on WWII in our analysis, our paper is different from Kesternich et al. (2014) in important dimensions, including data source, measurement of mental health, and the research design. Importantly, mental health is only one of the many outcomes considered in Kesternich et al. (2014) and expressed as a single dummy variable based on whether the respondent suffers from more than three depression symptoms on the EURO-D scale. In contrast, we rely on a measure derived from the 12-item Center for the Epidemiological Studies of the Short Form (CED-D), the main scale used to measure depressive symptoms internationally. Furthermore, the empirical design in Kesternich et al. (2014) is based on comparing the outcomes between European countries which suffered destruction (Austria, Belgium, Czech Republic, France, Germany, Greece, Netherlands, and Poland) versus countries not affected by the war (Denmark, Switzerland, and Sweden). In contrast, we use within-country variation in the intensity of bombing intensity in Germany across cohorts.

decline in the long–term standardized mental health score of an individual who was younger than five years of age at the onset of WWII or born during the war, relative to someone else in the control cohort. This effect translates into a 16.2 percent rise in the likelihood of being clinically depressed. Our examination into mechanisms suggests that measures capturing the extent of destruction in healthcare infrastructure and the loss in both housing and non-housing wealth during WWII exacerbate the negative impact of bombing exposure on long–term mental health, while the size of war relief funds transferred to municipalities following the war in 1948 has a mitigating impact. Our findings are robust to numerous empirical checks and specifications. For example, we test the robustness of our results to measurement and sampling errors, and changes in sample composition that could be associated with parental investment, selective wartime, and long–term mortality and fertility. These results reveal that there is no meaningful variation in the long–term mortality rates, in the size of the wartime cohorts across cities or selective sample attrition with varying bombing intensity. Our results also indicate that the bombing intensity and prewar city characteristics in a given city fails to predict a battery of parental characteristics including the mother's age at birth, parental education, father's occupation, whether the child's parent died during WWII or data on their father is missing.

Our study has important policy implications. First, it highlights the need for public health to prioritize children and take a long-term approach when responding to armed conflicts. Protecting and supporting children's mental health during these events, for example, by providing healthcare facilities and mental health services, may play a crucial role in reducing the negative impacts of war. Furthermore, our findings imply that there is a positive role that war relief funds may play in shielding children from some of these harmful consequences, suggesting that policymakers should allocate sufficient resources towards rebuilding and supporting affected communities. Recent conflicts, such as those in Ukraine, Gaza, Syria, and Sudan, highlight the urgency of these implications, as these wars have led to rising mental health issues, particularly among children. Our study emphasizes the importance of recognizing the lasting effects of conflict and taking proactive measures to mitigate its impact on the mental health of individuals and populations, both during and long after the conflict has ended.

The remainder of the paper is organized as follows. Section II provides a brief background of the

AAF bombing of German cities over the course of WWII. Section III discusses the historical city–level bombing data and individual-level survey data used in the analysis. Section IV describes the identification strategy and estimation framework. Section V presents the main results, mechanisms, and robustness analyses. Section VI concludes the paper.

2 Background on Allied Bombing of German Cities during WWII

> The Navy can lose us the war, but only the Air Force can win it. Therefore, our supreme effort must be to gain overwhelming mastery of the Air. The Fighters are our salvation, but the bombers alone provide the means of victory. (Churchill, 1940)

As Churchill indicated in the quote above, the Bomber Command's area offensive was the only offensive action in Germany between June 1940 and June 1944 (Werrell, 1986), supporting and overseeing all the different branches of the armed forces. In the Bomber Command's offensive area campaign during WWII, more than 1.25 million tons of mostly high explosive bombs were dropped over Germany (Davis, 2006). After February 1942, the majority of the AAF's air strikes were carried out at night using area bombing instead of precision bombing (Davis, 2006; USSBS, 1945). An area bombing strategy, also known as "carpet bombing" or "morale bombing", involved continuous night attacks on German cities without a particular target designed to defeat the enemy by demoralizing its citizens. During these aerial attacks, the fire was typically ignited in the center of each city with the goal of eventually completely destroying it.

Area bombing was initially premised on the inability to configure the Norden Bombsight in European weather accurately. As summarized in Gladwell (2021, p.104), the Norden Bombsight, the leading technology of the Amllied Air Force (AAF) during World War II, required clear sight of the target to achieve precision.[7] However, frequent cloud cover and overcasts over Germany made this process highly challenging. A report from Bomber Command admitted that even in the best weather conditions, 50 percent of inexperienced crews would miss the target (Gladwell, 2021). The seasons in Germany also conspired against Bomber Command. Summer, with the clearest weather, also brought shorter nights, which limited how far missions could penetrate deep into

[7] For the precision bombing with Norden Bombsight, once the target is located, information including wind direction, airspeed, temperature, the curvature of the earth had to add to the Bombsight.

Germany at night, while long winter nights hampered operations due to cold weather and overcast skies. Another major challenge in carrying out the precision bombing was the advanced early warning system in the German aircraft, which enabled them to detect the approaching attacks by the short range of the RAF's Spitfire V aircraft.[8] Consequently, striking the city centers using the navigation aids was easier and more technologically manageable, compared with the small targets aimed at precision bombings against the risk of being hit by German aircraft.

Due to the intense bombing campaign, the German cities were repeatedly attacked by the Allied Air Forces over the course of the war and experienced significant disruptions in daily life, exacerbated by the uncertainty of the aerial attacks and the destruction of homes, schools, hospitals, and other public spaces.[9] However, the intensity of the bombing varied significantly across cities, as shown in Figure 1. In fact, as the figure illustrates, the targeted cities were not necessarily chosen for their significance to the war effort, but rather for their visibility from the air, determined by weather conditions or the visibility of noteworthy landmarks such as cathedrals (Friedrich, 2002, Gladwell, 2021). Furthermore, the distance to the RAF's air bases in Mildenhall, UK, which were also used later in the war by American aircraft, significantly contributed to the bombing intensity in a given city. Thus, the bombing was concentrated in northern and western Germany – areas that were more easily accessible from the AAF bases in the UK. Taken together, given these historical accounts, it appears that the intensity of bombing in German cities was affected by both time-invariant characteristics of those cities such as size, proximity to the British air bases, and the existence of easily identifiable landmarks and random factors such as weather conditions and visible landmarks. Therefore, it is plausible to assume that the city-by-cohort variation in the intensity of the WWII bombing is essentially exogenous after controlling for the fixed city-specific characteristics.[10]

One potential concern is the displacement of children due to aerial attacks during World War II. Indeed, the German government implemented the Kinderlandverschickung (KLV) program, which relocated children from cities targeted by bombing raids to safer, rural areas within Germany and

[8]This early warning technology enabled the German aircraft to climb to higher altitudes than the AAF aircraft, and they were able to engage their bombardiers from above, taking them out of the sun (Davis, 2006).

[9]This extensive bombing campaign, for instance, led to the destruction of 91 percent of Wurzburg's built-up residential area; in Cologne, it was 90 percent; in Hamburg and Wuppertal, it was 75 percent (Diefendorf, 1993; Gladwell, 2021).

[10]We extend our estimation model by adding linear state and city trends and the interaction terms between birth year dummies and prewar city characteristics in our analysis; thus, our assumptions in these specifications extend that the bombing is exogenous to the individual's long-term mental health after accounting for a wide range of factors.

occupied territories, if transportation conditions allowed (Mouton, 2019). This program began around 1940 and continued until the end of the war in 1945, primarily involving children aged 10 to 14, who were evacuated to avoid the dangers of Allied bombings (Pine, 1997; Mouton, 2019). However, the younger children, particularly those aged five years or younger, who are the focus of our study, were not part of this evacuation effort. These younger children often remained with their parents, as evacuating them required more care and supervision, making their relocation more challenging; thereby, minimizing their likelihood of being affected by the KLV program.[11]

3 Data and Descriptive Statistics

Our analyses are performed using individual and household data from the German Socio-Economic Panel (GSOEP). As a representative survey of West Germans residing in private households, GSOEP provides detailed information on individual and household characteristics, including parental characteristics, childhood environments, and whether an individual lost their father or mother during the war years. Furthermore, the survey includes information on the city of residence since 1985, which allows us to assign the early childhood exposure to aerial bombing among the affected individuals at a finer granularity. We focus on individuals born between 1923 and 1960 in our analysis and consider those who resided in West Germany at the start of the GSOEP data in 1985.[12] This is necessitated by the fact that we have the postwar city-level data for West Germany only. Furthermore, the residents of former East Germany were subjected to a substantially different political regime until the reunification of Germany. As a result, their mental health trajectory might have been significantly different than those of West German residents. Excluding residents of former East Germany also helps with the homogeneity of our analysis sample.[13]

[11] To further address this concern, we reanalyze our data excluding older cohorts from the control group who were more likely to be impacted by this policy. As shown in the first two columns of Appendix Table A2, our results remain robust after excluding these children and only focusing on control cohorts born after WWII. We also note that the RORs we use in our study also encompass the rural areas as well.

[12] The cohorts born before 1923 were dropped from the main analyses due to the likelihood of selective mortality and small sample size.

[13] We note that the affected cohorts might have moved over time. However, in Germany, a significant proportion of individuals who move tend to resettle in a different address within the same ROR (Hochstadt, 2011). Therefore, their exposure to bombing intensity would remain unchanged. However, the internal migration information available in the GSOEP is limited and does not allow for distinguishing between moves across RORs or within the same ROR. Restricting the sample based on this measure alone would result in the exclusion of numerous valid observations, especially given the size of the RORs. Additionally, the data do not provide information on the birthplaces of individuals. Therefore, our main results are based on the full sample, acknowledging the possibility that some individuals in the sample may no longer reside in their childhood ROR. Nevertheless, we conduct a thorough examination of the potential impact of internal displacement on our findings. The results, as presented in Appendix Table A1 and discussed in detail in the subsequent section, demonstrate that our results remain robust to alternative specifications that account for various assumptions

The GSOEP measures physical and mental health quality with generic health-related quality–of–life instrument with 12–Item Short Form Survey (SF–12V2), derived from the 36–Item Short Form (SF–36) Survey Instrument.[14] These health measures have been demonstrated to be reliable and valid in clinical and population-based applications across countries (Vilagut et al., 2013; Ware et al., 1996). The GSOEP reports the mental health measures bi-annually since 2002; therefore, we use the 2002–2010 waves of GSOEP in our analysis. Our sample includes individuals whose interviews are flagged as complete and valid, meaning that the respondent completed all twelve questions required to calculate the SF–12 scales. The mental health dimension of the instrument comprised of the four items — emotional problems, vitality, social functioning, and mental health— of the Mental Health Inventory, which has been validated in tests of sensitivity and specificity relative to other screening tools for depression and other mental disorders (Ware et al., 1995). Our main variable of interest for mental health is denoted by the Mental Component Summary (MCS) and is an index ranging between 0 and 100 with a higher score indicating less dysfunction or impairment. MCS is calculated from the four subscales mentioned above using explorative factor analysis. To facilitate the interpretation of coefficients in our analysis, we normalize the MCS scores by subtracting the mean and dividing by the standard deviation.

We further supplement our main analysis by exploring different threshold levels of MCS used to diagnose mental illness. More specifically, individuals with an MCS score lower than 42 are classified as clinically depressed in the medical literature (Ware et al., 1995).[15] Incorporating these insights, we generate a binary indicator for clinical depression, which takes on the value of 1 if an individual has an MCS score of below 42, and zero otherwise. The health measurement model of SF-36 and SF-12 surveys along with the specific questions included in these instruments are described in the Appendix.

We focus on the impact of the Allied Forces' aerial attacks at the level of the smallest geographical unit publicly provided in GSOEP, called the Raumordnungsregionen (RORs or cities for

regarding internal migration.

[14] The SF–36 is a widely used, well-researched, and validated measure of physical and mental health, based on a set of generic, coherent, and easily administered questions.

[15] The literature suggests that the threshold level for clinical depression might vary by age, gender, and country of origin. For example, Vilagut et al. (2013) find that while the MCS-36 cutoff point of 42 is applicable to US norms, the MCS-12 cutoff point of 45.6 is more valid in Europe. Furthermore, Yu et al. (2015) suggest that the optimal cutoff values of MCS for Eastern populations are higher (i.e., 48–50) than those reported for Western populations (i.e., 42–45). We tested the sensitivity of our results to slightly different cut-off points in the literature and our results remained very similar.

short). RORs are similar to the metropolitan statistical areas (MSAs) in the United States. Unlike MSAs, however, RORs include both urban and rural areas, thereby providing complete coverage of Germany regardless of urban density. Former West Germany was divided into 75 RORs as shown in Figure 1.[16] Similar to Miguel and Roland (2011), our measure of bombing intensity is defined as the total number of bombs dropped per square kilometer in each city during WWII. Data on the Allied aerial attacks are obtained from Davis (2006), who provides a full account of the European Campaign of the Allied Air Forces during WWII. Data documented in Davis (2006) were compiled from the Bomber Command night raid reports, weekly operations, and intelligence reports as well as the Air Ministry War Room monthly operations summaries on the night and daylight raids. The data cover all Allied aerial attacks on Germany and other European countries and include the exact date of each attack, the targeted city and the type of the target within the city, the total number of bombs dropped, the type of the bombs dropped (i.e., high explosives, incendiary bombs, fragmentation bombs), visibility conditions during the attack, and the number of airplanes involved in each aerial attack. Following the approach suggested by Davis (2006, p.15), we aggregate the total number of bombs dropped on each city during WWII to enhance data accuracy. This aggregated figure serves as our measure of bombing intensity for each city. We then normalize the total number of bombs dropped by the area of the given city, measured in kilometer squares.[17]

We also use information from various years of the German Municipalities Statistical Yearbooks to compile the municipality-level historical data in an effort to obtain a picture of the prewar conditions as well as the conditions in the immediate aftermath of WWII. The richness of this historical data set allows us to provide insights into the mechanisms explaining the estimated long–term mental health effects of the warfare. More specifically, we collected municipality-level data on the prewar characteristics including population, city area in 1939, per capita income in 1937, and the number of mental health and children hospitals in 1938 from the 1939 German Municipalities Statistical Yearbook. Moreover, we also collected measures capturing changes in

[16] All cities in our sample were matched using the 1985 boundaries, and the historical boundaries were carefully followed to ensure accurate alignment with the current data. The matching process was cross-validated using the detailed ROR map provided by the data researchers in the GSOEP data, which was requested for this purpose. Further, our historical data is at the municipality level. Each municipality in the historical data corresponds to a unique ROR in 1985, minimizing potential inaccuracies due to post-war changes in city boundaries.

[17] Therefore, our measure of war exposure, defined as bombing intensity, is at the ROR level, and our data do not provide more granular spatial information.

various municipal characteristics that occurred during the war. These include per capita savings loss during WWII, measured as the change in per capita savings held in bank accounts; the destruction of housing stock, calculated as the change in the number of houses over the course of the war; hospital destruction, measured by the percentage change in the number of hospitals between 1937 and 1948; the loss of healthcare personnel, defined as the percentage of nurses and midwives killed or displaced during WWII; the change in infant mortality rates between 1938 and 1946; and the percentage of out-of-wedlock pregnancies during the war. Finally, we have several post-war characteristics compiled from the first post-war German Municipalities Statistical Yearbook published in 1949. These include the number of child patients in 1948 and a variable representing the size of the per capita war relief fund released in 1948. Each municipality in the historical data corresponds to a unique ROR in 1985, minimizing potential inaccuracies due to post-war changes in city boundaries. Thus, these measures are aggregated at the ROR-level, using the 1985 RORs reported in the GSOEP data and merged with the individual level data provided in the GSOEP using these ROR boundaries.

In Table 1, we present descriptive statistics on city characteristics for the full sample in column (1), and separately for cities with above and below-average exposure to bombing intensity during WWII in columns (2) and (3), respectively. Column (1) shows that on average 24,884 tons of bombs were dropped on German soil during the WWII campaign, which corresponds to about 114 tons of bombs per square kilometer. This immense bombing campaign led to the destruction of 37 percent of the housing stock by the end of the war. Furthermore, the data also show a significant degree of variation in bombing intensity across cities, where the bombs per square kilometer range from 176 tons among the most stricken cities summarized in column (2) to 63 tons among less affected cities in column (3). The summary statistics presented in Table 1 also underscore the importance of accounting for the fixed city characteristics in our estimations, because prewar population density and income per capita are larger in areas more severely hit during the AAF aerial attacks. The results of a simple cross–city analysis exploring the WWII bombing intensity across cities could yield lower bound estimates of childhood exposure to bombing if pre–war city–level incomes and population are associated with long–term mental health. We therefore instead exploit city–by–cohort variation in exposure to aerial attacks during WWII to credibly isolate the true long–term mental health effects of WWII bombing among the affected cohorts. We note, however, that ex-ante the differences in city characteristics might also lead to differential trends in mental health in the future. We test whether our results are sensitive to the inclusion of the various trends through placebo experiments as well as the inclusion of state-specific trends, city-specific trends and the interaction terms between the year of birth dummies and the pre–war city characteristics in our analysis. These exercises do not yield any evidence of differential pre-war or post-war cohort-specific trends across cities.

Table 2 summarizes the characteristics of individuals and households from the GSOEP. As illustrated in the table, around 20 percent of the sample exhibits symptoms of clinical depression according to our measure. Similarly, Table 2 depicts that on average individuals in our sample have about 11.4 years of schooling, and over 80 percent of the sample have mothers and/or fathers with basic education. Furthermore, the average age for the sample in 2002 is 59 (i.e., the first wave of the GSOEP where mental health indicators were available). About 53 percent of the sample is female, and 43 percent of the respondents live in rural areas. Approximately 9 percent of children lost their fathers during WWII, and an additional 2 percent had fathers who were prisoners of war (POW).

Figures 2a and 2b illustrate how the outcome measures change over time between the most intensely bombed RORs and those largely spared from aerial bombings. Specifically, these figures present the mean values of the MCS index and binary depression indicator at four-year intervals for different birth cohorts, comparing those who grew up in the top 20 percent of the most bombed RORs with those from the least bombed 20 percent. The figures show a clear reversal in the pattern for the treatment group cohorts across both measures. The visual evidence shown helps motivate our empirical strategy described in the next section.

4 Empirical Framework and Identification Strategy

Our approach to estimating the long–term mental health effect of childhood exposure to intense aerial bombing is to use a generalized difference–in–differences strategy following Duflo (2001) and

Hoynes et al. (2016).[18] Specifically, we exploit plausibly exogenous city–by–cohort variation in the intensity of early childhood exposure to aerial bombing, where the treatment variable is defined as an interaction between the bombing intensity per square kilometer in a given city and an indicator for being between zero and five years of age during the WWII.[19] This strategy can be formalized by the following empirical equation:

$$Y_{irtw} = \alpha + \beta(Bombing_r * WarCohort_{it}) + \delta_r + \theta_t + \mu_w + \omega' X_{irt} + \epsilon_{irtw} \quad (1)$$

where Y_{irtw} denotes mental health outcome for individual i, in city r, born in year t surveyed in wave w. These variables include a mental component summary (MCS), its sub-components, and the clinical depression indicator. $Bombing_r$ denotes the intensity of the aerial attacks in each city r measured by the total number of bombs per square kilometer.

$WarCohort_{it}$ is a dummy variable that takes on the value of 1 if an individual i was born between 1934 and 1945, and zero otherwise. Note that individuals born between 1934 and 1945 were five or younger at the onset of and during WWII; therefore they constitute our treatment group. These individuals were between ages 57 and 76 at the time their mental health was assessed. On the other hand, individuals who were older than 5 years of age at the beginning of WWII (i.e., 1923–1933 cohorts) and individuals who were born after the war ended (i.e., the 1950 and 1960 cohorts) form the control group.[20] In equation (1), δ_r represents city-specific fixed effects, accounting for time-invariant differences across cities including pre-war city characteristics; θ_t is the year of birth fixed effects, controlling for the likely secular changes in mental health across cohorts;[21] μ_w is survey wave fixed effects accounting for contemporaneous shocks to mental health as well as a potential decline in mental health due to aging between 2002 and 2010.[22] We further control for several

[18] Recent research has demonstrated that the application of the standard difference–in–differences, the estimator can produce biased results in the presence of heterogeneous treatment effects (e.g., Goodman–Bacon, 2021; de Chaisemartin and d'Haultfoeuille, 2020). However, this problem does not apply in our context since the timing of treatment does not vary over time. More specifically, as postulated in our data source, compiled by Davis (2006), we only explore the total number of bombs dropped in a given city over the entire course of the war in our analysis. Therefore, there is no staggered structure in our estimation framework.

[19] We note that our generalized difference–in–differences strategy explores within the city across cohort variation in exposure to wartime bombing; thereby, our point estimates may be lower bounds for the aggregate nation-wide effects of WWII exposure on German children's mental health later in life.

[20] We exclude the cohorts born immediately after the war (i.e., the 1946-1949 cohort) from the analysis since they were exposed to the post-reconstruction and potential immediate spillover effects of the war. However, our results are robust, both in magnitude and statistical significance, to the inclusion of these cohorts in the affected group as well as several other minor deviations in the way treatment and control groups are constructed. These results are shown in Appendix Table A2 and discussed in the next section.

[21] Since we use a repeated cross-section, θ_t accounts not only for the year of birth but also for age effects.

[22] As a robustness check, we include the linear-state trends and linear-city trends in our estimations to flexibly account for the post-war state-specific policies. We also note that healthcare services are funded and administered by state governments in Germany. Therefore,

individual and household characteristics in the vector X_{irt}, including indicators for gender and rural residence, parental education, father's occupation, and mother's age at birth.[23] The error term, denoted by ϵ_{irtw}, is assumed to be possibly correlated across individuals within the same city, and therefore the standard errors are clustered at the city level.[24] Finally, we use sample weights in our analysis to better capture the representation of the GSOEP data.

The validity of the difference–in–differences estimate hinges on the parallel trend assumption, which postulates that had the WWII bombing not occurred, the difference in mental health outcomes between the affected and control cohorts would have been the same across cities with varying intensities of the bombing. We assess this assumption by estimating an event–study version of equation (1), in which we trace out cohort–specific impacts of the aerial attacks on long–term mental health outcomes as follows:

$$Y_{irtw} = \alpha + \sum_{c=1}^{j} \beta_{ic} Bombing_r * Cohort_{ic} + \delta_r + \theta_t + \mu_w + \omega' X_{irt} + \epsilon_{irtw} \quad (2)$$

In equation (2), $Cohort_{ic}$ is a dummy variable that indicates whether individual i was born in cohort c (a cohort dummy). Birth cohorts are divided into five-year groups beginning in 1924 to improve statistical precision. Individuals born between 1956 and 1960 constitute the control group, and this cohort dummy is omitted from the regression. Each coefficient β_{ic} in Equation (2) can be interpreted as the cohort-specific estimate of the warfare on the long–term mental health of a given cohort c compared to the omitted cohort. This exercise aims to demonstrate that there are no systematic trends in mental health across cohorts and cities with different aerial bombing intensities, except for the cohorts who were five years of age and younger at the onset of WWII and those who were born during the war. This exercise also would inform us about the potential spillover effects of the bombing campaign during WWII on long-term mental health.

Table 3 presents the results from the estimation of equation (2), which enables us to trace out cohort-specific effects of the bombing intensity.[25] Each coefficient in the table represents the impact

controlling for state-level trends would help account for any state-specific factors in the post-war period that might be correlated with mental health, such as healthcare expenditures and reconstruction efforts of healthcare infrastructure.

[23] We refrain from including potentially endogenous characteristics such as years of schooling or occupation in equation (1). However, our results are robust to controlling for these variables.

[24] Our results remain robust when clustered at the state level. We also estimate our models with the standard errors described in Conley (1999) using the implementation by Colella et al. (2019) to account for possible spatial autocorrelation. Our results with the Conley standard errors remain very similar to those from the baseline specification.

[25] For ease of presentation, a graphical representation of Table 3 is provided in Figures 3a and 3b.

of bombing intensity on a different birth cohort relative to the control group, i.e., individuals born between 1956 and 1960. As shown in the table and figures, the estimates for the birth cohorts born between 1946 and 1955 are all statistically and economically indistinguishable from zero. Note that these cohorts were all born after the war had ended. Therefore, it is no surprise that the bombardment that occurred during WWII had no influence on the mental health of these cohorts any differently than the omitted cohort, also born in the post–war period. Table 3 also illustrates that the estimates for birth cohorts born between 1924 and 1933, who were ages 6–18 at the onset of WWII are insignificant. Interestingly, war destruction caused by the bombing of cities had no differential effect on the long-term mental health of these earlier cohorts relative to the omitted cohort. According to these estimates, it is really the exposure to war trauma during the intrauterine period or within the first five years of life that is linked to poor mental health experienced in the very long–term in life, with effects becoming more pronounced after the intensification of the AAF bombing campaign. These null effects in the pre– and post–war birth cohorts presented in Table 3 also indicate that our results are not confounded by pre– and post–war city–specific trends. Taken together, the results in Table 3 support our identifying assumption and suggest that the estimates from equation (1) would not be confounded by pre- and post-war city-specific cohort trends in mental health.

We perform a series of balancing tests to further assess the plausibility of our identification strategy. We first find that bombing intensity is not correlated with any of the control variables presented in X_{irt} supporting that our data is balanced. Further, as shown in Table 4, the intensity of the aerial bombing attacks experienced by our treatment cohort is not correlated with a set of parental characteristics both for all affected cohorts (as summarized in Panel A) and when we exclusively focus on cohorts born during WWII (in Panel B). These parental characteristics include the mother's age at birth, parental education, the father working in a blue-collar occupation, or whether the child's parent died during WWII and whether the father's data is missing in the survey. In addition, we investigate whether city-level prewar indicators such as pre-war city population and income per capita are associated with parental characteristics to rule out selection by parental characteristics for all affected cohorts and especially among cohorts born during WWII. Our analysis finds no significant associations with pre-war city characteristics and parental controls.

Overall, it is reassuring that bombing intensity and pre-war city characteristics are not statistically associated with various parental or individual characteristics, which further supports the validity of the difference-in-differences estimates.

Next, we test whether our results are confounded by pre-war characteristics that might be correlated with long–term mental health. Specifically, it is possible that the differences in city-level characteristics prior to the beginning of the war might have influenced the mental health trajectory of the impact of bombing intensity exposed by children ages 0-5 at the onset of the war. To test this, we explore the relationship between our mental health outcomes and city-level pre-war characteristics including the number of children and mental health hospitals in 1938, the population and city area in 1939, and income per capita in 1937. We note that these variables are presumed to be exogenous to bombing intensity after controlling for city-fixed effects. We regressed the outcome variables on each of these pre-war characteristics, controlling for gender, rural residence, and city and year of birth fixed effects. As shown in Appendix Table A3, except for one case, the estimates from this analysis are statistically insignificant. Additionally, when all variables are included together, as shown in the final column, none of the estimates are statistically significant, either individually or collectively. Moreover, the economic significance of these effects is minimal, given the very small magnitudes of the coefficients. These results are consistent with the notion that our analysis of the impact of bombing intensity exposed by the war cohort is unlikely to capture some unobserved differences across cities after controlling for city-fixed effects.

Finally, as summarized in Appendix Table A4, we test whether differential mortality, fertility, or sample attrition are of concern for our analysis. In column (1), we first investigate whether the affected cohorts experienced a higher rate of mortality relative to the other cohorts in a way correlated with the bombing intensity. In this analysis, we measure the mortality of the affected cohorts over the period of 1984-2017 using the panel structure of GSOEP. More specifically, the mortality variable in the first column of the Appendix Table A4 refers to a dummy variable, which takes the value of 1 if an individual died between 1984 and 2017, and 0 otherwise.[26] The estimate reported in the table reveals no such evidence, suggesting that exposure to different intensities of bombardment within five years of life did not cause differential mortality later in life in a

[26] We multiplied the variable by 100 to simplify the interpretation of the point estimate.

way to led to changes in sample composition at the time of mental health of affected cohort is assessed in GSOEP 2002-2010. In column (2), we generate a measure of cohort size for each birth year and city similar to the approach used by Meng and Qian (2009) to assess whether there is a significant composition change associated with exposure to bombing. The estimate shown in column (2) indicates that the size of the affected cohort in our data is not affected by bombing intensity relative to other cohorts. We also find no significant variation in cohort sizes between heavily bombed and less bombed cities. Furthermore, we test whether the attrition of the sample across the GSOEP waves is related to the bombing intensity and find no evidence to support this hypothesis.[27] More specifically, we created a dummy variable representing whether the respondent was surveyed in all waves of GSOEP, and zero otherwise. In addition, in column (4), we assess whether bombing intensity affects the likelihood of individuals leaving the sample, by using an indicator for leaving the sample before 2010 (the last year of the GSOEP we used in our analysis). The results from both of these analyses further support our earlier findings, as we continue to observe that bombing intensity does not predict sample attrition.[28]

5 Results

5.1 Main Results

Table 5 presents our baseline estimates of the impact of early life exposure to warfare on mental health in adulthood obtained from the estimation of equation (1). The estimates for the standardized mental component scale (MCS) are shown in column (1) and the estimates for the binary indicator for meeting a diagnosis of clinical depression are presented in column (2). All the regressions control for gender, an indicator for living in a rural area, city fixed effects, year of birth fixed effects, and survey year fixed effects. The parameter on interaction term, $Bombing * WarCohort$, represents the difference–in–differences estimate, which reveals the long–term mental health impact of bombing intensity experienced by those born between 1934 and 1945

[27] Our sample in the 2002 wave is 2169, then declines to 1962, 1772, 1563, and 1304 in the 2004, 2006, 2008, and 2010 waves, respectively.

[28] We also investigate whether there is attrition from our sample due to old age using survey status information documenting reasons for not being surveyed. We find that only 3 percent in our data is not surveyed due to old age; thereby, suggesting that the attrition from our sample due to old age is very uncommon. Additionally, we analyze to explore whether individuals who exit the sample differ significantly in terms of their health and education status. This analysis reveals no clear evidence that attrition is linked to these factors. In the interest of space, these results are not included in the paper but are available from the authors upon request.

above and beyond any impact experienced by those in the control cohort, i.e., individuals who were older than five years of age at the onset of WWII or those born after the war had ended.[29] The estimate in column (1) of Table 5 indicates that a one–standard deviation increase (82.7 bombs per square kilometer) in bombing intensity results in approximately 10 percent ($82.7 * 0.0012$) decline in the long–term standardized mental health score of an individual who was younger than five years of age at the onset of the WWII or born during the war, relative to someone else in the control cohort. According to the estimate in column (2), this effect translates into a 3.3 percentage point increase in the likelihood of meeting a diagnosis of clinical depression. Calculated at the mean, this is equivalent to a 16.2 percent rise in the likelihood of being clinically depressed. These results provide clear evidence to indicate that increased exposure to bombing intensity as a young child during WWII has a negative mental health effect that manifests itself later in life when an individual is between ages 57 and 76.[30]

The results shown in Table 5 are based on a continuous measure of bombardment intensity defined as the total number of bombs dropped per square kilometer in each city over the course of WWII. This measure is used to reveal a response–dose relationship, assuming that the marginal effect of bombing intensity on long–term mental health is constant across its distribution. However, it is plausible that this relationship follows a nonlinear pattern, for example, with effects getting stronger as one moves upward along the distribution of bombing intensity. To test this possibility, we estimate our empirical model specified in equation (1) using a dichotomized measure of bombing intensity. Specifically, we created binary indicators, corresponding to cities that fall into the top 10 percent, 20 percent, and 25 percent of the bombing intensity distribution. The results shown in Table 6 confirm that the negative impact of aerial attacks carried out by AAF had the most damaging effects on mental health among children in cities that had been most intensely bombed. For example, children who lived in cities in the top 10 percent of the distribution of bombing intensity (approximately 326.7 tons of bombs per square kilometer on average) during WWII experienced a 40 percent decline in their mental health score in their adult and elderly years

[29]We also estimate our models redefining the control group to include only those who were born after the war had ended or only focusing on cohorts born after 1929. As we show later in the paper, these results are similar to our main estimates.

[30]We also estimate versions of equation (1) with alternative fixed effects as well as a more comprehensive set of control variables, which includes indicators of maternal and paternal education, the occupation of father and mother's age at birth as well as the interaction of prewar city characteristics and year of birth fixed effects. As shown in Appendix Table A5, our results are robust to controlling for state-specific linear trends (columns (1) and (5)), city-specific linear trends (columns (2) and (6)), and the additional control variables (columns (3), (4) and (7) and (8)).

in life compared to children who lived in other cities during WWII. As expected, the effect size decreases monotonically as we redefine the binary bombing indicator at the 20^{th} and 25^{th} percentile of the distribution. A similar pattern emerges when we consider the binary outcome signifying the presence of clinical depression. According to the point estimates displayed in the last three columns, children who lived in cities that fell into the top 10, 20, and 25 percent of the bombing intensity distribution had a 10.2, 8.3, and 6 percentage point higher likelihood of meeting a diagnosis of clinical depression later in their lives. These results suggest that the relationship does not strictly follow a linear pattern but rather indicates a threshold effect where the most intensely bombed cities see significantly larger impacts on mental health.

In Table 7, we present results from a heterogeneity analysis in which we explore whether the level of bombing intensity exposed by our treatment cohort varies by several characteristics. As shown in columns (1) and (2), both female and male children who lived in more intensely bombed cities experience a higher likelihood of poor mental health later in life of similar magnitude, compared to children in less severely bombed cities. In columns (3) and (4), we examine heterogeneity based on urban versus rural status. To do this, we estimate our main model separately for individuals who grew up in medium cities (with over 20,000 inhabitants) and large cities (with over 100,000 inhabitants) and for those who lived in the countryside. The results show no significant effects for individuals from rural areas, while we observe substantial effects for those from medium and large cities. This is expected, as bombing campaigns primarily targeted urban locations. Columns (5)–(7) of Table 7 present the estimates obtained from samples comprised of children of mothers and fathers with less than a high–school degree and fathers with blue-collar occupations, respectively. Again, these estimates are statistically indistinguishable from our baseline estimates obtained from the analysis of the full sample of children. To the extent that parental education and the father's occupation are a proxy for economic status, this finding suggests that the risk of developing poor mental health associated with exposure to intense bombing is independent of access to economic resources. In the last column of Table 7, we present the estimates from a subsample of 776 children whose fathers had died during WWII. Given that we exclusively focus only on children who lost their fathers during WWII in this analysis, this subsample is significantly smaller than the full sample. The results indicate that these children are substantially more likely to have poor mental health later in their lives. According to the point estimates, a one–standard deviation increase in bombing intensity results in an approximately 66 percent decrease in the long–term standardized mental health score of an individual in the treatment cohort, relative to a person in the control cohort. The estimate in the bottom panel suggests that these children are also 26 percentage points more likely to meet the criteria for a clinical diagnosis of depression later in life.[31]

5.2 Mechanisms

There are likely both direct (e.g., physical and psychological trauma, displacement, income loss) and indirect mechanisms (inadequate and unsafe living conditions, environmental hazards, caregiver mental health, separation from family, displacement–related health risks, and the destruction of health, public health, education, and economic infrastructure) through which our measure of war exposure, i.e., bombing intensity, might influence long–term mental health. Destruction of physical health infrastructure, death and displacement of healthcare personnel, and loss in economic welfare and shelter may compromise access to basic necessities, such as food, health care, and education, increasing the severity and chronicity of the trauma that children endure. Consequently, even short-lived experiences of war can have harmful effects on mental health across the life course and through adulthood. Next, we examine the sensitivity of our baseline estimates to several factors that might partially account for the relationship between exposure to aerial bombings during childhood and long-term mental health. These factors include variables representing the damage to healthcare infrastructure, the loss of healthcare personnel due to warfare and displacement, the capacity burden of the healthcare system, loss in wealth via housing stock and saving loss, and the size of relief funds transferred to municipalities to be distributed to the households in need following the war.

Table 8 presents the results that provide several insights into the channels linking exposure to bombing in early childhood and later life mental health. To do this, we categorized cities in our sample by each of the proposed mechanisms, and estimate our main specification separately for the cities that fall in the top third of each indicator and the rest of the sample in separate columns

[31] Note that none of the children in the post-war control cohorts had a father who could have died during the war. Accordingly, the control group in the analysis with the sub-group of children whose fathers died during the war is limited to cohorts born before the war.

for each of the mental health outcome measures.[32] As shown in the table, the impact of bombing intensity is stronger among cities that suffered the most damage to their healthcare infrastructure captured by the destruction of hospitals. Similarly, variables that likely proxy the capacity burden of the healthcare system (i.e., the number of child patients and the increase in infant mortality) imply that the long–term mental health effect of bombing intensity is worse in cities with a more severely overburdened healthcare capacity when the war had ended.

In the next column, we investigate whether the long-term mental health effects of bombing are amplified by the percentage of out-of-wedlock births.[33] The results indicate that the effects of bombing intensity are significantly stronger in cities with a higher incidence of out-of-wedlock births. The next two columns reveal the potential role of economic wealth in moderating the relationship between bombing intensity and long–term mental health. Specifically, a steeper decline in the amount of funds in savings accounts in banks and a larger loss of housing stock during the war are both associated with a stronger negative impact of bombing intensity on the long–term mental health among the affected cohorts. In fact, the estimates are small in magnitude and insignificant among individuals from cities outside the top third housing loss.

Finally, in the last column of Table 8, we examine the role of "War Consequences Aid" as a moderator in the relationship between bombing intensity and the long-term mental health of affected cohorts. War Consequences Aid refers to the various assistance programs introduced in post-war Germany to address the severe social and economic disruptions caused by World War II.[34] These programs were aimed at promoting economic recovery, stabilizing society, and rehabilitating displaced populations. The aid, typically distributed on a per capita basis in marks, was allocated based on the extent of need in 1948, reflecting the damage experienced by individuals and communities. It primarily targeted those who had directly suffered due to the war (i.e., people who lost their homes, had family members killed or injured, or suffered significant property

[32] In principle, this analysis could be performed by including triple interaction terms among bombing intensity, treatment cohort indicator, and each of these potential channels. Instead, we adopt a split sample approach for ease of interpretation. We also acknowledge that these macro indicators are quite likely to be correlated with one another, so causal and direct inference of each indicator separately from the bombing could be challenging at times.

[33] We acknowledge that bombing was just one of many traumatic experiences during the war. To address this, we excluded Berlin and cohorts born in 1945, given the distinct effects of the Battle of Berlin and its aftermath. The results remain consistent, reinforcing that the Allied bombing campaign is the main factor driving the long-term mental health effects we estimate.

[34] We collected these data from the German Municipalities Statistical Yearbook as presented in welfare statistics collected in 1948, specifically from Table 4 on pages 162-167. We use the average per capita payments given in each municipality to capture the amount of payments per capita.

damage). A considerable portion of the aid also went to displaced persons, including refugees and those individuals forced to leave their homes during the war. In addition, individuals and families facing economic hardships, such as loss of employment, destruction of businesses, or depletion of savings due to the war, were also eligible for this assistance. Local authorities, in cooperation with federal and state governments, managed the distribution of these funds, with the amount each municipality received often determined by the extent of damage and the number of displaced persons or war victims in the area. The results shown in the last two columns of Table 8 indicate that war relief payments significantly moderated the long-term mental health consequences of aerial bombings. Specifically, the estimates from the analysis of cities in the top third for war relief payments are small in magnitude and statistically insignificant. This finding highlights the effectiveness of post-war relief efforts in mitigating the long-term mental health consequences of intense bombing, particularly in cities that received the highest levels of aid, as they experienced little to no negative effects.

Next, we further explore the extent to which our results are explained by some of the more tangible consequences of war, such as the physical destruction of healthcare infrastructure and loss of healthcare personnel, or the invisible wounds of war such as the loss of a parent, psychological trauma, disrupted relationships, and damaged social support also play a role. One way to test this is to assess the sensitivity of our treatment coefficient to controlling for a direct measure of physical destruction caused by the war. To investigate this possibility, we supplement our analysis with a measure of wartime physical destruction defined as the aggregate rubble in cubic meters per capita.[35] As shown in Table 9, our difference–in–differences estimates are remarkably robust to controlling for this variable. Moreover, the physical destruction measure is statistically insignificant for both outcome measures. This finding lends further support to the notion that the damage to the long–term mental health caused by our war exposure measure has its origins triggered by bombing intensity not necessarily captured by physical destruction.[36] Unfortunately, we do not

[35] Note that Akbulut–Yuksel (2014) and (2017) show that physical destruction had detrimental effects on the human capital formation, health, and labor market outcomes of Germans who were exposed to war in-utero or early in life.

[36] We acknowledge that this result may also reflect several important distinctions between the measures of bombing intensity and physical destruction. Bombing intensity captures the frequency and concentration of aerial attacks, which does not necessarily translate directly into physical destruction, as bombing precision often varied due to weather conditions, technology limitations, and the resilience of urban infrastructure. Additionally, post-bombing clearing efforts, variations in urban density, and discrepancies in how rubble was measured across cities might have further weakened the correlation between bombing intensity and the amount of rubble per capita. As a result, it is possible that bombing intensity and physical destruction represent different dimensions of wartime exposure, with bombing intensity capturing more of the psychological trauma and disruption that leads to long-term mental health consequences.

have direct measures of psychological trauma in our data. However, we do have a measure of whether the mother or the father died during the war. To test the role of the loss of a parent during WWII in explaining our results, we estimate our regressions controlling for the loss of a father or mother during the war years and the father being a POW. Results with these controls are summarized in Appendix Table A6. The estimate on the interaction of bombing intensity and war cohort remained robust to this exercise. In particular, the estimate on the standardized MCS measure is -0.0011 and the clinical depression indicator is 0.031, both of which are nearly identical to our main estimates from Table 5. The finding implies that the long-term mental health effect of bombing intensity is independent of whether a parent had died during the war. Therefore, the relationship is unlikely to be explained by a single factor, but rather it is likely the manifestation of psychological trauma that originates from the accumulation of a multifaceted set of factors.

We also investigate whether the estimated long-term mental health effects of early childhood exposure to bombing operate through the income effects. Our findings indicate that there is no significant difference in the effect of childhood exposure to bombing on income in adulthood between the affected and control cohorts, as shown in Appendix Table A7. This suggests that the observed impact on mental health is not mediated through income but rather likely operates through a trauma channel.

5.3 Robustness Analyses

As previously mentioned, the data available in the GSOEP does not provide specific information on whether individuals' internal migration occurred within the same regional planning region (ROR) or across different RORs. Restricting the sample based solely on respondents' reports of whether they had moved would result in the exclusion of many valid observations. Therefore, our main results are derived from the full sample, acknowledging that some individuals may no longer reside in their childhood ROR. However, it is well-documented that Germany historically had very low levels of geographic mobility, particularly in the post-WWII period. For example, Pischke and von Wachter (2008), Pischke (2007), Rainer and Siedler (2009) and Hochstadt (1999) indicate that internal migration rates among native Germans were typically low, around two to three percent per year, and it is particularly low among older households we focus on our study

(Rainer and Siedler, 2009). Further, Hochstadt (1999) has shown that short-distance moves and intra-state migration followed similar patterns, with the postwar period marking one of the most stable periods in migration rates over the past 200 years of German history (p.245). Notably, until 1980, net population changes due to migration for all cities with populations over 20,000 were nearly zero.[37]

Nevertheless, we carefully considered the possibility of internal migration which could be selective and its potential influence on our results by performing a series of robustness analyses. Our initial investigation focuses on examining the relationship between bombing intensity and the probability of internal migration. To construct an indicator for internal migration, we use the information on whether individuals still reside in the town where they were raised. If an individual no longer lives in their childhood town, the indicator takes a value of 1. We present the results of this analysis in the first column of Appendix Table A1. It is reassuring to find that the probability of internal migration is not significantly associated with bombing intensity in a given locality. In our supplementary analysis, we also assess whether the health and education status of the respondents predict their internal migration status. We find that differences in mental health or education status do not drive internal migration patterns, thereby mitigating concerns that selective migration based on these factors could significantly bias our estimates.[38]

In columns (2) and (3) of Appendix Table A1, we exclude city-states including Berlin, Bremen, and Hamburg, where individuals may be more likely to relocate. Despite the reduction in sample size resulting from this exclusion, the estimates for both mental health indicators remain virtually unchanged. Next, we focus exclusively on individuals who either still reside in their childhood town or have returned to it (columns (4) and (5)). It is worth noting that the interpretation of this question relies on the perception of respondents, which means that individuals may be classified as movers even if they relocated within the same ROR, thereby maintaining their exposure to aerial

[37] This low also rate reflects conditions after WWII as families divided by war and evacuation attempting to reunite. With postal and telephone communication destroyed, the only way that family members could achieve reunification was by returning to their home cities (Geo Epoche Panorama 2014). Before the Red Cross established a central, searchable database in Munich, individuals sought out lost relatives by posting signs and messages on house walls, train stations, parishes and community centers in their home cities (Meiners 2011; Geo Epoche Panorama 2014). Furthermore, movement between occupation zones was restricted, and individuals could not travel beyond their local areas (Allied Control Authority Germany 1946; Meiners 2011).

[38] To further explore potential bias associated with selective migration, we perform a bounding exercise, where we re-estimate our baseline specification with the probability of internal migration as the outcome of interest. The results indicate that selective migration based on these characteristics does not bias our estimates as both mental health and education controls are statistically insignificant, thereby reinforcing the robustness of our findings.

bombing unchanged. As illustrated in columns (4) and (5) of Appendix Table A1, the estimates obtained from this analysis align closely with our main results, further supporting the robustness of our findings. Additionally, we exclude individuals who indicated being raised in the countryside under the assumption that they may be more likely to have moved. These results, shown in columns (6) and (7), continue to align well with our main estimates. Finally, in the last two columns of Appendix Table A1, we perform an additional analysis in which we exclude individuals with more than a high school education from the sample, who might be more likely to move. It is reassuring that this analysis also produces estimates similar to our baseline specification.[39] Overall, the findings summarized in Appendix Table A1 provide further support for the credibility of our results, indicating that they cannot be influenced by internal migration patterns.

Our treatment cohort is composed of people who were born between 1934 and 1945 and therefore who were between ages 0-5 at the beginning of WWII or were born during the war. While all of these individuals were exposed to war within the first five years of life, the duration of exposure varies across individuals based on their year of birth. For example, someone who was born in 1939 had a full five years of exposure to war, while another person born in 1944 would only have had only one year of exposure. It is possible that the cumulative psychological trauma caused by war might increase by the duration of exposure to bombing intensity. To test this possibility, we replaced our binary war cohort indicator with a variable defined as the number of years an individual lived through the WWII bombing. As shown in Table 10, longer exposure to bombing intensity is associated with worse mental health in the long–term. For example, the estimate in the first column indicates that one standard deviation increase in the bombing intensity would result in a 3 percent (82.7 x 0.0003) decrease in mental health during adulthood among those with one year of exposure, while the effect would increase to 15 percent for those with five years of exposure. The estimates in the second column reveal a similar pattern. Specifically, a one–standard deviation increase in bombing intensity translates into an approximately one percentage point increase in the likelihood of suffering from clinical depression in adulthood if the exposure is one year, but

[39] We perform an additional robustness check by aggregating the bombing intensity data to a higher geographical level, specifically at the state level. This aggregation helps mitigate potential biases introduced by local migration patterns that could influence our analysis, but it also reduces the spatial variation in our data from 75 RORs to just 10 states. Despite this reduction, the impact of bombing intensity on both standardized mental health scores and the likelihood of clinical depression remains significant, particularly for the urban sample, as the bombing predominantly targeted urban areas.

this effect increases to five percentage points if the duration of exposure is five years.

A related question is whether the timing of exposure to bombing intensity within our treatment cohort affects our results. This is important because it addresses whether exposure during the intrauterine period versus the first years of life has a greater impact on the long-term mental health outcomes observed. Children born during WWII would have been exposed to bombings both in utero and after birth, while those born earlier experienced the war only after birth. Prior research suggests that shocks during the fetal period or early childhood can have lifelong effects on adult health, including mental health (Almond and Currie, 2011; Barker, 1990; Schlotz and Phillips, 2009). Repeatedly, there is an emerging body of evidence linking fetal growth with behavioral and mental health outcomes later in life. For example, Persson and Rossin-Slater (2018) find that a highly stressful event experienced in utero entails a more harmful effect on mental health than an event experienced shortly after birth and that the adverse mental health impacts of exposure to stress in utero are larger when the stress is more severe. Similarly, van den Broek and Fleischmann (2019) find that in the cities affected by famine caused by the Dutch Hunger Winter (1944-45), mental health was significantly better for the pre-famine and post-famine cohorts compared to the cohort born during the famine. Given this literature, a discrepancy in our results between the cohort who were in utero when WWII started and those who were born after the onset of the war may serve as suggestive evidence in favor or against the fetal origins hypothesis linking fetal shocks to long-term mental illness. To explore this question, we refined our main analysis to break down the treatment cohort into more specific age categories: those born between 1934-1936, 1937-1938, 1939-1941, and 1942-1945. As shown in Table 11, the analysis reveals that individuals born in 1937 and after, particularly those who were in utero or at very young ages during the height of the bombing campaigns, were most strongly affected by the aerial bombings. Overall, this analysis indicates that our results are largely driven by individuals who were either born during the war years or were very young (less than 2 years old) at the onset of the war. This is consistent with the notion that disruptions to development during the in-utero period or within the first few years of life can significantly contribute to an increased risk of mental health problems later in life.[40]

[40] However, our data do not allow us to definitively determine whether in-utero exposure or early childhood exposure has the most significant impact, as WWII spanned from 1939 to 1945, affecting children beyond their gestation period. Additionally, the Bomber Offensive was particularly intense between 1942 and 1945, as documented by Davis (2006, p.568), which complicates our ability to make a clear distinction.

We also note that our control cohort includes two groups of individuals. The first group comprises individuals born between 1923 and 1933 who were older than five years at the start of the war. The second group consists of individuals born between 1950 and 1960, who were not exposed to war. To further assess the reliability of our findings, we carried out additional robustness exercises by using more homogeneous control groups. We begin by estimating our main specification using only individuals born between 1950 and 1960 and therefore had no exposure to bombings in the control group. The results from the form analysis are shown in columns (1) and (2) of Appendix Table A2. Despite the reduction in sample size by approximately one-third, the estimates obtained from this subgroup are similar to those from the main sample in terms of both magnitude and statistical significance. Next, we perform the same exercise in columns (3) and (4) by exclusively focusing on individuals born between 1923 and 1933 as the control group. In the next two columns, we drop individuals born before 1929 from the control group who were older than 10 at the beginning of WWII. The results obtained from both of these analyses align well with our main estimates derived from the full sample, bolstering our confidence in the robustness of our results.

Another robustness check involves testing the validity of our results using alternative categorizations of the affected cohorts. In columns (7) and (8) of Appendix Table A2, we limit the affected cohorts to those born between 1937 and 1945 to capture individuals impacted after the intensification of the bombing campaign in 1942. In the final two columns of Appendix Table A2, we include the 1946–1949 cohort as part of the affected group. Although we exclude this cohort from the main analysis due to their exposure to post-reconstruction efforts and the potential immediate spillover effects of the war, our results remain robust under both alternative categorizations.

Further, we explore whether these two distinct control groups (those born between 1950-1960 and 1923-1933) exhibit different trends in mental outcomes by city-level bombing intensity. In the analysis presented in Appendix Table A8, we conduct a placebo experiment in which individuals born between 1950 and 1960 are incorrectly assigned to the placebo treatment group, while those born between 1923 and 1933 serve as the control group. We then estimate our baseline specification using these groups, even though neither group was between the ages of 0-5 during the war years. The results, summarized in the first row of Appendix Table A8, are both economically and statistically insignificant, suggesting that there are no discernible differences between these two control groups regarding the impact of bombing intensity. This further supports the evidence presented in the event study in Table 3.

As discussed in the Appendix, our MCS index is composed of four subscales including *Vitality*, *Social Functioning*, *Role Emotional*, and *Mental Health*. The questions used to represent these subscales are described in Figure A1. Next, we estimate our equation (1) separately for each of these four components to get a sense of which of them drives our results. As shown in Appendix Table A9, the estimates on the interaction term between bombing intensity and war cohort are negative for all of the four components. However, they are estimated with statistical significance only for the individual components of *Mental Health* and *Social Functioning*. A closer look at the questions used to form these components reveals that these two are also the survey instruments, which are most closely related to mental well-being. Specifically, the *Mental Health* component is comprised of the following two questions: "During the past four weeks have you felt calm and peaceful?" and "During the past four weeks did you have a lot of energy?"; and *Social Functioning* is created by the question: "During the past four weeks, how much of the time has your physical health or emotional problems interfered with your social activities (like visiting friends, relatives, etc)?".[41] We interpret the results in Appendix Table A9 as further support for the notion that long–term mental health effects of bombing intensity are manifested by individual responses that reflect mental well-being most closely.

6 Conclusion

Incidences of armed conflict and warfare constitute a global health problem of the highest order with significant direct and indirect consequences on mortality and morbidity. Recently, there has been a surge in the scale and scope of armed conflicts. Running parallel to this development is the rise in the number of children living in territories or countries in armed conflict or emerging from war. Accordingly, there have been concerns over the long–term psychological harm to children caused by the trauma of war and armed conflicts. Although there is mounting evidence on the

[41] The questions used to construct other two components have to do with energy level and work/accomplishments. See questions 6 and 7 for *Role Emotional* and question 10 for *Vitality*.

relationship between early-life risk factors such as exposure to war trauma and mental health and well-being, questions about causality still remain (Angelini et al., 2021). Furthermore, investigations considering a long–term perspective that extends into the late stages of life are relatively rare, possibly due to the paucity of data sources. This paper examines the long–term mental health consequences of exposure to intense bombing during early childhood, using the arguably exogenous variation in the intensity of bombardment suffered by the German cities during WWII. Our results demonstrate that children bear the invisible wounds of wars that continue to adversely affect their mental health well into late adulthood. Specifically, we document that increased bombing intensity experienced as a young child during WWII had a significant negative impact on mental health in later stages of life when these individuals are in their 50s to 70s. Our analysis shows that the early years in life, particularly the first five years of life including the intrauterine period, are especially important in terms of vulnerability to long–term mental health consequences of war.

Our analysis reveals several important mechanisms through which wartime exposure, specifically bombing intensity, influenced long-term mental health outcomes. Cities that suffered significant damage to healthcare infrastructure and experienced an overburdened healthcare system showed stronger negative mental health impacts. Additionally, the economic consequences, such as loss of wealth and housing stock, further exacerbated these effects. Interestingly, the percentage of out-of-wedlock births also amplified the mental health consequences of bombing. However, cities that received the highest levels of War Consequences Aid in 1948 experienced little to no long-term negative mental health effects, indicating the moderating power of substantial post-war relief. These findings underscore the critical importance of post-war recovery programs in mitigating the long-term mental health consequences of conflict. The significant moderating effect of War Consequences Aid, especially in the cities receiving the highest levels of assistance, demonstrates the value of robust financial and social support mechanisms in promoting psychological resilience among affected populations. Policymakers should consider the long-lasting benefits of comprehensive relief efforts, which not only address immediate needs but also help reduce the adverse mental health impacts of traumatic wartime experiences.

The results in this paper suggest that it is likely the youngest children who appear to be most vulnerable to poor mental health in the long–run. Extensive research shows that the periods of infancy and early childhood are critical periods for interventions to prevent poor outcomes in the future (Currie and Rossin-Slater, 2015; Garcia et al., 2020; Heckman and Masterov, 2007). There are well-established early intervention strategies targeted at young children that have been demonstrated to ameliorate the effect of traumatic experiences that are antecedents of later mental health problems (Izett et al., 2021; Davis et al., 2010). More recently, the mental health concerns related to war trauma have become even more pressing due to conflicts in Ukraine, Gaza, Syria, and Sudan, where millions of children have endured months of bombing, shelling, and displacement. The findings in this paper highlight the importance of scaling up services to children by governments and international organizations such as UNICEF. The benefits of these interventions are likely to be substantial because the mental health effects of early–life conditions manifest at young ages and persist throughout the life course, with their costs compounding over time (Angelini et al., 2021). In addition to the importance of prioritizing children, our results suggest that adopting a long-term perspective in public health planning and response is crucial for mitigating the adverse mental health consequences of armed conflict, particularly during the decades of recovery that follow such events.

References

Adhvaryu, A., Fenske, J., Nyshadham, A. (2019). Early life circumstances and adult mental health. Journal of Political Economy, 127(4): 1516–1549.

Akbulut–Yuksel, M. (2014). Children of war the long-run effects of large–scale physical destruction and warfare on children. Journal of Human Resources, 49(3): 634–662.

Akbulut–Yuksel, M. (2017). War during childhood: The long-run effects of warfare on health. Journal of Health Economics, 53: 117–130.

Akresh, R., Bhalotra, S., Leone, M., Osili, U.O., (2012). War and stature: Growing up during the Nigerian civil war. American Economic Review, 102(3): 273-277.

Akresh, R., Bhalotra, S., Leone, M., Osili, U.O., (2021). First and Second Generation Impacts of the Biafran War. Journal of Human Resources.

Allied Control Authority Germany, (1946). Enactments and Approved Papers. Vol.2, Jan.-Feb. 1946. The Army Library. Washington D.C.

Almond, D. (2006). Is the 1918 Influenza Pandemic Over? Long-Term Effects of In-Utero Influenza Exposure in the Post-1940 U.S. Population. Journal of Political Economy, 114(4): 672-712.

Almond, D. and J. Currie. (2011). Killing me softly: The fetal origins hypothesis. Journal of Economic Perspectives, 25(3): 153–172.

Almond, D. and B. Mazumder. (2011). Health capital and the prenatal environment: the effect of Ramadan observance during pregnancy. American Economic Journal: Applied Economics, 3(4): 56-85.

Alonso, J., Liu, Z., Evans–Lacko, S., Sadikova, E., Sampson, N., Chatterji, S., WHO World Mental Health Survey Collaborators. (2018). The treatment gap for anxiety disorders is global: Results of the world mental health surveys in 21 countries. Depression and Anxiety, 35: 195–208.

Angelini, V., Mierau, J. O., & Viluma, L. (2021). Socioeconomic Conditions in Childhood and Mental Health Later in Life. In Handbook of Labor, Human Resources and Population Economics (pp. 1-18). Cham: Springer International Publishing.

Annan, J., Blattman, C., Mazurana, D., Carlson, K. (2011). Civil war, reintegration, and gender in Northern Uganda. Journal of conflict resolution, 55(6):877–908.

Barker, D. J. (1990). The fetal and infant origins of adult disease. BMJ: British Medical Journal, 301(6761): 1111.

Blattman, C., Annan, J. (2010). The consequences of child soldiering. The Review of Economics and Statistics, 92(4): 882–898.

Bleakley, H. (2007). Disease and Development: Evidence from Hookworm Eradication in the American South. Quarterly Journal of Economics, 122(1): 73-117.

Bleakley, Hoyt. (2010). Malaria Eradication in the Americas: A Retrospective Analysis of Childhood Exposure. American Economic Journal: Applied Economics, 2(2): 1-45.

Bozzoli, Carlos, and Climent Quintana-Domeque. (2014). The Weight of the Crisis: Evidence from Newborns in Argentina. Review of Economics and Statistics, 96(3): 550-562.

Bratti, M., Mendola, M., Miranda, A. (2015). Hard to forget: The long–lasting impact of war on mental health. Centro Studi Luca d'Agliano Development Studies Working Paper, (388).

Cabral, Marika, Bokyung Kim, Maya Rossin–Slater, Molly Schnell, and Hannes Schwandt. (2021). Trauma at School: The Impacts of Shootings on Students. Human Capital and Economic Outcomes. NBER WP 28311.

Cesur, R., Sabia, J. J., Tekin, E. (2013). The psychological costs of war: Military combat and mental health. Journal of Health Economics, 32(1): 51–65.

de Chaisemartin, Clement, and Xavier d'Haultfoeuille (2020). Two way fixed effects estimators with heterogeneous treatment effects. American Economic Review, 110(9): 2964–2996.

Chrisman, A. K. and Dougherty, J. G. (2014). Mass trauma: Disasters, terrorism, and war. Child and Adolescent Psychiatric Clinics, 23(2): 257–279.

Colella , F. , Lalive, R., Sakalli, S.O. and Thoenig, M. (2019). Inference with arbitrary clustering, IZA Discussion Papers 12584, Institute of Labor Economics (IZA).

Conley , T.G. (1999). Gmm estimation with cross sectional dependence. Journal of Econometrics, 92(1): 1–45.

Conzo, P., and Salustri, F. (2019). A war is forever: The long–run effects of early exposure to World War II on trust. European Economic Review, 120, 103313.

Copeland WE, Adair CE, Smetanin P, et al. (2013). Diagnostic transitions from childhood to adolescence to early adulthood. Journal of Child Psychology Psychiatry, 5.

Corcoran, J., Brown, E., Davis, M., Pineda, M., Kadolph, J., Bell, H. (2013). Depression in older adults: a meta-synthesis. Journal of Gerontological Social Work, 56(6): 509–534.

Currie Janet. (2009). Healthy, Wealthy, and Wise: Socioeconomic Status, Poor Health in Childhood, and Human Capital Development. Journal of Economic Literature, 47(1): 87-122.

Currie, Janet and Douglas Almond. (2011). Human capital development before age five In Editor(s): David Card, Orley Ashenfelter, Handbook of Labor Economics, 4(B): 1315-1486.

Currie, J. and Rossin-Slater, M. (2015). Early-life origins of life-cycle well-being: Research and policy implications. Journal of Policy Analysis and Management, 34(1): 208-242.

Currie, Janet and Erdal Tekin. (2012). Understanding the Cycle: Childhood Maltreatment and Future Crime. Journal of Human Resources, 47(2): 509-549.

Davis, R. G. (2006). Bombing the European Axis Powers. A Historical Digest of the Combined Bomber Offensive 1939–1945.

Davis, E., Sawyer, M. G., Lo, S. K., Priest, N., & Wake, M. (2010). Socioeconomic risk factors for mental health problems in 4-5-year-old children: Australian population study. Academic Pediatrics, 10(1): 41-47.

Diefendorf, Jeffry. (1993). In the Wake of the War: The Reconstruction of German Cities After World War II. New York: Oxford University Press.

Duflo, Esther (2001). Schooling and labor market consequences of school construction in Indonesia: Evidence from an unusual policy experiment. American Economic Review, 91(4), 795-813.

Friedrich, Joerg. (2002). Der Brand: Deutschland im Bombenkrieg, 1940-1945. Munich: Propylaen Publishing.

Gade, D. M. and Wenger, J. B. (2011). Combat exposure and mental health: the long–term effects among US Vietnam and Gulf war veterans. Health Economics, 20(4): 401–416.

Garcia, J. L., Heckman, J. J., Leaf, D. E., & Prados, M. J. (2020). Quantifying the life-cycle benefits of an influential early-childhood program. Journal of Political Economy, 128(7): 2502-2541.

Geo Epoche Panorama. (2014). Truemmerzeit und Wiederaufblau: Deutschland 1945- 1955. Gruener Jahr AG Co KG, Druck- und Verlagshaus.

Gladwell, M. (2021). The Bomber Mafia: A Dream, a Temptation, and the Longest Night of the Second World War. Little, Brown and Company.

Goodman–Bacon, Andrew. (2021). Difference–in–differences with variation in treatment timing. Journal of Econometrics.

Heckman, J. J., & Masterov, D. V. (2007). The productivity argument for investing in young children. Applied Economic Perspectives and Policy, 29(3): 446-493.

Hochstadt, Steve , Mobility and Modernity: Migration in Germany 1820-1989, (Ann Arbor: University of Michigan Press, 1999).

Hoynes, Hilary, Diane Whitmore Schanzenbach, and Douglas Almond. (2016). Long Run Impacts of Childhood Access to the Safety Net. American Economic Review, 106(4): 903-934.

Huang, C., Phillips, M. R., Zhang, Y., Zhang, J., Shi, Q., Song, Z., & Martorell, R. (2013). Malnutrition in early life and adult mental health: evidence from a natural experiment. Social Science & Medicine, 97: 259-266.

Izett, E., Rooney, R., Prescott, S. L., De Palma, M., & McDevitt, M. (2021). Prevention of mental health difficulties for children aged 0-3 years: a review. Frontiers in Psychology, 2794.

Kadir, A., Shenoda, S., Goldhagen, J., Pitterman, S., Suchdev, P. S., Chan, K. J., Arnold, L. D. (2018). The effects of armed conflict on children. Pediatrics, 142(6).

Kesternich, I., Siflinger, B., Smith, J. P., Winter, J. K. (2014.) The effects of World War II on economic and health outcomes across Europe. Review of Economics and Statistics, 96(1): 103–118.

Kim, A. Y. I. (2017). Long–run Mental Health Impact of the Korean War. Seoul Journal of Economics, 30(4).

Kim, Y. I., Lee, J. (2014). The long–run impact of a traumatic experience on risk aversion. Journal of Economic Behavior Organization, 108: 174–186.

Kondeleon, Nicholas. (2022). 'Nation under stress': Doctors say mental health a top priority in Ukraine. ABC News.

Levy, B. S., Sidel, V. W. (Eds.). (2008). War and public health. Oxford University Press.

Lyk–Jensen, S. V., Weatherall, C. D., Jepsen, P. W. (2016). The effect of military deployment on mental health. Economics and Human Biology, 23: 193–208.

Mansour, Hani, and Daniel I. Rees. (2012). Armed Conflict and Birthweight: Evidence from the al-Aqsa Intifada. Journal of Development Economics, 99(1): 190-199.

Meiners, Antonia. (2011). Wir haben wieder aufgebaut: Frauen der Stunde null erzaehlen. Elisabeth Sandmann Verlag GmbH. Muenchen.

Meng, Xin, and Nancy Qian. (2009). The Long–term Consequences of Famine on Survivors: Evidence from a Unique Natural Experiment using China's Great Famine. NBER WP 14917.

de Menil, V. and A. Glassman (2015). Missed opportunities in global health: Identifying new strategies to improve mental health in LMICs. CGD Policy Paper 068. Washington DC: Center for Global Development.

Miguel, Edward and Gerard Roland. (2011). The Long Run Impact of Bombing Vietnam. Journal of Development Economics, 96(1): 1-15.

Minoiu, Camelia and Olga Shemyakina. (2014). Armed conflict, household victimization, and child health in Cote d'Ivoire, Journal of Development Economics, 108: 237-255.

Mnookin, S. (2016). Out of the shadows, Making mental health a global developmental priority. World Bank Group and World Health Organisation.

OECD (2018). Promoting mental health in Europe: Why and how?, in Health at a Glance: Europe 2018: State of Health in the EU Cycle, OECD Publishing, Paris.

OECD (2019). Mental Health.

Ostby, G., Rustad, S. A., Tollefsen, A. F. (2020). Children affected by armed conflict, 1990–2019. Oslo: Peace Research Institute Oslo.

Persson, P., and Rossin-Slater, M. (2018). Family ruptures, stress, and the mental health of the next generation. American Economic Review, 108(4-5): 1214-1252.

Pine, Lisa. 1997. Nazi Family Policy, 1933-1945. Oxford and New York: Berg Publishers.

Pladson, Kristie (2019). Germany: Depression in seniors severely overlooked. Deutsche Welle.

Ramirez, D., Haas, S. A. (2021). The Long Arm of Conflict: How Timing Shapes the Impact of Childhood Exposure to War. Demography, 58(3): 951–974.

Schlotz, W., Phillips, D. I. (2009). Fetal origins of mental health: evidence and mechanisms. Brain, Behavior, and Immunity, 23(7): 905–916.

Singhal, S. (2019). Early life shocks and mental health: The long–term effect of the war in Vietnam. Journal of Development Economics, 141: 102244.

Soni, Aparna and Erdal Tekin (In press). How do mass shootings affect community well-being? The Journal of Human Resources.

Thornicroft, G., Chatterji, S., Evans–Lacko, S., Gruber, M., Sampson, N., Aguilar–Gaxiola, S., Kessler, R. C. (2017). Undertreatment of people with major depressive disorder in 21 countries. British Journal of Psychiatry, 210: 119–124.

UNICEF., United Nations. (2009). Office of the Special Representative of the Secretary-General for Children, Armed Conflict. Machel study 10–year strategic review: Children and conflict in a changing world.

USSBS. (1945). The United States Strategic Bombing Survey: The Effects of Strategic Bombing on the German War Economy. Washington, D.C.: Overall Economic Effects Division.

Van den Broek, T., & Fleischmann, M. (2019). Prenatal famine exposure and mental health in later midlife. Aging & Mental Health, 23(2): 166-170.

Vigo D, Thornicroft G, Atun R.(2016). Estimating the true global burden of mental illness. Lancet Psychiatry, 3:171–178.

Vilagut, G., Forero, C. G., Pinto–Meza, A., Haro, J. M., De Graaf, R., Bruffaerts, R., ... ESEMeD Investigators. (2013). The mental component of the short–form 12 health survey (SF–12) as a measure of depressive disorders in the general population: results with three alternative scoring methods. Value in Health, 16(4): 564–573.

Ware JE, Jr, Kosinski M, Keller SD. (1996). A 12–Item Short–Form Health Survey: Construction of Scales and Preliminary Tests of Reliability and Validity. Medical Care, 34(3): 220–233.

Ware J, Kosinski M, Keller S. (1995). SF–12: How to score the SF–12 Physical and Mental Summary Scales. 2nd ed. Boston, MA: The Health Institute, New England Medical Center.

Ware, J., Kosinski, M., Dewey, J., & Gandek, B. (2001). How to score and interpret single-item health status measures: A manual for users of the SF-8 health survey. Boston: QualyMetric.

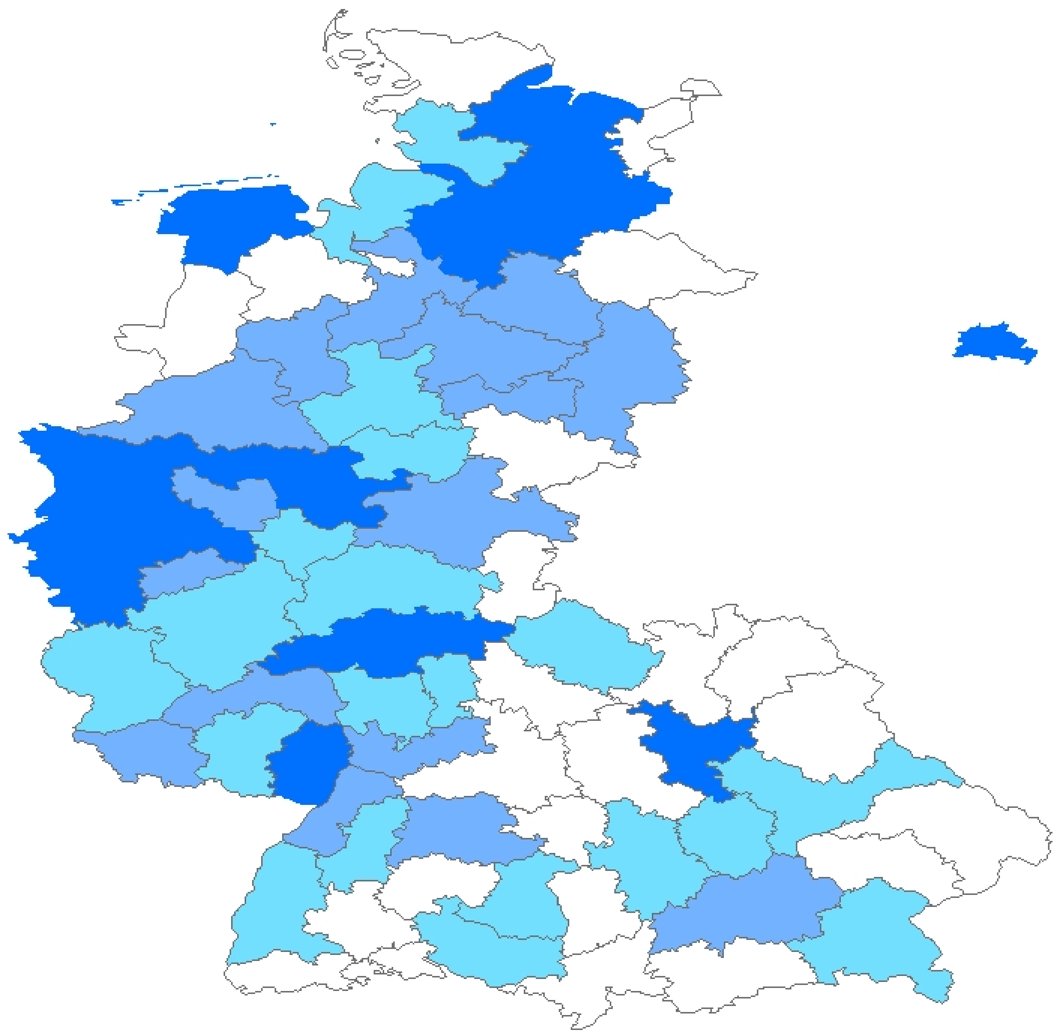
Werrell, Kenneth. (1986). The Strategic Bombing of Germany in World War II: Costs and Accomplishments. The Journal of American History, 73(3): 702-713.

WHO (2020). World Mental Health Day: An opportunity to kick–start a massive scale–up in investment in mental health.

WHO (2021). The WHO special initiative for mental health: Universal health coverage for mental health.

Yu, D.S.F., Yan, E.C.W. & Chow, C.K. (2015). Interpreting SF–12 mental component score: an investigation of its convergent validity with CESD–10. Qual Life Res, 24: 2209–2217.

Figure 1: WWII Bombing Intensity Across Raumordnungsregionen (RORs) in West Germany



Note: Map shows the bombing intensity across 75 Raumordnungsregionen (RORs) in West Germany and Berlin.

Figure 2a : Mental Component Summary (MCS) across Cohorts by Aerial Bombing

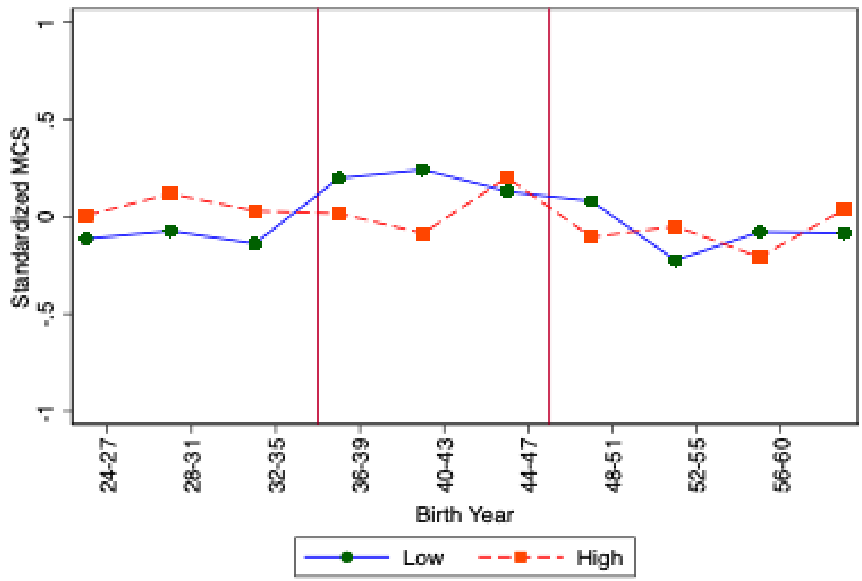


Figure 2b : Clinical Depression Indicator across Cohorts by Aerial Bombing

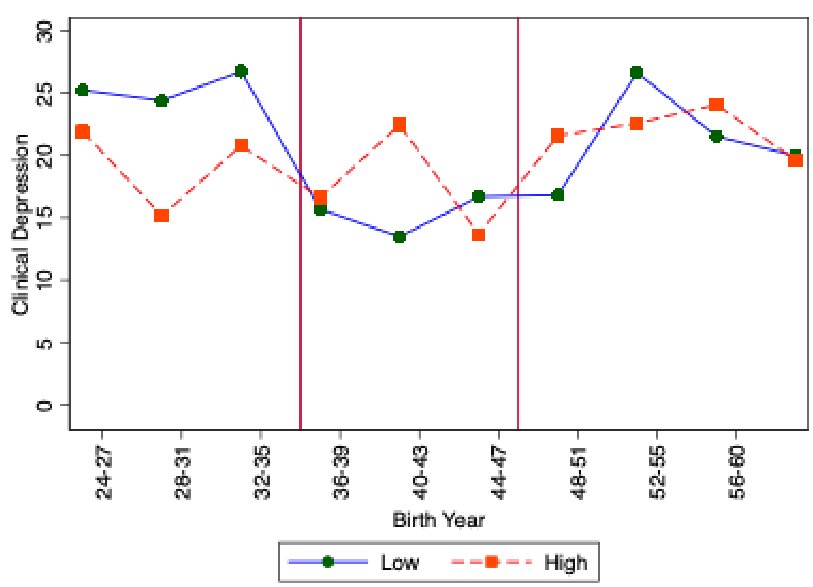
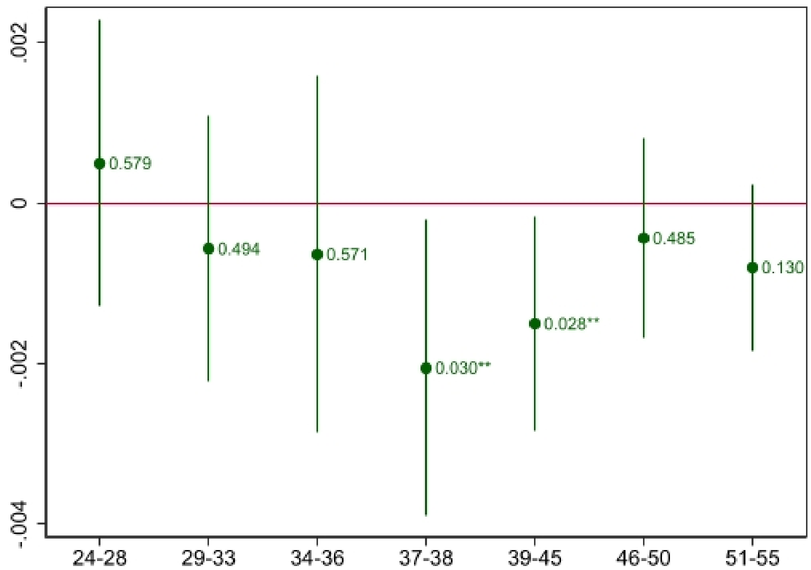
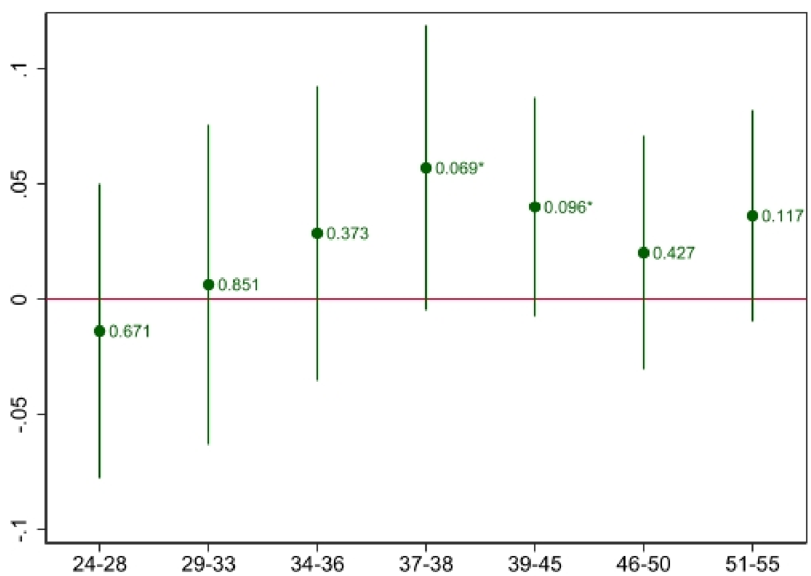


Figure 3a : WWII Bombing and Standardized MCS



Note: Bars represent 0.95 confidence intervals and points represent p-values with significance levels.

Figure 3b : WWII Bombing and Clinical Depression Indicator



Note: Bars represent 0.95 confidence intervals and points represent p-values with significance levels.

Table 1: Descriptive Statistics: City Characteristics

	All Cities (1)	Cities with Above Average Bombs (2)	Cities with Below Average Bombs (3)
Total Bombs Dropped (Tons)	24884.440	30301.690	20393.610
	(22305.700)	(20075.500)	(23054.160)
Housing Units Destroyed (%)	36.856	40.000	34.251
	(19.237)	(13.598)	(22.553)
Number of Bombs per Area	114.224	176.422	62.663
	(82.610)	(81.842)	(32.616)
Area in 1938 (km^2)	24,921.040	21,520.240	27,740.270
	(23,750.000)	(18,052.310)	(27,278.380)
Population Density in 1939	1,997.828	2,282.032	1,762.226
	(903.499)	(756.161)	(946.939)
Income per Capita in 1938 (RM)	463.375	481.424	444.957
	(105.685)	(103.564)	(104.659)
Out of Wedlock Children (%)	11.402	11.877	11.023
	(4.200)	(4.403)	(3.991)
Saving Loss per capita	222.739	299.195	136.670
	(372.855)	(497.623)	(53.003)
War Relief Payment per capita	12.408	13.067	11.865
	(4.848)	(3.648)	(5.591)
Hospital Destruction (%)	34.240	30.236	37.925
	(57.308)	(52.691)	(61.028)
Loss of Healthcare Personnel (%)	0.460	21.029	-17.538
	(54.579)	(25.621)	(65.707)
Increase in Infant Mortality Rate	4.542	4.989	4.184
	(2.234)	(2.249)	(2.157)
Postwar Number of Children Patients	213.273	257.080	174.208
	(230.161)	(301.694)	(125.787)
Prewar Number of Children Hospitals	1.578	1.477	1.669
	(2.019)	(0.979)	(2.617)
Prewar Number of Mental Hospitals	1.339	0.620	2.011
	(2.712)	(0.638)	(3.592)
N	8,770	3,975	4,795

Notes: The sample consists of 75 Regional Policy Regions (Raumordnungsregionen, ROR) in the former territory of West Germany. The means for destruction measures are weighted by population. The sample was divided into above and below-average bombing intensity. Standard deviations are in parentheses.

Table 2: Descriptive Statistics: Individual Characteristics

	All Cities (1)	Cities with Above Average Bombs (2)	Cities with Below Average Bombs (3)
Mental Component Summary (MCS)	0.000	0.007	-0.005
	(1.000)	(0.997)	(1.003)
Clinical Depression Indicator	0.203	0.199	0.206
	(0.402)	(0.399)	(0.405)
Years of Schooling	11.374	11.370	11.377
	(2.337)	(2.315)	(2.355)
Mother with Basic Education	0.875	0.880	0.871
	(0.331)	(0.325)	(0.335)
Father with Basic Education	0.830	0.827	0.832
	(0.376)	(0.378)	(0.374)
Age in 1985	41.797	42.294	41.386
	(10.817)	(10.817)	(10.800)
Female	0.532	0.534	0.530
	(0.499)	(0.499)	(0.499)
Rural	0.431	0.416	0.442
	(0.495)	(0.493)	(0.497)
Mother's Age at Birth	28.316	28.449	28.206
	(5.740)	(5.728)	(5.749)
Father died during WWII (%)	9.170	9.177	9.165
	(28.862)	(28.873)	(28.856)
Mother died during WWII (%)	1.366	1.235	1.474
	(11.609)	(11.045)	(12.054)
Father was POW	0.020	0.024	0.017
	(0.140)	(0.154)	(0.017)
Father had a blue collar job	0.409	0.410	0.407
	(0.492)	(0.492)	(0.491)
Father had a white collar job	0.126	0.118	0.133
	(0.332)	(0.322)	(0.339)
Father had a civil servant job	0.093	0.106	0.083
	(0.291)	(0.308)	(0.275)
N	8,770	3,975	4,795

Notes: Data are from the 2002-2010 GSOEP. The sample consists of individuals born between 1923 and 1960.

Table 3: Effect of WWII Bombing on Mental Health by Cohorts

	Mental Component Summary (1)	Clinical Depression Indicator (2)
Bombing*Born btw. 1924-1928	0.0005	-0.0137
	(0.0009)	(0.0321)
Bombing*Born btw. 1929-1933	-0.0006	0.0066
	(0.0008)	(0.0349)
Bombing*Born btw. 1934-1936	-0.0006	0.0288
	(0.0011)	(0.0321)
Bombing*Born btw. 1937-1938	-0.0021**	0.0572*
	(0.0009)	(0.0309)
Bombing*Born btw. 1939-1945	-0.0015**	0.0402*
	(0.0007)	(0.0239)
Bombing*Born btw. 1946-1950	-0.0004	0.0203
	(0.0006)	(0.0254)
Bombing*Born btw. 1951-1955	-0.0008	0.0364
	(0.0005)	(0.0229)
R^2	0.071	0.048
N	9874	9874
City Fixed Effects	Yes	Yes
Birth Year Fixed Effects	Yes	Yes
Survey Wave Fixed Effects	Yes	Yes

Notes: Standard errors clustered at the city level are shown in parentheses. Asterisks denote significance levels (*=.10, **=.05, ***=.01). The control group is individuals born between 1956 and 1960. Each column is from a separate regression including controls for gender and rural dummies, city and birth year fixed effects and survey year fixed effects.

Table 4: Validity Checks by Parental Characteristics and Source of Selection

	Mother's Age at Birth (1)	Parental Education (2)	Father Had a Blue Collar Job (3)	Parent Died During WWII (4)	Father's Data Missing (5)
Panel A: All War Cohorts					
Bombing*Born in 1934-1945	-0.0003	0.0003	0.0093	0.0002	-0.0001
	(0.0018)	(0.0002)	(0.0262)	(0.0002)	(0.0001)
R^2	0.073	0.120	0.097	0.131	0.085
N	8661	8770	8770	8770	8770
Panel B: Only Cohorts Born during WWII					
Bombing*Born in 1939-1945	0.0030	0.0000	0.0308	-0.0001	-0.0001
	(0.0025)	(0.0003)	(0.0310)	(0.0002)	(0.0002)
R^2	0.086	0.133	0.105	0.140	0.092
N	7275	7375	7375	7375	7375
City Fixed Effects	Yes	Yes	Yes	Yes	Yes
Birth Year Fixed Effects	Yes	Yes	Yes	Yes	Yes

Notes: War Cohort is defined as individuals who were either ages 0-5 at the onset of WWII or born during the war. The control group is individuals born between 1923 and 1960. Parental education is a dummy variable that takes a value of 1 if either the individual's mother or father has a high school degree or more. Each column is from a separate regression including controls for gender and rural dummies, city and birth year fixed effects. Standard errors clustered at the city level are shown in parentheses. Asterisks denote significance levels (*=.10, **=.05, ***=.01).

Table 5: Early Life Exposure to WWII Bombing and Mental Health in Adulthood

	Mental Component Summary (1)	Clinical Depression Indicator (2)
Bombing*War Cohort	-0.0012**	0.0331**
	(0.0005)	(0.0143)
R^2	0.086	0.062
N	8770	8770
Mean of Dependent Variable	0.001	20.319
Mean of Bombing per Area	114.396	114.224
City Fixed Effects	Yes	Yes
Birth Year Fixed Effects	Yes	Yes
Survey Wave Fixed Effects	Yes	Yes

Notes: War Cohort is defined as individuals who were either ages 0-5 at the onset of WWII or born during the war. The control group is individuals born between 1923 and 1960. Each column is from a separate regression including controls for gender and rural dummies, city and birth year fixed effects and survey year fixed effects. Standard errors clustered at the city level are shown in parentheses. Asterisks denote significance levels (*=.10, **=.05, ***=.01).

Table 6: Early Life Exposure to WWII Bombing and Mental Health in Adulthood

	Mental Component Summary			Clinical Depression Indicator		
Bombing Intensity:	Top 10%	Top 20%	Top 25%	Top 10%	Top 20%	Top 25%
	(1)	(2)	(3)	(4)	(5)	(6)
Bombing Quartile*War Cohort	-0.404***	-0.267**	-0.205*	10.206***	8.338***	6.030*
	(0.103)	(0.110)	(0.116)	(3.262)	(2.929)	(3.551)
R^2	0.087	0.086	0.086	0.062	0.062	0.062
N	8,770	8,770	8,770	8,770	8,770	8,770
City Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Birth Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Survey Wave Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: War Cohort is defined as individuals who were either ages 0-5 at the onset of WWII or born during the war. The control group is individuals born between 1923 and 1960. The sample is divided according to bombing intensity (i.e. cities that fall into the top 10 percent, 20 percent, and 25 percent of the bombing intensity distribution). Each column is from a separate regression including controls for gender and rural dummies, city and birth year fixed effects and survey year fixed effects. Standard errors clustered at the city level are shown in parentheses. Asterisks denote significance levels (*=.10, **=.05, ***=.01).

Table 7: The Heterogeneity in the Long-term Mental Health Effects of Early Life Exposure to WWII Bombing

	Female Only (1)	Male Only (2)	Medium/Large Cities Only (3)	Countryside Only (4)	Mother less than HS (5)	Father less than HS (6)	Father had BC Occ. (7)	Father Died (8)
Panel A: Mental Component Summary								
Bombing*War Cohort	-0.0014***	-0.0010*	-0.0020***	-0.0010	-0.0012**	-0.0013**	-0.0016***	-0.0078***
	(0.0005)	(0.0006)	(0.0006)	(0.0007)	(0.0005)	(0.0006)	(0.0006)	(0.0019)
R^2	0.094	0.113	0.13	0.14	0.092	0.094	0.102	0.382
N	4,664	4,106	3,171	3,776	7,333	6,860	5,166	776
Panel B: Clinical Depression Indicator								
Bombing*War Cohort	0.0326	0.0343**	0.0755***	0.0159	0.0372**	0.0416**	0.0495***	0.2601***
	(0.0210)	(0.0170)	(0.0204)	(0.0229)	(0.0164)	(0.0181)	(0.0179)	(0.0653)
R^2	0.069	0.093	0.102	0.106	0.064	0.068	0.076	0.319
N	4,664	4,106	3,171	3,776	7,333	6,860	5,166	776
City Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey Wave Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: War Cohort is defined as individuals who were either ages 0-5 at the onset of WWII or born during the war. The control group is individuals born between 1923 and 1960. Each column is from a separate regression including controls for gender and rural dummies, city and birth year fixed effects and survey year fixed effects. Standard errors clustered at the city level are shown in parentheses. Asterisks denote significance levels (*=.10, **=.05, ***=.01).

Table 8: Early Life Exposure to Warfare and Mental Health in Adulthood: Channels

	Hospital Destruction		Child Patients		Loss of Health Professionals		Out of Wedlock	
	Top	Mid & Low	Top	Mid & Low	Top	Mid & Low	Top	Mid & Low
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Mental Component Summary								
Bombing*War Cohort	-0.0019**	-0.0017	-0.0029**	-0.0009	-0.001	-0.0015	-0.0016	-0.0008*
	(0.0008)	(0.0010)	(0.0011)	(0.0006)	(0.0010)	(0.0010)	(0.0011)	(0.0005)
R^2	0.139	0.064	0.101	0.089	0.115	0.092	0.131	0.093
N	2151	4836	2588	5191	1930	4023	2613	6018
Panel B: Clinical Depression Indicator								
Bombing*War Cohort	0.0565**	0.0398	0.0745**	0.0265	0.0365	0.0312	0.0434	0.0217
	(0.0207)	(0.0298)	(0.0274)	(0.0170)	(0.0330)	(0.0306)	(0.0299)	(0.0150)
R^2	0.115	0.045	0.081	0.059	0.083	0.064	0.102	0.065
N	2151	4836	2588	5191	1930	4023	2613	6018
City Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey Wave Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: War Cohort is defined as individuals who were either ages 0-5 at the onset of WWII or born during the war. The control group is individuals born between 1923 and 1960. Each column is from a separate regression including controls for gender and rural dummies, city and birth year fixed effects, and survey year fixed effects. Standard errors clustered at the city level are shown in parentheses. Asterisks denote significance levels (*=.10, **=.05, ***=.01).

Table 8-continued: Early Life Exposure to Warfare and Mental Health in Adulthood: Channels

	Infant Mortality Increase		Saving Loss		Housing Loss		War Relief Payment	
	Top	Mid & Low	Top	Mid & Low	Top	Mid & Low	Top	Mid & Low
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Panel A: Mental Component Summary								
Bombing*War Cohort	-0.0017**	-0.0002	-0.0037***	-0.0024**	-0.0027***	-0.0008	0.0004	-0.0017**
	(0.0008)	(0.0007)	(0.0011)	(0.0009)	(0.0008)	(0.0005)	(0.0006)	(0.0007)
R^2	0.131	0.078	0.154	0.077	0.097	0.099	0.099	0.085
N	2,778	5,795	1,905	3,962	2,799	5,971	2,706	5628
Panel B: Clinical Depression Indicator								
Bombing*War Cohort	0.0397*	-0.0038	0.0827***	0.0798**	0.0817***	0.0203	-0.0209	0.0520***
	(0.0222)	(0.0230)	(0.0256)	(0.0325)	(0.0211)	(0.0151)	(0.0199)	(0.0186)
R^2	0.106	0.054	0.126	0.049	0.078	0.073	0.068	0.067
N	2,778	5,795	1,905	3,962	2,799	5,971	2,706	5,628
City Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey Wave Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: War Cohort is defined as individuals who were either ages 0-5 at the onset of WWII or born during the war. The control group is individuals born between 1923 and 1960. Each column is from a separate regression including controls for gender and rural dummies, city and birth year fixed effects, and survey year fixed effects. Standard errors clustered at the city level are shown in parentheses. Asterisks denote significance levels (*=.10, **=.05, ***=.01).

Table 9: Early Life Exposure to WWII Bombing and Mental Health in Adulthood: Controlling for Rubble per Capita

	Mental Component Summary (1)	Clinical Depression Indicator (2)
Bombing*War Cohort	-0.0012**	0.0317**
	(0.0005)	(0.0158)
Destruction*War Cohort	0.0018	-0.1101
	(0.0057)	(0.1772)
R^2	0.087	0.063
N	8,448	8,448
City Fixed Effects	Yes	Yes
Birth Year Fixed Effects	Yes	Yes
Survey Wave Fixed Effects	Yes	Yes

Notes: War Cohort is defined as individuals who were either ages 0-5 at the onset of WWII or born during the war. The control group is individuals born between 1923 and 1960. Physical destruction intensity is measured by aggregate rubble in cubic meters per capita in 1945. Each column is from a separate regression including controls for gender and rural dummies, city and birth year fixed effects and survey year fixed effects. Standard errors clustered at the city level are shown in parentheses. Asterisks denote significance levels (*=.10, **=.05, ***=.01).

Table 10: Length of Early Life Exposure to WWII Bombing and Mental Health in Adulthood

	Mental Component Summary (1)	Clinical Depression Indicator (2)
Bombing*Length of WWII Exposure	-0.0003***	0.0095***
	(0.0001)	(0.0031)
R^2	0.091	0.064
N	8,444	8,444
City Fixed Effects	Yes	Yes
Birth Year Fixed Effects	Yes	Yes
Survey Wave Fixed Effects	Yes	Yes

Notes: Length of WWII Exposure is defined as the total years an individual was affected by the WWII bombing. The control group is individuals born between 1923 and 1960. Each column is from a separate regression including controls for gender and rural dummies, city and birth year fixed effects and survey year fixed effects. Standard errors clustered at the city level are shown in parentheses. Asterisks denote significance levels (*=.10, **=.05, ***=.01).

Table 11: Early Life Exposure to WWII bombing and Mental Health in Adulthood

	Mental Component Summary (1)	Clinical Depression Indicator (2)
Bombing*Born btw. 1942-1945	-0.0011*	0.0167
	(0.0006)	(0.0224)
Bombing*Born btw. 1939-1941	-0.0013**	0.0475**
	(0.0006)	(0.0234)
Bombing*Born btw. 1937-1938	-0.0019**	0.0530*
	(0.0009)	(0.0285)
Bombing*Born btw. 1934-1936	-0.0003	0.0210
	(0.0011)	(0.0293)
R^2	0.087	0.062
N	8,770	8,770
City Fixed Effects	Yes	Yes
Birth Year Fixed Effects	Yes	Yes
Survey Wave Fixed Effects	Yes	Yes

Notes: The control group is individuals born between 1923 and 1960. Each column is from a separate regression including controls for gender and rural dummies, city and birth year fixed effects and survey year fixed effects. Standard errors clustered at the city level are shown in parentheses. Asterisks denote significance levels (*=.10, **=.05, ***=.01).

APPENDIX TABLES

Table A1: Internal Migration

		Drop City States		Non-Movers		Exclude who were Raised in Countryside		Exclude More Than High School	
	Probability Of Moving	Standardized MCS	Clinical Depression	Standardized MCS	Clinical Depression	Standardized MCS	Clinical Depression	Standardized MCS	Clinical Depression
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Bombing*War Cohort	0.0107	-0.0010**	0.0300**	-0.0013**	0.0302*	-0.0014***	0.0544***	-0.0015***	0.0396**
	(0.0220)	(0.0005)	(0.0149)	(0.0006)	(0.0181)	(0.0004)	(0.0144)	(0.0005)	(0.0168)
R^2	0.146	0.089	0.063	0.114	0.086	0.100	0.072	0.088	0.062
N	8,744	8,008	8,008	4,348	4,348	4,994	4,994	7,217	7,217
City Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey Wave Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The control group is individuals born between 1923 and 1960. Each column is from a separate regression including controls for gender and rural dummies, city and birth year fixed effects and survey year indicators. War Cohort is defined as individuals 5 and younger at the start of WWII in 1939. Standard errors clustered at the city level are shown in parentheses. Asterisks denote significance levels (*=.10, **=.05, ***=.01).

Table A2: Mental Health in Adulthood: Different Treatment and Control Groups

Treatment Group:	Original Treatment		Original Treatment		Original Treatment		Born 1937-1945		Born 1934-1949	
Control Group:	Born 1950-1960		Born 1923-1933		Drop 1923-1928		Original Control		Original Control	
	Stand. MCS (1)	Clinical Depression (2)	Stand. MCS (3)	Clinical Depression (4)	Stand. MCS (5)	Clinical Depression (6)	Stand. MCS (7)	Clinical Depression (8)	Stand. MCS (9)	Clinical Depression (10)
Bombing*War Cohort	-0.0011**	0.0254*	-0.0012*	0.0416**	-0.0011**	0.0321**	-0.0013***	0.0334**	-0.0009**	0.0250*
	(0.0005)	(0.0152)	(0.0006)	(0.0211)	(0.0005)	(0.0130)	(0.0005)	(0.0143)	(0.0004)	(0.0134)
R^2	0.093	0.064	0.113	0.086	0.084	0.061	0.093	0.064	0.077	0.052
N	6,862	6,862	5,369	5,369	7,933	7,933	8,040	8,040	9,984	9,984
City Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey Wave Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each column is from a separate regression including controls for gender and rural dummies, city and birth year fixed effects and survey year indicators. Standard errors clustered at the city level are shown in parentheses. Asterisks denote significance levels (*=.10, **=.05, ***=.01).

Table A3: Balance Test using the Prewar City Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Mental Component Summary			
Prewar Number of Children Hospitals*War Cohort	0.0053					0.0162
	(0.0190)					(0.0440)
Prewar Number of Mental Hospitals*War Cohort		0.0211**				0.0296
		(0.0102)				(0.0307)
Prewar Population per 10K*War Cohort			0.0002			-0.0031
			(0.0004)			(0.0027)
Prewar Area in hectares*War Cohort				0.018		0.0609
				(0.0163)		(0.0503)
Prewar Income per Capita*War Cohort					-0.0007	-0.0005
					(0.0005)	(0.0005)
R^2	0.000	0.000	0.094	0.094	0.083	0.085
N	6,957	7,096	8,770	8,770	6,906	6,666
			Panel B: Clinical Depression Indicator			
Prewar Number of Children Hospitals*War Cohort	-0.0411					-0.1547
	(0.5128)					(1.4003)
Prewar Number of Mental Hospitals*War Cohort		-0.5521				-1.1045
		(0.3482)				(0.9552)
Prewar Population per 10K*War Cohort			-0.0143			0.0707
			(0.0110)			(0.0840)
Prewar Area in hectares*War Cohort				-0.9159		-1.8422
				(0.4757)		(1.6031)
Prewar Income per Capita*War Cohort					0.0182	0.0121
					(0.0165)	(0.0185)
R^2	0.000	0.000	0.069	0.069	0.060	0.060
N	6,957	7,096	8,770	8,770	6,906	6,666

Notes: War Cohort is defined as individuals who were 5 and younger during WWII. The control group includes individuals born between 1923 and 1960. Each column is from a separate regression including controls for gender, rural dummies, city and birth year fixed effects, and survey year fixed effects. Standard errors clustered at the city level are shown in parentheses. Asterisks denote significance levels (*=.10, **=.05, ***=.01).

Table A4: Validity Checks for Source of Selection

	Mortality	Cohort Size	Attrition	Left the sample before 2010
	(1)	(2)	(3)	(4)
Bombing*War Cohort	0.0027	0.0059	0.0004	0.0003
	(0.0045)	(0.0043)	(0.0004)	(0.0002)
R^2	0.073	0.38	0.085	0.14
N	8,770	8,770	8,770	8,770
City Fixed Effects	Yes	Yes	Yes	Yes
Birth Year Fixed Effects	Yes	Yes	Yes	Yes

Notes: War Cohort is defined as individuals who were 5 and younger during WWII. The control group is individuals born between 1923 and 1960. Each column is from a separate regression including controls for gender and rural dummies, city and birth year fixed effects. Standard errors clustered at the city level are shown in parentheses. Asterisks denote significance levels (*=.10, **=.05, ***=.01).

Table A5: Early Life Exposure to WWII Bombing and Mental Health in Adulthood: Parental Controls

	Mental Component Summary				Clinical Depression Indicator			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bombing*War Cohort	-0.0010**	-0.0010**	-0.0014***	-0.0014***	0.0289*	0.0280*	0.0458***	0.0440***
	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0147)	(0.0151)	(0.0155)	(0.0148)
Mother's Education			0.0673	0.0793			0.301	0.301
			(0.0644)	(0.0630)			(2.6068)	(2.5773)
Father's Education			-0.0643	-0.0648			2.6368	2.6368
			(0.0768)	(0.0758)			(2.5451)	(2.5057)
Father had a blue collar job			-0.0478	-0.066			2.7276	2.7276
			(0.0624)	(0.0475)			(2.6263)	(2.0533)
Father had a white collar job			0.0794	0.0624			-3.5752	-3.2136
			(0.0740)	(0.0652)			(2.7643)	(2.4850)
Father had a civil servant job			0.0492	0.0389			-5.1058*	-4.8327**
			(0.0702)	(0.0696)			(2.5951)	(2.3426)
Mother's age at birth			0.0018	0.0024			0.0118	-0.0066
			(0.0034)	(0.0035)			(0.1340)	(0.1379)
R^2	0.097	0.123	0.082	0.093	0.070	0.089	0.061	0.067
N	8770	8770	6386	6386	8770	8770	6386	6386
City Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Linear State-trends	Yes	No	No	Yes	Yes	No	No	Yes
Linear City-trends	No	Yes	No	No	No	Yes	No	No
Prewar City Characteristics X Birth Year	No	No	Yes	Yes	No	No	Yes	Yes

Notes: War Cohort is defined as individuals who were 5 and younger during WWII. The control group is individuals born between 1923 and 1960. Each column is from a separate regression including controls for gender and rural dummies, city and birth year fixed effects and survey year fixed effects. Standard errors clustered at the city level are shown in parentheses. Asterisks denote significance levels (*=.10, **=.05, ***=.01).

Table A6: Early Life Exposure to WWII Bombing and Mental Health in Adulthood: Controlling for Parental Death

	Mental Component Summary (1)	Clinical Depression Indicator (2)
Bombing*War Cohort	-0.0011**	0.0313**
	(0.0005)	(0.0140)
Father died during WWII	-0.0005	0.0146
	(0.0006)	(0.0218)
Mother died during WWII	-0.0005	0.0044
	(0.0013)	(0.0519)
Father was POW	0.0016	-0.0667
	(0.0013)	(0.0490)
R^2	0.088	0.063
N	8,744	8,744
City Fixed Effects	Yes	Yes
Birth Year Fixed Effects	Yes	Yes
Survey Wave Fixed Effects	Yes	Yes

Notes: War Cohort is defined as individuals who were 5 and younger during WWII. The control group is individuals born between 1923 and 1960. Each column is from a separate regression including controls for gender and rural dummies city and birth year fixed effects and survey year fixed effects. Standard errors clustered at the city level are shown in parentheses. Asterisks denote significance levels (*=.10, **=.05, ***=.01).

Table A7: Early Life Exposure to WWII Bombing and Income

	Logarithm of Annual Earnings (1)	Logarithm of Monthly Earnings (2)	Logarithm of Hourly Wage (3)
Bombing*War Cohort	0.0002	0.0004	0.0005
	(0.0004)	(0.0003)	(0.0005)
R^2	0.375	0.456	0.235
N	6,507	6,216	6,450
City Fixed Effects	Yes	Yes	Yes
Birth Year Fixed Effects	Yes	Yes	Yes

Notes: War Cohort is defined as individuals who were 5 and younger during WWII. The control group is individuals born between 1923 and 1960. Each column is from a separate regression including controls for gender and rural dummies, city and birth year fixed effects. Standard errors clustered at the city level are shown in parentheses. Asterisks denote significance levels (*=.10, **=.05, ***=.01).

Table A8: Falsification Test

	Mental Component Summary (1)	Clinical Depression Indicator (2)
Bombing*Placebo War Cohort	-0.0002	-0.0074
	(0.0006)	(0.0250)
R^2	0.091	0.069
N	5,309	5,309
City Fixed Effects	Yes	Yes
Birth Year Fixed Effects	Yes	Yes
Survey Wave Fixed Effects	Yes	Yes

Notes: The "Placebo" War Cohort is individuals born between 1950 and 1960. The control group is individuals born between 1923 and 1933. Each column is from a separate regression including controls for gender and rural dummies, city and birth year fixed effects and survey year fixed effects. Standard errors clustered at the city level are shown in parentheses. Asterisks denote significance levels (*=.10, **=.05, ***=.01).

Table A9: Early Life Exposure to WWII Bombing and Mental Health in Adulthood: Sub-components

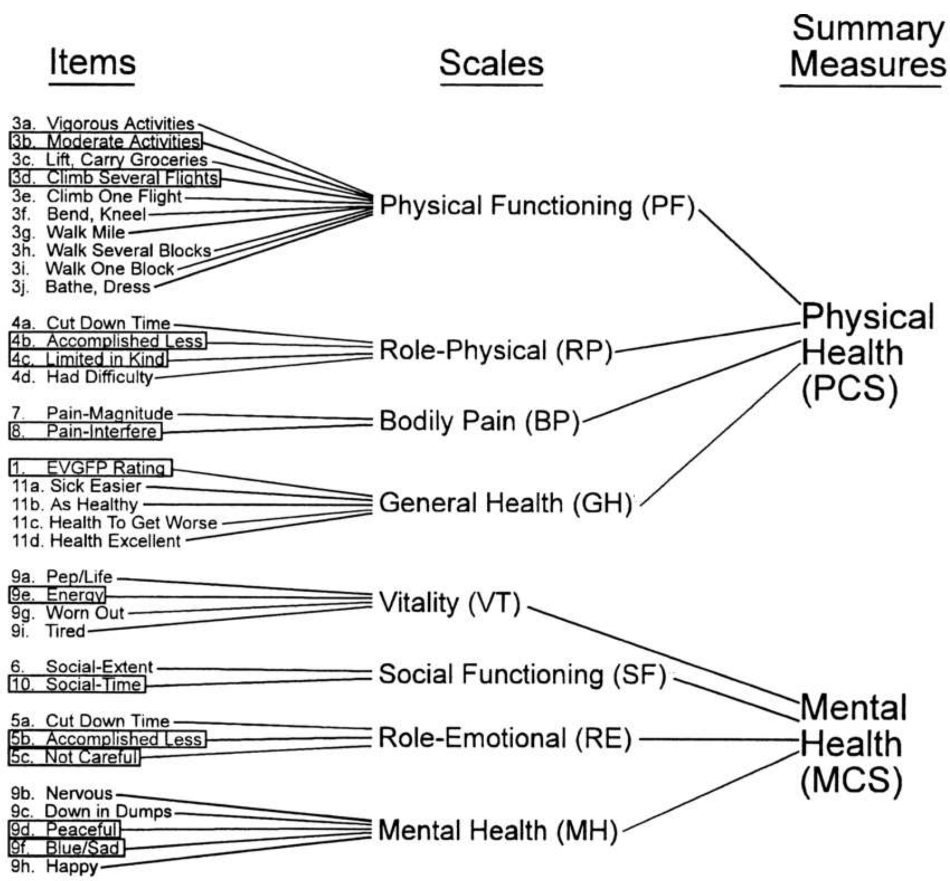
	Mental Health	Vitality	Social Functioning	Role Emotional
	(1)	(2)	(3)	(4)
Bombing*War Cohort	-0.0063	-0.0022	-0.0131**	-0.0068
	(0.0043)	(0.0038)	(0.0053)	(0.0046)
R^2	0.093	0.102	0.081	0.098
N	8,770	8,770	8,770	8,770
City Fixed Effects	Yes	Yes	Yes	Yes
Birth Year Fixed Effects	Yes	Yes	Yes	Yes
Survey Wave Fixed Effects	Yes	Yes	Yes	Yes

Notes: War Cohort is defined as individuals who were 5 and younger during WWII. The control group is individuals born between 1923 and 1960. Each column is from a separate regression including controls for gender and rural dummies, city and birth year fixed effects and survey year fixed effects. Standard errors clustered at the city level are shown in parentheses. Asterisks denote significance levels (*=.10, **=.05, ***=.01).

APPENDIX

The SF-12 questionnaire is a simplified version of the SF-36 questionnaire on health-related quality of life. (Ware et al., 2001). While SF-36 consists of 36 questions, 8 subscales, and 2 superordinate dimensions of physical and mental health, SF-12 contains only 12 of the original 36 questions, which are again grouped into 8 subscales and two final dimensions of physical and mental health. Figure A1 represents the health measurement model of SF-36 and SF-12 surveys:

Figure A1: Health Measurement Model



Notes: Items in boxes are selected for SF-12. Source: Ware, Kosinski, and Keller (1996).

The SF-12 survey contains categorical questions (yes/no), Likert response formats on a three-point scale (limited a lot, limited a little, or not limited at all), and a five-point scale (not at all, a little bit, moderately, quite a bit, and extremely). In the measuring procedure, first, all items are scored so that a high score reflects a more favorable health state between 0 and 100. Next, using these questions sub-scales consisting of one or two questions (boxed items in Figure A1) are constructed. For each subscale, a mean value is computed and transformed to a 0–100 scale (z- transformation). Sub-scales with one question are directly transformed to a 0–100 scale, and for sub-scales with two questions each, the mean value of the two items is computed (arithmetic mean). Then, these subscale scores were transformed into two 0–100 scale (physical and mental) with the higher score indicating less dysfunction or impairment.

Mental Component Summary (MCS) consists of four subscales —Vitality, Social Functioning, Role Emotional, and Mental Health as depicted in Figure A1. The questions in the SF-12 Health Survey are listed below. In constructing these four Mental Health Subscales, question 10 is used for *Vitality*, question 12 is used for *Social Functioning*, questions 6 and 7 are used for *Role Emotional*, and questions 9 and 11 are used for *Mental Health*.

SF-12 Survey Questions:

1. In general, would you say your health is? (Excellent/ Very Good/ Good/ Fair / Poor)

   The following items are about activities you might do during a typical day. Does your health now limit you in these activities? If so, how much?

2. Moderate activities, such as moving a table, pushing a vacuum cleaner, bowling, or playing golf? (Yes, limited a lot / Yes, limited a little / No, not limited at all)

3. Climbing several flights of stairs? (Yes, limited a lot / Yes, limited a little / No, not limited at all)

   During the past four weeks have you had any of the following problems with your work or other regular daily activities as a result of your physical health?

4. Accomplished less than you would like (Yes/No)

5. Didn't do work or other activities as carefully as usual (Yes/No)

During the past four weeks have you had any of the following problems with your work or other regular daily activities as a result of any emotional problems (such as feeling depressed or anxious)?

6. Accomplished less than you would like (Yes/No)
7. Didn't do work or other activities as carefully as usual (Yes/No)
8. During the past four weeks, how much did pain interfere with your normal work (including both work outside the home and housework)
9. During the past four weeks have you felt calm and peaceful? (All of the time/ Most of the time/ A good bit of the time/ Some of the time/ A little of the time/ None of the time)
10. During the past four weeks did you have a lot of energy? (All of the time/ Most of the time/ A good bit of the time/ Some of the time/ A little of the time/ None of the time)
11. During the past four weeks have you felt downhearted and blue? (All of the time/ Most of the time/ A good bit of the time/ Some of the time/ A little of the time/ None of the time)
12. During the past four weeks, how much of the time have your physical health or emotional problems interfered with your social activities (like visiting friends, relatives, etc)? (All of the time/ Most of the time/ A good bit of the time/ Some of the time/ A little of the time/ None of the time)