A war is forever:
The long-run effects of early exposure to World War II on trust*

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Abstract
This paper sheds lights on the historical roots of trust across European regions. We embrace a life-course perspective and estimate the effect of early exposure to World War II on current levels of trust among Europeans aged above 50. Our identification strategy combines the variation in place and time of conflict episodes with the variation in the respondents' month-year of birth and region of residence during the war. We focus on the pre-school period, which is a crucial stage of life for the formation of persistent trust attitudes. Our evidence provides support to this hypothesis. Individuals exposed to war episodes in the first six years of life display lower levels of trust in the adulthood. The gap persists when controlling for region and date-of-birth fixed effects, current and past socio-economic status, parental investment in human capital and other socio-demographic and economic controls, including mental and physical health. Placebo results corroborate the validity of our findings.

Keywords: Trust, World War II, Childhood experiences, Conflict, Social preferences, Europe.

JEL Classification: A13 (Relation of Economics to Social Values); D74 (Conflict; Conflict Resolution; Alliances; Revolutions); J14 (Children); J14 (Economics of the elderly); N34 (Europe 1913).

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1. Introduction

Trust in others is recognized as a key ingredient of societal success. It is a pillar of the broader concept of social capital (Coleman 1990; Putnam 1995; Uslaner 2002) and a 'lubricant' of the entire socioeconomic system (Arrow 1974), with positive effects on growth (Zak and Knack 2001; Algan and Cahuc 2010), financial development (Guiso et al. 2004), quality of institutions (La Porta et al. 1997), innovation (Gulatim and Wang 2003), and subjective well-being (Bjørnskov 2003). Trust has been also documented to be persistent in time as a result of the transmission of values from parents to children (Dohmen et al. 2012). However, while outcomes of trust have been largely examined in the economic literature, less emphasis has been placed on what makes a trusting person. For instance, the type of society and institutions individuals have been exposed to during their lives play a major role in shaping other-regarding preferences, beliefs on others' level of cooperation and social norms (e.g. Fehr and Hoff 2011; Tabellini 2010). Similarly, historical events tend to have long-run effects on two important social drivers of economic performance, i.e. trust and civicness (e.g. Guiso et al. 2016; Bigoni et al. 2015).

The aim of this paper is to deepen the enquiry into the historical roots of social preferences, with a specific focus on trust in unknown persons. To this purpose, we embrace a life-course perspective and look whether the hardships witnessed in the early childhood during the Second World War predict future levels of trust.

The reason why we look at the early childhood is that, since the seminal contribution by Erikson (1950), social psychologists have emphasized that the stable components of preferences and attitudes are formed very early in life. Under this perspective, trust becomes an integral part of ones' personality (Allport 1961; Cattell 1965; Rosenberg 1956; Uslaner 1999, 2002), which is developed through early childhood socialization and tends to change only slowly thereafter. However, in this period of life children's trust is highly sensitive to traumatic experiences such as war events, which might generate psychological

distress (Kijewski and Freitag 2016) and lead to the development of pessimistic beliefs about the trustworthiness of other persons (e.g. Bauer at al. 2017).
In addition, because trust embraces the subjective expectations about others' behavior (Gambetta 1998), the beliefs that a person refrains from causing emotional harm (Rotenberg et al. 2010), and a general faith in the self and in the world (Erikson 1963), parents may play a significant role in the formation of children's trust. In particular, stressed parents might be unable to establish a secure attachment bond with their child (Bowlby 1979; Ainsworth & Bowlby 1991) or to instil the belief that unknown others, in general, can be trusted. Anxiety caused by frequent war episodes might also increase parents' emotional instability and inconstant care-giving. For instance, food scarcity or lack of job opportunities require parental investment of time, physical and cognitive resources in coping strategies, which are implemented often outside the household. This idea finds empirical support in the psychological observations made at the residential war nurseries in London during the Second World War (WW2), which suggest that quality of care-giving and parental reactions to bombing events indeed served as a buffer against traumatization of pre-school children (Burlingham and Freud 1942).

Against this backdrop, if quality of care-giving and personal exposure to war episodes during the childhood shape long-run trust, early-exposure to war might have persistent effects on infants' preferences. We test this hypothesis by investigating the impact of early-life exposure to WW2 on levels of trust at adult age. We use the Survey on Health, Ageing, and Retirement in Europe (SHARE), which contains retrospective data of Europeans aged above 50 (e.g. childhood characteristics and life histories) as well as measures of generalized trust. This data are merged with an original dataset we have generated containing the time and location of WW2 conflict events occurred in most European regions. Our identification strategy therefore hinges on two sources of variation, i.e. the variation in the period and the place of conflicts, and the plausibly exogenous variation in the respondents' month-year and region of birth at the time of the war. This strategy provides us with an objective measure of war that varies almost at the individual level and that is not subject to endogenous

misreporting, which is often an issue when respondents are asked to recall adverse childhood experiences (Child and Nikolova 2016; Hardt and Rutter 2004).

This study builds on the economic literature that investigates the persistent effects of WW2 on health and socio-economic status (Ichino and Winter-Ebmer 2004; Kesternich et al. 2014 and 2015; Akbulut-Yuksel 2014; Havari and Peracchi 2016). Our original contribution stems from the fact that we focus on what is considered by the developmental psychologists as a critical age for the formation of values (e.g. pre-school years). By considering a broad set of European regions, we also provide an historical rationale for the observed differences in trust within and between European countries. In addition, by exploiting between- and within-region variation in exposure to war we are able to net out region fixed effects. This is not just a technical issue since accounting for region-specific characteristics mitigates the confounding effects that stable societal characteristics (e.g. culture and institutions) may have on the individual's trust.

Our results show a negative effect of early exposure to WW2 on trust as measured in the adulthood. The effect is significant both at the intensive and extensive margin (i.e. months of exposure), and robust to alternative specifications. The effect of WW2 exposure persists when replacing the attitudinal measure of trust with a behavioural measure of other-regarding preferences (e.g. volunteer work), when using a subjective measure of victimization, when controlling for the GDP level and the share of victims at the country level during WW2, and when accounting for migration as a war-coping strategy. Placebo tests on different cohorts show that the effect of the war on trust is significant only for respondents born during WW2, thereby confirming the validity of our results. We also show that endogenous selection on mortality, fertility and migration are not the key drivers of our results.

Finally, our findings show that the trust gap between exposed *vis-à-vis* non-exposed respondents does not narrow when we control for childhood

characteristics such as parental investment in human capital (i.e. health and cognitive endowments), socio-economic status (SES), and the absence of a parent. The war effect is robust also to the inclusion of current differences in socio-economic characteristics, health status and mental well-being.

Because neither self-reported victimization nor the childhood/adulthood environment contribute to narrow the systematic differences in trust due to war exposure, a candidate explanation of our results hinges on the insecure-ambivalent attachment bond developed between the infant and the caregiver during the war. This type of attachment can result from the parental stress perceived by children when a war event occurred. This interpretation is supported by the insignificant placebo results for respondents born outside the WW2 time window (i.e. before 1939 and after 1945). Because the trust levels of respondents born in a war region after WW2 do not seem to be affected by the conflict, the war effect is not likely to be driven by a change in the trust attitudes of their parents. Hence the pathway from WW2 exposure to trust has to be searched for in the type of the parent-infant relationship developed during the years of the war.

The remainder of this paper is structured as follows. In Section 2, we discuss the background literature, while in Section 3 we introduce the data. In the fourth section we present descriptive findings and in Section 5 discuss our basic econometric results. Section 6 describes placebo tests and additional robustness checks, while in Section 7 we discuss the potential selection issues. The eighth section offers our conclusions.

2. Background literature

The relationships between exposure to conflict and social preferences have been explored recently by the economic literature through a variety of empirical strategies and in different countries. Results, however, often appear inconsistent. For instance, Becchetti et al. (2014) and Cassar et al. (2013) document negative effects of exposure to violence on social preferences,

respectively in Kenya and Tajikistan. Similarly, Kijewski and Freitag (2016) show a negative effect of the civil war in Kosovo on social trust, highlighting the role of war-related distress on beliefs about the others' trustworthiness. Positive effects, instead, are showed by Bellows and Miguel (2009) and Voors et al. (2012) with respect to the violence experienced during the civil war in Sierra Leone and Burundi. A meta-study conducted by Bauer at al. (2016) suggests that exposure to violence harnesses civic engagement after a conflict, but trust and altruism increase only among individuals sharing the same identity (a phenomenon called 'parochialism'). Since most of these studies emphasize the long-lasting feature of a war, childhood – a crucial period for the development of social motivation (Bauer et al. 2014 and 2017; Eisenberg et al. 2006) – is therefore a fertile ground for understanding if and how life trajectories are modified by conflict episodes.

The importance of childhood is further highlighted by developmental psychologists, who suggest that the quality of the relationship with parents is one of the most relevant factors affecting value development in pre-school children (e.g. Ainsworth and Bowlby 1991). Caregivers appear for young children as key interfaces between the self and the others. Not only parents are the primary source of information, judgement, and filter on the external world, but also they provide children with role models and emotional stability. Furthermore, the Erikson's seminal contribution emphasizes that trust or mistrust depends on the type of caregiving received during the childhood (Erikson 1959). In particular, trust emerges when infants experience responsive caregiving, while harsh treatments or tardive responsiveness could instead stimulate mistrust (Crain 2005; Erikson 1950). Importantly, the trust formed in the early childhood through the interaction with caregivers has been showed to predict also social functioning in the adulthood (Waters et al. 1995). However, children in pre-school age appear particular vulnerable to the effects of wartime violence and destruction (Arroyo and Eth 1996; Pynoos and Nader 1993), which creates in them a sense confusion and self-blame. These reactions are sometimes amplified by the surrounding adults, especially if they are perceived by children as stressed and overextended because of the traumatic events. More specifically

to the WW2, Burlingham and Freud (1942) provide clinical evidence that children under five years are little affected by bombing, provided they were not injured, they were in their mother's care and the mother showed no signs of panic.

All these studies motivate our analysis since they emphasize that the traumatic events witnessed directly (through personal victimization) or indirectly (through parents' reactions to war events) during the initial years of life might produce significant effects on trust thereafter.

From a methodological point of view, our paper is closely related to the studies providing empirical evidence on the negative effect of WW2-exposure on health outcomes (Kesternich et al. 2014; Havari and Peracchi 2016). However, such studies rely on an objective victimization measure capturing a broad effect of societal/cohort exposure to war. In addition, they consider also Europeans born after the war, and implicitly assume that for these individuals living in a region of conflict during the WW2 has the same effects as living in a non-conflict region. This might not be a plausible assumption in our case because the average levels of trust and trustworthiness in a war area may influence the beliefs of individuals growing up in that area also after the end of the war. Finally, since Kesternich et al. (2014)'s war exposure varies at regional level, unobserved regional characteristics could lead to a spurious correlation between victimization and later outcomes.[1] Our approach instead allows to net out region fixed effects because - by exploiting variation in place-period of birth and place-period of conflict - our measure of war exposure varies almost at the individual level. This is a substantial improvement since trust levels are very

[1] The authors control for country fixed effects. However, while indeed helpful, this is not sufficient to mitigate unobserved heterogeneity. First, country boarders changed frequently during WW2 and it is therefore difficult to identify country characteristics during the years of the war. Moreover, it is not clear whether they control for the country of residence during WW2 or at the time of the interview. Second, individuals in conflict regions might have moved to non-conflict areas during and after the war. It is therefore difficult to assign to each individual a unique country dummy during and after the years of the war. Third, trust varies substantially within-countries (and so do other socio-economic characteristics), and country fixed-effects do not account for this source of heterogeneity.

heterogeneous across EU regions (Tabellini 2010), though stable in time (Volken 2002).

Closely related to our study, Ichino and Winter-Ebmer (2004) show that the individuals living in Germany and Austria who were ten years old during WW2 turn out to be less educated than the same cohort living in Switzerland and Sweden (non-war countries). With respect to social preferences, Hörl et al. (2016) show that the hunger suffered by the young German cohorts born after WW2 in response to calorie restrictions policies had detrimental effect on trust levels in the adulthood. We build on these results by including individuals living in a large set of EU regions and experiencing conflicts during their childhood in different time periods. We compare not only individuals exposed to war episodes occurring in different periods and regions, but also individuals who might have grown up in the same region, though differentially exposed to war depending on their month of birth. A similar approach is followed by Bundervoet et al. (2009) to assess if exposure to violence during the civil war in Burundi affected childhood health. In the same spirit, but limited to Germany, Akbulut-Yuksel (2014) exploit the exogenous region-by-cohort variation in the intensity of WW2 to assess the long-term consequences of the war on human capital and labour market outcomes. However, despite of the recognized importance of trust for societal success, none of these studies look at trust as a long-term outcome potentially affected by an adverse early-life environment.

3. Data

We use three sources of data. The first is the Survey on Health, Ageing, and Retirement in Europe (SHARE), which is a rich and multidisciplinary database that collects socio-demographic and health information of Europeans aged above 50. More specifically, we use wave two and five, which include a specific question on generalized trust as well as a rich set of socio-demographic characteristics. We merge these waves with the retrospective data about life events contained in wave three (SHARELIFE). Our measure of trust is the answer on an eleven point-scale to the question 'Generally speaking, would you say that

most people can be trusted or that you can't be too careful in dealing with people? (0 = you can't be too careful; 10 = most people can be trusted)'.

Regarding the retrospective data, SHARELIFE focuses on past life events of respondents including the regions where they lived and the characteristics of their childhood, which provide us with a measure of their socio-economic status (SES) during the childhood (Havari and Peracchi 2016). Through these data we can identify the regions where individuals lived since they were born and the year in which they move (if they did). In order to mitigate potential bias due to selective migration, we restrict the sample to individuals, born during the WW2, who never moved to other regions. However, in a robustness check we relax this restriction and consider also people who migrated during the war period.[2] The reliability of the retrospective data contained in SHARELIFE has been proved by Havari and Mazzonna (2015), who document the internal and external consistency of self-reported measures of childhood health and SES.

The second source of data hinges on the original and detailed description of combat events during WW2, including battles, attacks, bombings, invasions, and occupations by Ellis (1994), Davies (2006) and Collier et al. (2004). The dataset we have generated from these sources includes major bombing and minor attacks at *regional* level and *in each month* between September 1939 and September 1945.[3]

Finally, we combine information on the month-year of birth, the region where respondents lived during the war, and the war events occurred in each region to create a measure of exposure (*War*) varying nearly at the individual level. This variable captures the number of months with conflict episodes that each respondent has experienced in the period Sept. 1939 - Sept. 1945. Figure 1 shows an example for three hypothetical individuals (A, B, and C) that grew up in the same region during the period Sept. 1939 - Sept. 1945, but have different values of the *War* variable because they did not witnessed the same events.

[2] Geographical information are collected at NUTS2-level and report the month-year of moving.
[3] We consider the beginning of the war on September 1939, when Germany occupied Poland, and the ending of the war on September 1945, when the formal Japanese surrender was signed.

Individual A experienced two war events and therefore her *War* variable takes value of two; individual B experienced one war event since (s)he was born after the first war episode; therefore his/her *War* variable will be equal to one. Hence for the individual C *War* is equal to zero since (s)he did not experienced any war event. Hence respondents who grew up in the same region but were born in different periods turn out to have a different length of exposure to WW2.[4]

Our dataset includes Austria, Germany, Sweden, The Netherlands, Italy, Spain [5], France, Denmark, Greece, Switzerland, Belgium, Czech Republic, and Poland. The analysis of the WW2 by months of conflicts allows us to isolate the effect of the WW2 from that of other wars that ended before September 1945, e.g. the Spanish Civil War and the German occupation of Czech Republic.

4. Descriptive evidence

Table 1 shows descriptive statistics of the main variables used in the econometric analysis. Respondents' trust takes on average the value of 5.8 and 39 percent of the sample was exposed to at least one conflict episode during WW2 (variable *War*). The sample is almost perfectly balanced in terms of gender (54 percent are women), with the majority of individuals being married (74 percent), retired (73 percent) and with primary education (29 percent).

In order to measure respondents' socio-economic status during the childhood, we extract the first factor from a principal component analysis aimed at capturing latent family traits at the age of ten. Similar to Havari and Peracchi (2016), the principal component analysis includes the number of rooms per capita, the number of books at home and the main occupation of the

[4] Note that if a war event occurred in region *x* in a specific day, e.g. May 1st 1940, we consider one more month of exposure for all respondents born in May 1940 and before, provided that they lived in region *x* in 1940. Moreover, if multiple events occurred in the same month in a region, we consider them as a single episode of war, and therefore as *one* month of war. SHARE data does not distribute residential information at a lower level than the region. For this reason, we cannot attribute WW2-episodes occurring in a city to respondents who were living in that city at the time of the war. However, if a war episode occurred miles away from a respondent's house produces a significant effect on his/her trust, our estimate can be considered as a lower-bound of the true effect.

[5] In an additional robustness check we excluded Spain. Results are robust and available upon request.

breadwinner. In Figure 2a we compare the distribution of SES in childhood by war exposure. The two distributions almost perfectly overlap, highlighting that on average there are no significant differences in SES at the age of ten between respondents exposed vs. non-exposed to war episodes. By comparing average SES in childhood by war exposure and semester of birth, we notice that the main difference in SES is among respondents who were older during the war, i.e. those born in 1939 (Figure 2b). The average difference in childhood SES between exposed and non-exposed starts decreasing from 1940 onwards and, apart from the last quarter of 1940, the SES trend for the two groups looks similar.

Generalized trust is on average significantly higher for respondents not exposed to conflict (Figure 3a). Comparing average levels of trust over semesters of birth, non-exposed respondents systematically report higher trust than exposed respondents, while – as expected – the trend tends to convergence for those born towards the end of the war (Figure 3b).[6] The maps in Figure 4a and 4b report respectively the geographical distribution of war episodes and the fraction of respondents who experienced at least one month of conflict during the childhood. Both maps highlight a substantial within-country variability in conflict magnitude and respondents' degree of exposure.

This preliminary evidence suggests that individuals who did not experience war events early in life have higher trust levels in the adulthood than those who witnessed at least one war episode. This difference does not seem to be driven by differences in SES during the childhood.

5. Econometric analysis

By exploiting variation in i) month-year of war episodes, ii) region where the latter occurred, and iii) month-year of birth of respondents during WW2, we identify the effect of being exposed to war episodes in early childhood (0-6 years) on later levels of trust. Instead of considering cohorts as in Bundervoet et

[6] This result is consistent with the placebo tests implemented on the sample of respondents born after the WW2 (see section 6).

al. (2009) and Akbulut-Yuksel (2014), we compare individuals born in different months during WW2, and in different regions where conflicts occurred. Hence the length of exposure (months of war) depends both on the timing and location of each war episode as well as on the (plausibly exogenous) respondent's month-year of birth.

Our estimating equation writes:

$$\text{Trust}_{ijt} = \alpha_j + \gamma_t + \beta_1 \text{War}_{jt} + \sum_k \beta_k X_{k,ijt} + \varepsilon_{ijt} \quad \text{(eq. 1)}$$

where $Trust_{ijt}$ is the value of generalized trust of individual *i,* born in period *t* and living in region *j*; α_j and γ_t capture respectively region and period fixed effects. War_{jt} is a (0/1) dummy for individuals who experienced at least one episode of conflict. More specifically, this variable is equal to one if the respondent was born in a region where a conflict occurred and at least one month before the episode. Because some regions witnessed frequent conflict events during WW2, we measure war exposure also at the intensive margin by computing the number of months of war a respondent has been exposed to in the childhood. To this purpose, we create a categorical variable taking value zero for no exposure, one for one to three months of exposure and two for more than three months of exposure. This variable captures the plausibly exogenous variation in length of WW2-exposure induced by different birth periods also in case of individuals living in a region exposed to frequent conflicts. All model specifications include a (0/1) dummy for women (*Female*) and for wave of the respondent's answers. In alternative specifications we also control for a set of *k* socio-demographic variables ($X_{k,ijt}$) including education level, income percentile, marital status, job status, health status,[7] and memory performance (*Memory*).[8]

[7] Because SHARE contains several measures of individuals' health, we include in our analysis i) the number of chronic diseases (*n_chronic_diseases*) reported by the respondent and ii) the first extracted component (*health_functionalities*) from a factor analysis on a set of the indices capturing mobility functionalities, i.e. *adla* (sum of the scores for five tasks, i.e. dressing, bathing or showering, eating, cutting up food, walking across a room and getting in or out of bed), *iadla* (sum of scores for telephone calls, taking medications and managing money), *mobility_index* (sum of scores for walking 100 meters, walking across a room, climbing several flights of stairs

Baseline results in Table 2 report OLS estimates of eq. 1. They suggest that exposure to war during the childhood has negative effects on trust in the adulthood. This effect is significant both at the extensive margin (column 1) and the intensive margin, though in the latter case only for longer exposure (column 2). Since the effect of exposure might be mediated by other factors (e.g. education, income, and health), we control for socio-demographic and economic characteristics measured at the time of the interview (columns 3 and 4). Results are robust, especially at the extensive margin (column 3), suggesting that the negative effect war exposure during pre-school years is not mainly driven by observable (and potentially unobservable) characteristics correlated with health, education and income in the adulthood. Controlling for memory performance allows us also to mitigate measurement-error bias, which might be non-negligible when retrieving past information from aged respondents.

Finally we check whether the trust gap between the exposed and the non-exposed narrows when controlling for events or characteristics that are specific to the respondents' childhood. We first test the mediating role of SES in childhood, which might also proxy for the parental investment in human capital (variable *SES in childhood*). Since trust preferences tend to be high when personal or family SES is high (Gächter et al. 2004; Hörl et al. 2016), the omission of SES in the childhood could lead to an upward bias in the estimated effect of war-exposure. Results are reported in Tables 3a-b and show that the significance and the magnitude of the war effect do not change remarkably (columns 1-2 in Tables 3a-b), while the effect of SES in the childhood is positive and significant as expected.[9]

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and climbing one flight of stairs), *large_muscle* (sum of scores for sitting two hours, getting up from chair, stooping, kneeling, crouching, and pulling or pushing large objects), *gross_motor_skills* (the sum of scores for walking 100 meters, walking across a room, climbing one flight of stairs, and bathing or showering); the higher *health_functionalities*, the poorer the mobility performance.

[8] *Memory* is the sum of scores from two recalling tasks and contains the number of words recalled in the first trial of (and in a delayed) word recall task.

[9] In order to test for the presence of heterogeneous effects of WW2 by childhood characteristics, we added in an alternative specification the interaction between the war indicator and childhood SES. The coefficient of childhood SES and that of the war indicator remain significant, whereas the coefficient of the interaction term is positive but not statistically significant (results are

Recent papers have shown that exposure to war during the childhood can lead to worse health outcomes later in life, because of, for instance, limited availability of food (Kesternich et al. 2014 and 2015; Havari and Peracchi 2016). Similarly, calorie restrictions for Germans cohorts born after WW2 appear to lead to less trust in the adulthood, possibly because the increased competition over scarce resources crowds out mutual trust (Hörl et al. 2016). In addition, growing up without a parent might be both a consequence of WW2 and a possible determinant of trust since it may affect the emotional and cognitive development of the child (Tamis-LeMonda et al. 2004). In order to account for all these factors, we control for the respondents' health status and the presence of a parent during the childhood, acknowledging their possible endogeneity. We therefore include in the main regression the respondent's current number of chronic diseases, the presence of a parent at the age of ten (*mother at age 10*; *father at age 10*), any hunger episode occurred during the war (*hunger episode*), self-assessed health status when the respondent was a child (*health status in childhood*), residence in a rural area during the childhood (*rural area when child*) and any vaccination received at early age (*vaccinated when child*). Results are robust also to the inclusion of all these controls (columns 3 and 4 in Tables 3a-b). As expected, those who grew up with the mother show higher trust than those who report to have not lived with their mother at the age of ten. This result underlines the importance of responsive childcare for the development of values at the early stages of life, even though the variable may be an imperfect measure of parental support in the childhood.[10] Moreover, the mother effect disappears when standard errors are clustered by country and birth period, suggesting that the loss of a parent could be related to the differential intensity of the war over time and place, or because the national policies aimed at protecting citizens against the war were not similarly effective across countries and years. With respect to health, we find evidence of a negative impact of

available upon request). This result suggests that high childhood SES at the age of ten does not counterbalance the adverse effect of WW2 exposure on trust.

[10] Unfortunately, respondents are asked about the presence of family members in the house only at age of ten, and therefore we do not know whether they lived with their mother or with their father before or after that age.

current and past health status on trust. Nevertheless the effect of WW2 exposure remains significant and robust in magnitude.

Summarising, early exposure to WW2 produced a persistent effect on trust preferences, which does not seem to be driven by parental investment in human capital nor by the respondent's background characteristics.

6. Placebo and robustness tests

To exclude that our results are driven by noise in the war measure or in self-reported trust and by an arbitrary sample selection, we re-estimate the models in column 3 of Tables 3a-b on respondents born in different periods, namely in six-years time windows before 1939 and after 1945. During these periods there were no actual WW2 events and therefore we rely on the perfect independence between WW2 exposure and trust in the new samples. In addition, if early childhood is a crucial period for the formation of trust attitudes, we should expect small or no effect of war exposure on cohorts born before or after the WW2 period. The results of the placebo tests confirm our hypotheses since the effect of early exposure to WW2 is not significant for respondents born in time windows different from 1939-1945 (Table 4).

An additional implication of these findings is that respondents' parents who witnessed WW2 did not transmit their traumas to children born after the war. This suggests that the conflict, on average, did not persistently modify parental attitudes. Even in case parents revised their trust upward or downward according to personal experiences of cooperation or conflict during and after WW2, in our data such an update is not passed on to children born after the war. Hence, apart from the direct children's victimization that - as shown below – appears not statistically significant, a likely channel of transmission of WW2 effects is the stress witnessed by parents *during* conflict episodes, who instilled in their children the perception of insecure attachment and inconstant caregiving. As highlighted in Section 2, the psychological literature deems these two factors crucial for the formation of trust at early stages of life.

To check whether our results depend on the chosen econometric specification, we perform several robustness checks. First, we re-estimate the models in Table 3b replacing the attitudinal trust measure with a dummy variable equal to one if the respondent declares to have carried out voluntary work in the last month. This measure has been widely used as a proxy for other-regarding preferences and personal social capital (e.g. Putnam 1993; Glaeser 2000). The negative effect of war exposure remains significant also with this alternative dependent variable.[11] Second, we consider two different ways of clustering standard errors (Tables A1-A2 in the Appendix), i.e. at individual level in order to account for the presence of few panel respondents in waves 4 and 5 of SHARE, and by country/month year of birth in order to account for possible error correlation among individuals born in the same period. Third, we estimate eq. 1 through ordered probit, which accounts for the ordered nature of our dependent variable (Tables A3-A4 in the Appendix). Fourth, we include among controls country-specific characteristics during WW2 (Tables A5-A6 in the Appendix) such as per capita GDP (Maddison 2011), the share of victims during the war period, or the number of civilian and military deaths.[12] All these estimates confirm the negative and significant effect of early exposure to WW2 on generalised trust.

In an additional robustness check we re-estimate the specification in column 3 of Table 2 without excluding respondents who migrated during the war period. To account for the effect of migration across regions in our sample as a possible war-coping strategy, we add a dummy variable equal to one for respondents who moved to other regions during WW2. Results are shown in Table A7 in the Appendix and confirm the robustness of the war effect, while no significant direct effects (column 1) nor compensating effects (column 2) of migration on trust are found.

[11] In our sample 19 percent of respondents declare to have carried out voluntary work in the last month. We used a logistic regression model to account for the binary nature of the new dependent variable. Results are omitted for reasons of space and are available upon request.
[12] Data are collected from Van Mourik (1978), Putzger (1963), Overman (1999) and Statistical Yearbook for the German Reich (1939).

Finally, we re-run the main analysis by using proxies for self-reported victimization during WW2. We run a principal component analysis on a set of dummy variables equal to one if the respondent reports any hunger episode or period of financial hardship, stress or unhappiness during WW2. The first extracted component (*pc_war*) can be thought of as a self-reported measure of war victimization and can substitute or complement the objective war indicator in the specification in column 3 of Table 2. Results are summarized in Table A8 in the Appendix. When considered alone (column 1) or jointly with our war indicator (column 2) self-reported victimization is not significant, while it turns significant (and negative) if interacted with the war indicator (column 3). These results, however, should be considered with caution because the personal victimization measure can be subject to recalling bias due to misreporting of adverse childhood experiences or measurement error (respondents were aged 0-6 when the reported event supposedly happen).[13] Moreover, endogeneity is highly likely to affect estimates with *pc_war* if, for instance, unobserved personality traits (e.g. optimism) influence both the recall of traumatic events and the answer to the generalized trust question. However, also in this robustness check the coefficient of the war indicator remains significant and negative.

7. Selection effects

In spite of the large set of robustness checks run in the previous section, the WW2 effect can still be driven by the respondents' self-selection in our sample because of differential out-of-sample migration, endogenous fertility, and selective mortality. Although with the data at our disposal it is difficult to assess the characteristics of individuals who are not in the sample, we show in this section that our results cannot be entirely explained by these sources of endogeneity.

[13] The meta-study by Hardt and Rutter (2004) emphasizes that the measurement error in the retrospective account of traumatic childhood experiences is non-random with respect to individual characteristics. Endogenous misreporting is also shown to explain the impact of war exposure on civic and political engagement, with the use of objective as opposed to self-reported measures of victimization leading to opposite results (Child and Nikolova 2016).

First, migration would lead us to overestimate the effect of WW2 if respondents with high SES – which is positively associated with trust – had also higher chances to move to non-war places than low-SES respondents. High-SES individuals, for instance, could have exploited their influential connections with visa officials in the home country and relied on personal networks in the destination countries. In addition, they had larger financial resources at their disposal and presumably good knowledge of foreign languages, which made migration smoother and increased job opportunities in the destination country. However, Kesternich et al. (2014) show evidence that out-migration during WW2 was far to be easy. Using their data, we estimated that around 778,000 individuals migrated out of the countries in our sample, while 2,455,000 individuals moved in. In other words, for one person moving out, roughly three persons moved in.[14] This figure has two main implications. First, those entering the countries where the SHARE survey is administered are likely tracked in our analysis. However, we have shown that migration to other regions for them does not play a significant role (Table A7 in the Appendix). Second, if high-SES/high-trust respondents were more likely to migrate, we high-SES/high-trust people should be overrepresented in our sample. Therefore, if there was negative selection due to out-migration, it would have been more than compensated by in-migration, thus leading to an underestimation rather than an overestimation of the true effect of the war.

Second, fertility decisions during WW2 could have been affected by war events. For instance, mothers who anticipated war episodes would have postponed childbirth to non-war periods. If fertility control in Europe during WW2 is more frequent among high-status classes, high-SES/high-trust respondents would be underrepresented in our sample, thereby generating an upward bias to our estimates. To derive some clues on whether fertility systematically differs by SES, we compare the average number of respondents' siblings at the age of ten before, during and after WW2, and by SES at the age of ten (Table A9 in the

[14] What is left out from the migration data provided by Kesternich et al. (2014) is the number of people who moved to the United States. We do not believe this is a crucial omission as migration to the US from the 1920s to 1965 was at its minimum levels due to the quantitative restrictions, which imposed a ceiling on the number of immigrants accepted each year.

Appendix). This comparison shows that there are no remarkable differences in number of siblings over the considered periods, with both the high- and low-SES households displaying a marginally decreasing trend. This evidence is consistent with the fertility analysis carried out by Kesternich et al. (2014) on the same sample as ours (SHARELIFE).

Furthermore, we re-estimate the models in column 3 of Tables 3a-b checking for the heterogeneous impact of WW2 by war period. It seems reasonable to assume that fertility adjustment might not have taken place during the initial phases of WW2, when parents could not perfectly forecast the timing and location of conflict episodes. Hence if fertility played a role, we would overestimate the effect of the war especially on respondents born well after the outbreak of WW2. In later years of the war it was reasonably much easier for high-SES/high-trust parents to postpone childbearing, at least because conflict episodes occurred more frequently or, at least, less unexpectedly. However, Table A10 in the Appendix shows no differential effects of war exposure between those born before 1940 or 1941 vis-à-vis those born after, i.e. when war events were more predictable (Table A11 in the Appendix).[15]

As an additional check for endogenous fertility, Figure A1 in the Appendix displays the average trust levels by SES in the childhood and by conception periods of respondents exposed to WW2. More precisely, we compare trust levels of exposed respondents conceived up to one month *before* the first war-episode (CB) with those of respondents conceived *afterwards* (CA). For parents of the CB group the first WW2-event was realistically more unpredictable than it was for the parents of the CA group, which includes respondents conceived when WW2 already reached the region. If there was a fertility adjustment driven by parental SES and trust, we should observe a significant difference in trust between these two groups. In particular, respondents should have lower trust if conceived when WW2 episodes were predictable (e.g. after the first conflict in the region), because their parents were more likely to adjust fertility according to the dynamics of WW2. However, the figure displays no significant differences

[15] Similar results are obtained considering war exposure at the extensive margin (omitted for reasons of space).

in trust between those conceived before and those conceived after the first war event, neither within the high-SES group (HS) nor within the low-SES group (LS). All these checks jointly considered suggest that our results are not entirely explained by endogenous fertility decisions.

Incidentally, the last comparison leads to two additional considerations. First, selective out-migration, potentially driven by high-SES/high-trust parents leaving their regions of residence in response to WW2 episodes, does not drive our results. If this was the case, trust would have been significantly different on average between CA and CB respondents of the high-SES group. However, Figure A1 in the Appendix suggests that this is not the case. Second, it seems that the effect of war exposure is not driven by mothers' stress or health problems during pregnancy (i.e. 'in-utero' exposure). By construction, mothers of the CA group were exposed to WW2 episodes for a longer period during pregnancy than their counterparts in the CB group. If WW2 affected respondents' trust when they were in utero, we would expect a significant difference in trust between the CA and CB group. Again, this is not the case in our data.

Finally, mortality does not seem to be a major concern. If war-driven mortality was higher for low-SES individuals (whose trust would have been lower anyway), the WW2 effect we found is a lower bound of the real effect. To further exclude this unobserved source of selection, Kesternich et al. (2014) compare the age of death of the SHARE participants' father by i) SES, ii) living in war vs. non-war countries, and iii) year of birth (before 1946 or after 1945). Since no significant differences were found, they conclude that selection on mortality is not large enough to drive the main results.

8. Conclusion

If pre-school children form trust in others prevalently through the interaction with their parents, a secure attachment bond with caregivers and responsive childcare are important factors for nurturing positive expectations about the surrounding world. However, personal traumatic experiences and parents' reactions to distressing events during the early stages of life may adversely

affect children's trust and their beliefs about the trustworthiness of others, with long-term consequences on social preferences. In particular, apart through direct victimization into conflict episodes at a vulnerable age, a war may influence infants' trust in the long term also through the stress and anxiety intentionally or unintentionally exhibited by parents after acts of war.

With this paper we test the long-term impact of early exposure to the WW2 on the levels of trust of European adults. Our war measure captures the number of months of exposure to combat episodes related to the WW2 during the period 1939-1945. By exploiting variation in the period and place of combats, and in month-year and region of birth of respondents, we identify the effect of early exposure to WW2 on trust, both at the intensive and extensive margin.

Controlling for month-year of birth and region fixed effects, we provide empirical evidence of a significant and negative effect of war exposure at age 0-6 on trust in adulthood. Surprisingly, the gap in trust due to early exposure to war does not narrow when accounting for several confounding factors, including current socio-demographic, economic and health characteristics, socio-economic status and health conditions in the childhood, episodes of hunger witnessed during the war, the absence of a parent in the childhood, the country levels of GDP per capita and the number of deaths during WW2. Results are also robust to different estimation methods and seem not to be driven by differential migration, endogenous fertility and selective mortality. Furthermore, they are corroborated by placebo tests showing that the significant effects of WW2 on trust are significant only for those born in the period 1939-1945.

Our findings suggest that exposure to war at early age may have a persistent impact on trust later in life. The effect of WW2 is not explained by parental investment in human capital, current and past socio-economic status, or by personal traumatization. We posit that WW2 could have affected trust through perceived stress and anxiety of parents at home, e.g. via inconstant childcare and insecure attachment to parents. These pathways from war to trust seem to be supported by our placebo results. The latter suggest that the war did not modify

the trust levels of the respondent's parents, but rather the effects of WW2-exposure passed on through the temporary stress they faced when the conflict events occurred. However further research is needed to pin down the exact psychological channels through which war exposure early in life affects trust levels at adult age.

Concluding, the effect of a conflict may well be temporary, with affected communities re-establishing or increasing pro-social attitudes and cooperation soon after a violent conflict (Bauer et al. 2016). Yet, our findings provide robust evidence that for infants a war is forever. Given the effects of trust on a variety of socio-economic outcomes, traumatic events witnessed by the 'children of war' might influence the type of society they will live in when they become adults.

Data disclaimer
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Figure 1 – Examples of WW2-exposure

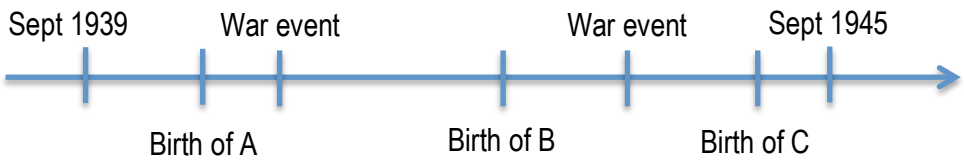
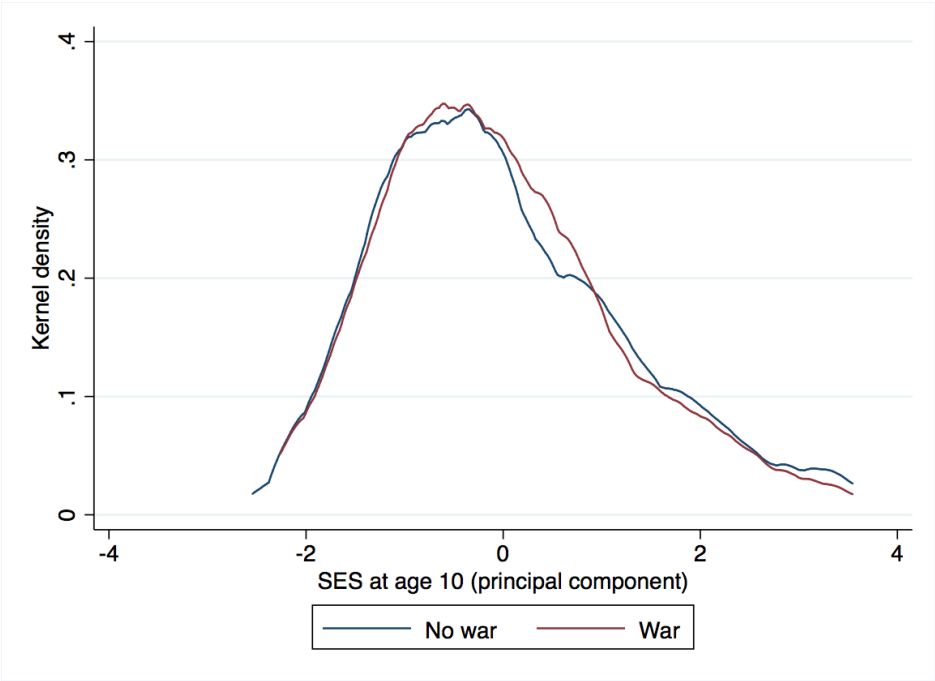


Figure 2 – Socio-economic status during childhood and exposure to WW2

A



B

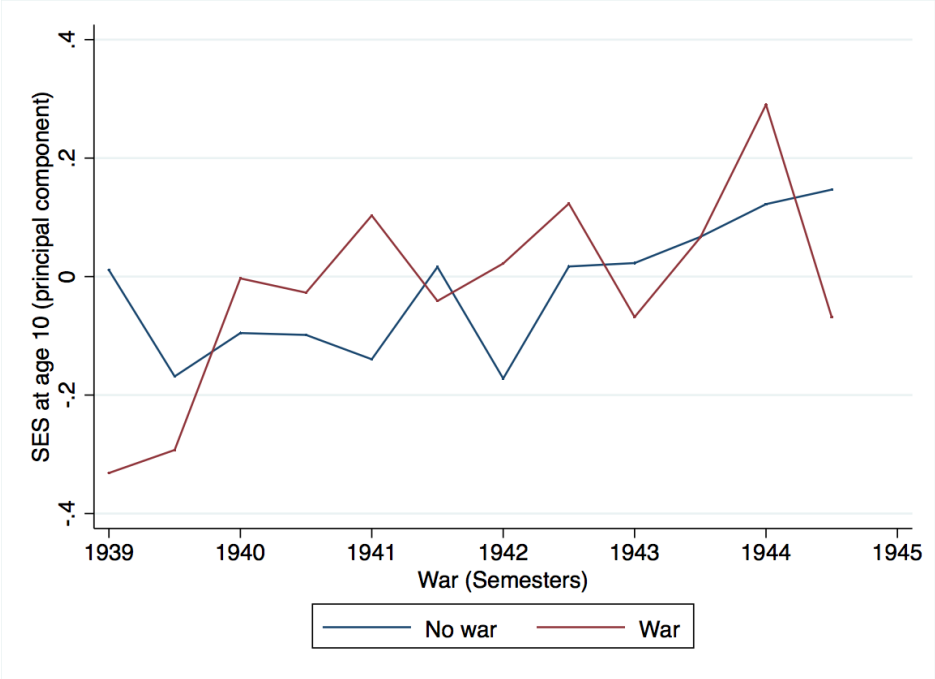
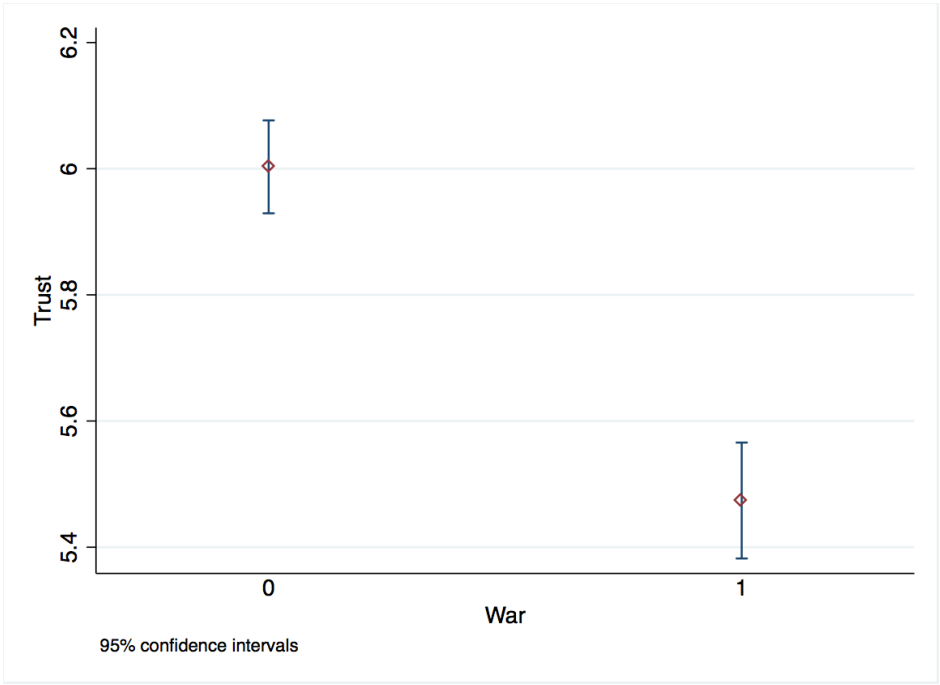


Figure 3 – Average trust by WW2-exposure

A



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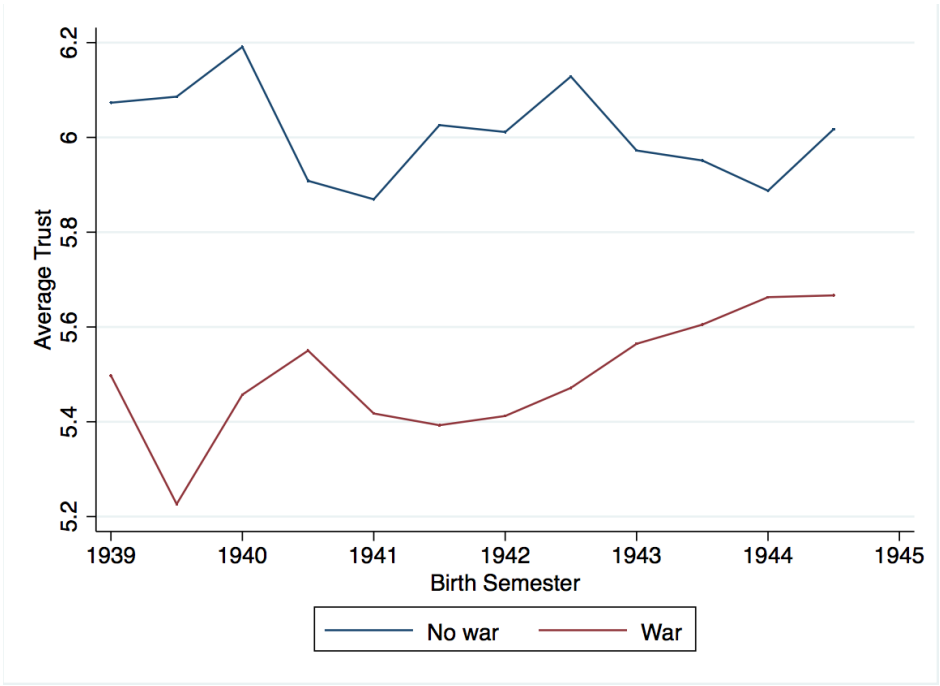


Figure 4a – Regional distribution of war episodes during WW2

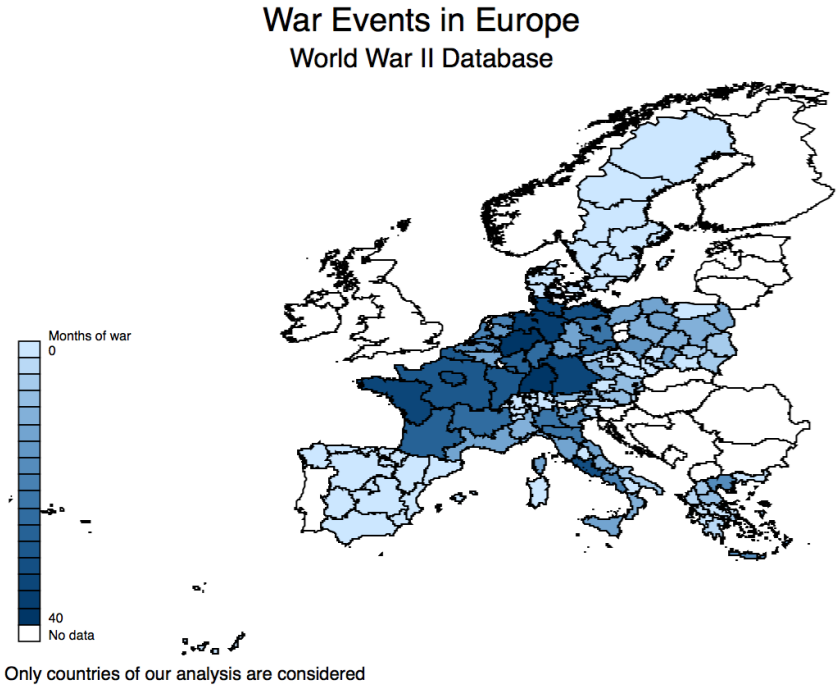


Figure 4b –Respondents exposed to at least one war episode during WW2

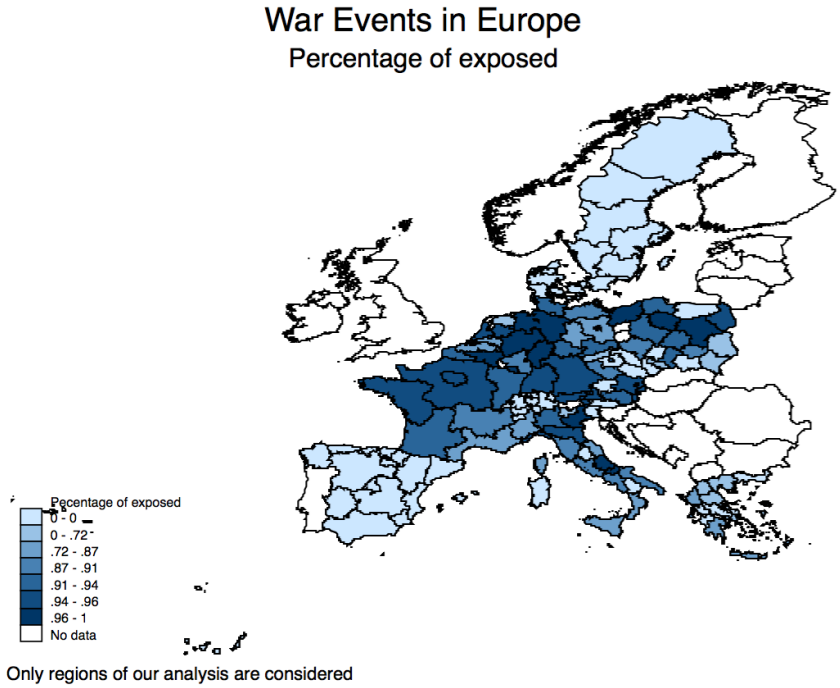


Table 1 – Descriptive Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Trust	6,759	5.80	2.423	0	10
War (0-1)	6,759	0.39	0.488	0	1
War (0-2)					
0 = No war	6,759	0.61	0.49	0	1
1 = 1-3 months	6,759	0.19	0.19	0	1
2 = 3+ months	6,759	0.20	0.19	0	1
Year of birth					
1939	6,759	0.05	0.208	0	1
1940	6,759	0.16	0.362	0	1
1941	6,759	0.14	0.347	0	1
1942	6,759	0.16	0.367	0	1
1943	6,759	0.17	0.377	0	1
1944	6,759	0.19	0.395	0	1
1945	6,759	0.13	0.340	0	1
Wave					
2	6,759	0.61	0.489	0	1
5	6,759	0.39	0.489	0	1
Female	6,759	0.54	0.498	0	1
Marital status					
Married and living together with spouse	6,756	0.75	0.435	0	1
Registered partnership	6,756	0.01	0.121	0	1
Married, living separated from spouse	6,756	0.02	0.122	0	1
Never married	6,756	0.05	0.210	0	1
Divorced	6,756	0.07	0.252	0	1
Widowed	6,756	0.10	0.311	0	1
Income percentile					
1	6,759	0.07	0.263	0	1
2	6,759	0.09	0.281	0	1
3	6,759	0.11	0.312	0	1
4	6,759	0.11	0.315	0	1
5	6,759	0.12	0.318	0	1
6	6,759	0.11	0.316	0	1
7	6,759	0.12	0.322	0	1
8	6,759	0.10	0.299	0	1
9	6,759	0.09	0.291	0	1
10	6,759	0.08	0.271	0	1
Job Status					
Retired	6,729	0.73	0.443	0	1
Job (Employed, self-employed, Homemaker)	6,729	0.23	0.423	0	1
No job (Unemployed, Sick or disabled)	6,729	0.04	0.184	0	1
Education					
None or Primary	6,755	0.29	0.454	0	1
Lower Secondary	6,755	0.20	0.395	0	1
Upper Secondary	6,755	0.28	0.450	0	1
Tertiary	6,755	0.23	0.423	0	1
Health functionalities	6,759	-0.18	1.449	-0.916	12.475
Memory	6,730	9.09	3.269	0	20
SES in childhood (first extracted component)	6,439	1.59e-09	1.251	-2.539	3.580
N. chronic diseases	6,752	1.15	1.151	0	7
Hunger episode (during WW2)	6,759	0.03	0.164	0	1
Mother at age 10 (0= absent)	6,753	0.95	0.225	0	1
Father at age 10 (0= absent)	6,753	0.872	0.334	0	1
Vaccinated when child	6,702	0.04	0.193	0	1
Rural area when child	6,737	0.45	0.498	0	1
Health status when child					
Excellent	6,723	0.35	0.476	0	1

Very Good	6,723	0.32	0.467	0	1
Good	6,723	0.25	0.431	0	1
Fair	6,723	0.07	0.247	0	1
Poor	6,723	0.02	0.146	0	1

Table 2 – War exposure and trust (OLS estimates)

Dep var: *Trust*	(1)	(2)	(3)	(4)
War	-0.243**		-0.243**	
	(0.123)		(0.122)	
War (Ref=No war)				
1-3 months		-0.175		-0.220
		(0.136)		(0.136)
4+ months		-0.336**		-0.318**
		(0.146)		(0.145)
Wave 5	0.210***	0.211***	0.327***	0.327***
	(0.0596)	(0.0596)	(0.0621)	(0.0621)
Female	0.0802	0.0812	0.0911	0.0918
	(0.0574)	(0.0574)	(0.0612)	(0.0612)
Marital status (Ref=Married)				
Registered partnership			-0.00606	-0.00222
			(0.236)	(0.236)
Married, living separated from spouse			-0.343	-0.333
			(0.238)	(0.239)
Never married			0.0457	0.0448
			(0.142)	(0.142)
Divorced			-0.217*	-0.214*
			(0.122)	(0.122)
Widowed			0.160	0.162
			(0.100)	(0.100)
Income percentile (Ref=10)				
1			-0.274*	-0.276*
			(0.153)	(0.153)
2			-0.531***	-0.534***
			(0.148)	(0.148)
3			-0.462***	-0.462***
			(0.137)	(0.137)
4			-0.268**	-0.268**
			(0.134)	(0.134)
5			-0.383***	-0.383***
			(0.132)	(0.132)
6			-0.259**	-0.256*
			(0.132)	(0.132)
7			-0.194	-0.195
			(0.130)	(0.130)
8			-0.174	-0.176
			(0.134)	(0.134)
9			-0.116	-0.115
			(0.136)	(0.136)
Job status (Ref=Retired)				
Job			0.239***	0.240***
			(0.0765)	(0.0765)
No job			0.249	0.240
			(0.162)	(0.162)
Education (Ref=Primary)				
Lower Secondary			0.243***	0.245***
			(0.0907)	(0.0907)

Upper Secondary			0.273***	0.274***
			(0.0862)	(0.0862)
Tertiary			0.541***	0.542***
			(0.0942)	(0.0942)
Health functionalities			-0.131***	-0.130***
			(0.0212)	(0.0212)
Memory			0.0653***	0.0652***
			(0.00981)	(0.00982)
Month/Year of birth FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Constant	5.616***	5.519***	4.873***	4.789***
	(0.490)	(0.496)	(0.499)	(0.504)
Observations	6,555	6,555	6,494	6,494
R-squared	0.157	0.157	0.189	0.189

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3a – War exposure and trust: *the role of childhood SES*

Dep var: *Trust*	(1)	(2)	(3)	(4)
War	-0.269**	-0.269*	-0.282**	-0.282**
	(0.123)	(0.138)	(0.124)	(0.138)
SES in childhood	0.144***	0.144***	0.145***	0.145***
	(0.0282)	(0.0319)	(0.0285)	(0.0321)
N. of chronic diseases			-0.128***	-0.128***
			(0.0261)	(0.0294)
Hunger episode			-0.192	-0.192
			(0.190)	(0.257)
Mother at age 10			0.360**	0.360
			(0.173)	(0.233)
Father at age 10			-0.152	-0.152
			(0.106)	(0.126)
Health status when child (Ref = Excellent)				
Very good			0.0214	0.0214
			(0.0734)	(0.0780)
Good			-0.0157	-0.0157
			(0.0823)	(0.0874)
Fair			-0.229*	-0.229
			(0.130)	(0.150)
Poor			-0.472**	-0.472*
			(0.211)	(0.285)
Vaccinated when child			-0.177	-0.177
			(0.154)	(0.156)
Rural area when child			-0.0216	-0.0216
			(0.0649)	(0.0726)
Month/Year of birth FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
Socio-dem. controls	Yes	Yes	Yes	Yes
Observations	6,213	6,213	6,134	6,134
R-squared	0.194	0.194	0.200	0.200

Robust standard errors in parentheses, clustered by country/month-year of birth (columns 2 and 4); Socio-dem. controls as in Table 2; *** p<0.01, ** p<0.05, * p<0.1.

Table 3b – War exposure and trust: *the role of childhood SES*

Dep var: *Trust*	(1)	(2)	(3)	(4)
War (Ref=No war)				
1-3 months	-0.178	-0.178	-0.188	-0.188
	(0.141)	(0.153)	(0.142)	(0.153)
4+ months	-0.387**	-0.387**	-0.404***	-0.404**
	(0.152)	(0.169)	(0.153)	(0.169)
SES in childhood	0.143***	0.143***	0.144***	0.144***
	(0.0282)	(0.0319)	(0.0285)	(0.0321)
N. of chronic diseases			-0.127***	-0.127***
			(0.0261)	(0.0294)
Hunger episode			-0.191	-0.191
			(0.190)	(0.258)
Mother at age 10			0.363**	0.363
			(0.173)	(0.233)
Father at age 10			-0.151	-0.151
			(0.106)	(0.126)
Health status when child (Ref = Excellent)				
Very good			0.0226	0.0226
			(0.0734)	(0.0780)
Good			-0.0133	-0.0133
			(0.0823)	(0.0872)
Fair			-0.226*	-0.226
			(0.130)	(0.150)
Poor			-0.474**	-0.474*
			(0.211)	(0.284)
Vaccinated when child			-0.180	-0.180
			(0.154)	(0.156)
Rural area when child			-0.0219	-0.0219
			(0.0649)	(0.0725)
Month/Year of birth FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
Socio-dem. controls	Yes	Yes	Yes	Yes
Observations	6,213	6,213	6,134	6,134
R-squared	0.194	0.194	0.200	0.200

Robust standard errors in parentheses, clustered by country/month-year of birth (columns 2, 4); *** p<0.01, ** p<0.05, * p<0.1.

Table 4 – War exposure and trust: *placebo tests.*

Birth cohort	**War effect**		
(1)	(2)	(3)	
	War [0/1]	*War [0/2]; Ref. = No War*	
1930-1936	0.0714	*1-3 months*	-0.0640
	(0.155)		(0.177)
		4+ months	0.257
			(0.195)
1931-1937	0.224	*1-3 months*	0.253
	(0.149)		(0.173)
		4+ months	0.187
			(0.185)
1932-1938	0.203	*1-3 months*	0.167
	(0.137)		(0.157)
		4+ months	0.253
			(0.172)
1939-1945	-0.282**	*1-3 months*	-0.188
	(0.124)		(0.142)
		4+ months	-0.404***
			(0.153)
1946-1952	0.0445	*1-3 months*	0.0774
	(0.113)		(0.127)
		4+ months	-0.000646
			(0.138)
1947-1953	-0.0648	*1-3 months*	0.0325
	(0.132)		(0.145)
		4+ months	-0.188
			(0.161)
1948-1954	0.141	*1-3 months*	0.177
	(0.126)		(0.140)
		4+ months	0.0896
			(0.157)
1949-1955	0.0672	*1-3 months*	0.122
	(0.184)		(0.187)
		4+ months	-0.0836
			(0.235)
1950-1956	0.228	*1-3 months*	0.256*
	(0.146)		(0.153)
		4+ months	0.166
			(0.193)
1951-1957	0.0672	*1-3 months*	0.122
	(0.184)		(0.187)
		4+ months	-0.0836
			(0.235)

Column 2 and 3 show regression coefficient and robust standard errors (in parentheses) from the replication of Tables 3a-b (column 3). All estimates include region and month/year of birth fixed effects, the number of chronic diseases measured in the adulthood and childhood controls (SES in childhood, the presence of a parent at age 10, any hunger episode happened during the war, self-assessed health status when the respondent was a child, residence in a rural area during the childhood, and any vaccination received at early age); *** p<0.01, ** p<0.05, * p<0.1.

APPENDIX

Table A1 – War exposure and trust (OLS estimates)

Dep var: *Trust*	(1)	(2)	(3)	(4)
War	-0.243*	-0.243*	-0.243*	-0.243*
	(0.142)	(0.139)	(0.141)	(0.138)
Month/Year of birth FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
Socio-dem. controls	No	Yes	No	Yes
Observations	6,555	6,494	6,555	6,494
R-squared	0.157	0.189	0.157	0.189

Robust standard errors in parentheses, clustered by country/month-year of birth (columns 1 and 2) and at individual level (columns 3 and 4); *** p<0.01, ** p<0.05, * p<0.1.

Table A2 – War exposure and trust (OLS estimates)

Dep var: *Trust*	(1)	(2)	(3)	(4)
War (Ref=No war)				
1-3 war events	-0.153	-0.164	-0.153	-0.164
	(0.156)	(0.152)	(0.159)	(0.155)
4+ war events	-0.356**	-0.344**	-0.356**	-0.344**
	(0.175)	(0.171)	(0.175)	(0.172)
Month/Year of birth FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
Socio-dem. controls	No	Yes	No	Yes
Observations	6,555	6,494	6,555	6,494
R-squared	0.157	0.189	0.157	0.189

Robust standard errors in parentheses, clustered by country/month-year of birth (columns 1 and 2) and at individual level (columns 3 and 4); *** p<0.01, ** p<0.05, * p<0.1.

Table A3 – War exposure and trust (ORDERED PROBIT estimates)

Dep var: *Trust*	(1)	(2)	(3)	(4)
War	-0.104*	-0.105*	-0.104*	-0.105*
	(0.0555)	(0.0559)	(0.0627)	(0.0623)
Month/Year of birth	Yes	Yes	Yes	Yes
Region during war	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
Socio-dem. controls	No	Yes	No	Yes
Observations	6,555	6,494	6,555	6,494

Robust standard errors in parentheses, clustered at individual level (columns 3 and 4) *** p<0.01, ** p<0.05, * p<0.1.

Table A4 – War exposure and trust (ORDERED PROBIT estimates)

Dep var: *Trust*	(1)	(2)	(3)	(4)
War (Ref=No war)				
1-3 months	-0.0576	-0.0624	-0.0576	-0.0624
	(0.0637)	(0.0641)	(0.0705)	(0.0698)
4+ months	-0.161**	-0.159**	-0.161**	-0.159**
	(0.0681)	(0.0687)	(0.0777)	(0.0778)
Month/Year of birth FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
Socio-dem. controls	No	Yes	No	Yes
Observations	6,555	6,494	6,555	6,494

Robust standard errors in parentheses, clustered at individual level (columns 3 and 4); *** p<0.01, ** p<0.05, * p<0.1.

Table A5 – War exposure and trust: *the effect of GDP and the share of deaths*

Dep var: *Trust*	(1)	(2)	(3)	(4)
War	-0.326**		-0.374**	
	(0.139)		(0.156)	
War (Ref=No war)				
1-3 months		-0.253		-0.287*
		(0.157)		(0.169)
4+ months		-0.417**		-0.487**
		(0.168)		(0.193)
Gdp	-0.00254	-0.000783	-0.00950	-0.00723
	(0.0113)	(0.0115)	(0.0122)	(0.0125)
Share of deaths	-18.15*	-19.25**	-16.42	-17.74
	(9.380)	(9.449)	(12.34)	(12.34)
Month/Year of birth FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
Socio-dem. controls	Yes	Yes	Yes	Yes
Childhood SES	No	No	Yes	Yes
Observations	5,118	5,118	4,830	4,830
R-squared	0.188	0.188	0.200	0.200

Standard errors in parentheses, clustered by country/month year of birth (columns 3-4). Columns 3 and 4 include the number of chronic diseases measured in the adulthood as well as childhood controls (i.e. SES in childhood, the presence of a parent at age 10, any hunger episode happened during the war, self-assessed health status when the respondent was a child, residence in a rural area during the childhood, and any vaccination received at early age); *** p<0.01, ** p<0.05, * p<0.1

Table A6 – War exposure and trust: *the effect of GDP and the number of deaths*

Dep var: *Trust*	(1)	(2)	(5)	(6)
War	-0.326**		-0.374**	
	(0.139)		(0.156)	
War (Ref=No war)				
1-3 months		-0.253		-0.287*
		(0.157)		(0.169)
4+ months		-0.417**		-0.487**
		(0.168)		(0.193)
Gdp	-0.00254	-0.000783	-0.00950	-0.00723
	(0.0113)	(0.0115)	(0.0122)	(0.0125)
Civilian deaths (x100,000)	0.00996	0.0103	0.0358	0.0362
	(0.0907)	(0.0907)	(0.0251)	(0.0249)
Military deaths (x100,000)	-0.404*	-0.428**	-0.371	-0.401
	(0.211)	(0.212)	(0.276)	(0.275)
Month/Year of birth FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
Socio-dem. controls	Yes	Yes	Yes	Yes
Childhood SES	No	No	Yes	Yes
Observations	5,118	5,118	4,830	4,830
R-squared	0.188	0.188	0.200	0.200

Standard errors in parentheses, clustered by country/month year of birth (columns 3-4). Column 3 and 4 include the number of chronic diseases measured in the adulthood as well as childhood controls (i.e. SES in childhood, the presence of a parent at age 10, any hunger episode happened during the war, self-assessed health status when the respondent was a child, residence in a rural area during the childhood, and any vaccination received at early age); *** p<0.01, ** p<0.05, * p<0.1.

Table A7 – War exposure and trust: *migration as a war-coping strategy*

Dep. Var.: *Trust*	(1)	(2)
War	-0.240**	-0.255**
	(0.117)	(0.118)
Moved during war	0.0224	-0.0397
	(0.0873)	(0.120)
War*Moved during war		0.131
		(0.175)
Month/Year of birth FE	Yes	Yes
Region FE	Yes	Yes
Wave FE	Yes	Yes
Socio-dem. controls	Yes	Yes
Observations	7,314	7,314
R-squared	0.182	0.182

Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table A8 – War exposure and trust: *self-reported victimization*

Dep var: *Trust*	(1)	(2)	(3)
War		-0.251**	-0.256**
		(0.123)	(0.123)
Pc_War	-0.00665	-0.00613	0.0399
	(0.0277)	(0.0277)	(0.0360)
War*Pc_War			-0.112**
			(0.0562)
Month/Year of birth FE	Yes	Yes	Yes
Region FE	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes
Socio-dem. controls	Yes	Yes	Yes
Observations	6,380	6,380	6,380
R-squared	0.190	0.190	0.191

Standard errors in parentheses. *Pc_war* is the first extracted component from a principal component analysis including a set of dummy variables equal to one if the respondent remembers a hunger episode or a period of financial hardship, stress or happiness during WW2. *** p<0.01, ** p<0.05, * p<0.1.

Table A10 – Average number of siblings by SES at age 10.

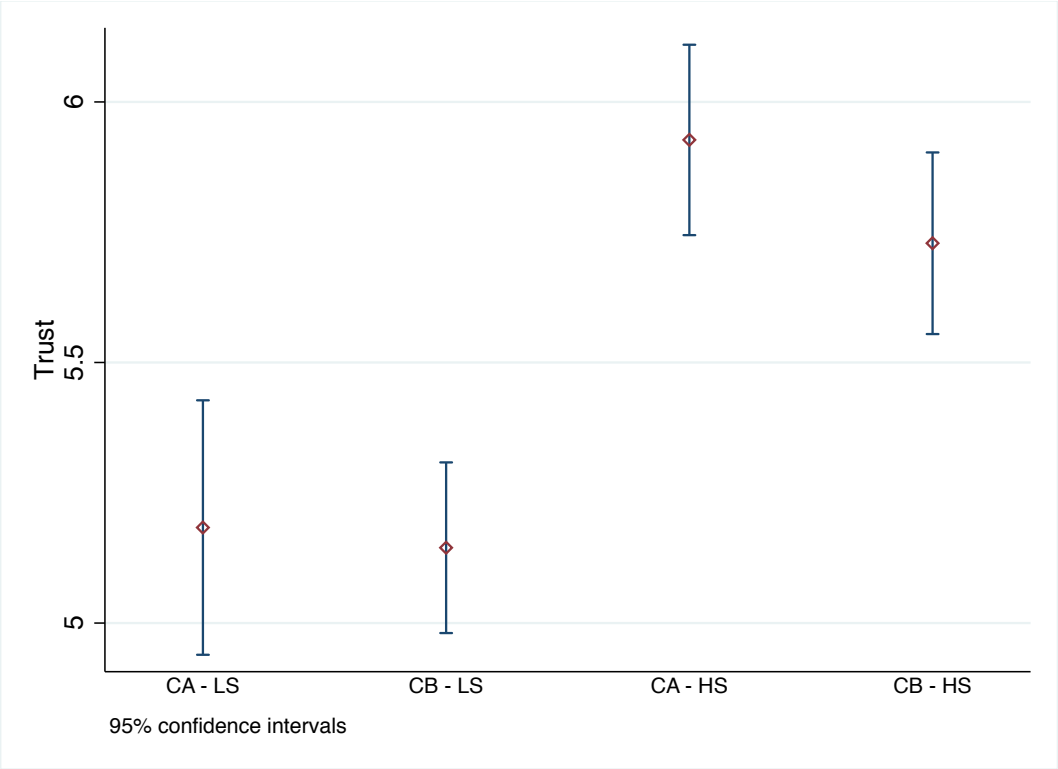
SES	**Born before 1929**	**Born in 1929-1935**	**Born after 1935**
Low	2.56	2.52	2.33
High	2.06	2.03	1.88

Table A11 – Heterogeneous war effects by years of birth.

	(1)	(2)	(3)	(4)
	x = 1941		*x* = 1940	
War	-0.284**	-0.226*	-0.286***	-0.262**
	(0.111)	(0.127)	(0.107)	(0.124)
Born before *x*	0.0746	0.163	-0.180	0.276
	(0.104)	(0.356)	(0.220)	(0.408)
Born before *x* * War	-0.0496	-0.0767	0.290	0.384
	(0.146)	(0.150)	(0.281)	(0.285)
Month/Year of birth FE	No	Yes	No	Yes
Region FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
Observations	6,555	6,555	6,555	6,555
R-squared	0.144	0.157	0.144	0.157

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Figure A1 – Heterogeneous war effects by childhood SES and conception period (only exposed to WW2).



Legend: *CA* = Conceived during or after the first WW2 event in the region of birth; *CB* = Conceived up to the first WW2 event in the region of birth; *LS* = Below the median value of SES in the childhood; *HS* = Above (or equal to) the median value of SES in the childhood.