Impact of early-life stress on long-run labour market outcomes *

Joanna Kopinska† Vincenzo Atella ‡

Edoardo Di Porto§ Maarten Lindeboom¶

VERY PRELIMINARY DRAFT
DO NOT CITE

Sept 2019

Abstract

This paper analyses effects of in-utero exposure to maternal stress on life-long labour market outcomes within an administrative dataset on the universe of Italian workers. We exploit a quasi-experimental setting, where among males conceived before the Armistice in World War II (WWII), we analyze differences in terms of long-run labour market outcomes between cohorts born in municipalities who experienced Nazi rides, versus the same cohorts born in municipalities without such violence. We find that exposure to stress in utero exerts a negative effect on a range of subsequent labour outcomes. More importantly, we find that the effects do not dissipate along the career, but rather they increase with time. The effects are strongest for exposure in the first months of pregnancy. Finally, we show that additional random shocks throughout the working career extends the penalty for the disadvantaged individuals.

JEL Codes: I1, O1
Keywords: Early-life; Stress; Life-long earnings; Labour market.

*We are grateful to Luciano Bucci for introducing us to the war events and military strategies during the WWII in Italy. Furthermore, we acknowledge an important contribution of "The Atlas of Nazi and Fascist massacres" project, providing us with a rich and detailed dataset crucial for the realization of the analysis. The authors declare that they have no relevant or material financial interests that relate to the research described in this paper. The usual disclaimers apply.

†University of Rome Tor Vergata; e-mail*kopinska@economia.uniroma2.it.*

‡University of Rome Tor Vergata; e-mail: *atella@uniroma2.it.*

§Federico II University of Napoli; e-mail: *edoardo.diporto@unina.it.*

¶Vrije Universiteit Amsterdam; e-mail: *m.lindeboom@vu.nl.*

1 Introduction

Environmental conditions affect health and this effect is strongest during the earliest periods of life, when growth is most rapid. The essential idea is that fetus exposed to a shock in the intrauterine period activates alert mechanisms which remain programmed later in life, leading to disequilibrium(Almond and Currie, 2012). A very pertinet shock in modern societies is stress, ranging from acute time-limited events to chronic indefinite stressors. Recently Aizer et al. (2016) have shown that high doses of stress during pregnancy reduce educational attainment of the offspring. If the ability gap between the advantaged and the disadvantaged opens up in the earliest phases of life, how does it evolve during their future working carries?

In this paper we attempt to describe this phenomenon by studying labour market outcomes of individuals whose pregnant mothers witnessed an episode of Nazi raid during World War II (WWII). In a quasi-experimental setting, we analyze the difference between cohorts of males born in municipalities right before and after a Nazi ride, relative to the same cohorts that went through no violence. Nazi rides represented unexpected episodes of various nature characterized by relatively few casualties. We carefully select episodes with low number of murders to focus on the consequences of psychological trauma underwent by pregnant mothers rather than physical harm

Stress is likely to affect a developing fetus through the production of cortisol (corticotrophin-releasing hormone) by the mother, which is found to interfere with the fetus development and its immune function. As low socio-economic status mothers experience on average higher levels of stress, a causal link between fetal stress exposure and later life outcomes sheds light on one channel trough which disadvantage might be transmitted across generations. From this perspective allocating resources in programs supporting fetal health might have relevant rates of return and bring efficiency gains with respect to other traditional childhood investments.

A very narrow group of studies examine the effect of stress exposure of pregnant mothers and, to our knowledge, no one discuss its role on outcomes relative to long-run working careers. Aizer et al. (2016) analyze the effects of directly measured stress hormone cortisol levels during pregnancy on educational attainment in a sibling fixed effects setting. They find that

in utero exposure to elevated levels of stress negatively affects offspring cognition, health, and educational attainment. Black et al. (2016) show that stress induced by the death of the mother's parent during pregnancy has a small negative effect on birth and long-term adult outcomes. Finally, Persson and Rossin-Slater (2018) show that a death of a maternal relative reduces psychological health in the long-run. These papers produce an interesting set of evidence, showing the important role played by stress during pregnancy, although from a methodological viewpoint, they focus on stress observed in mothers whose stress exposure might be correlated with a number of unobserved events and circumstances.

The novelty of our study with respect to this growing literature is twofold. First of all, for the first time we observe to which extent being exposed to mother stress while in utero can affect long-term labour market outcomes (onset of careers, qualifications, pensions) and the evolution of working careers (life-long earnings), as well as mortality and disability. Remarkably, our measures of labor outcomes are particularly interesting as we observe the universe of workers for more than 30 years of their labor activity. Second, we use a quasi experimental setting, based on the unexpected onset of Armistice and the subsequent Nazi rides, which provides a better identification strategy compared to previous studies. In fact, the incidence of the war events (and the Nazi Massacres in particular) varied widely and idiosyncratically across municipalities, where similar pregnant mothers experienced strikingly different environment while pregnant. This setting and the data characteristics give us the opportunity to employ a treatment effect disaggregated on a fine time-space scale and to control for several confounding effects such as selective fertility issues, survival and attrition. In fact, individuals who decide to conceive children after the onset of violence might not be a random sample of the population. To downplay the potential issues of endogenous fertility, we restrict the sample to individuals born within a 9 month window around the Armistice (01.1943-05.1944), all conceived before the unexpected increase in prenatal insults. Additionally, for each municipality, we consider only the first ride episodes, leaving out repeated massacres. We thus eliminate from the sample individuals conceived after the first Nazi ride. To avoid the selection process that might be at play during the intrauterine phase (as foetuses might not survive

the prenatal era and die in a miscarriage or in the early life) we control for mortality at birth and during the first year from birth. Finally, we control also for attrition checking if there is evidence of a differential survival pattern of individuals once they reach the ages of 35.

The empirical analysis is based on merging three unique datasets, which allow us to trace, for the universe of Italian workers, the in utero exposure to the episodes of Nazi rides, distinguishing them from the evolution of the WWII battlefront events, using an identification strategy that classifies individuals on their month and municipality of birth. The labour market outcomes come from a longitudinal administrative dataset collected by the Istituto Nazionale della Previdenza Sociale (INPS), the Italian social security and welfare institute and allow us to observe individual labour outcomes between the ages of 35 and 75. To correctly measure our effect, we isolate several forms of confounding coming from the daily-life difficulties brought around with WWII (i.e., nutritional shortages with potential physical effects on the pregnant mother) from specific episodes of violence which affected a number of municipalities causing severe psychological stress. In this respect, the data we have on Nazi violence provides us information on the age and gender composition of victims, thus allowing to isolate to a reasonable extent Nazi violence which might have been particularly traumatic for pregnant women. We also show that some of the confounding effects for which we control for are irrelevant. In particular, we find that that mortality due to pregnancy complications increased with WWII intensity, but that the relative effect of Nazi violence incidence was null. Finally, we find also that the disadvantaged were also more likely to die during the early childhood, but we find no effect of the stressful events on the cohort size. We also find no evidence of a differential survival pattern of individuals once they reach the ages of 35. On the contrary, we find that exposure to stress in utero exerts a negative effect on a range of labour outcomes. These effects do not dissipate along the career, but rather increases with time, and reach a penalisation of 4% of earnings. The effects are strongest when the insult occurs in the first months of pregnancy. Finally, we show that additional shocks throughout the working career causes a higher penalty for the disadvantaged individuals.

The paper unfolds in the following way: Section 2 describes specific aspects of WWII

in Italy, relevant from the point of view of this study, Section 3 presents the data and the relative sources. Section 4 discusses the empirical strategy, while in Section 5 we reviews the identification issues and solutions, and in Section 6 we discuss the core results together with the robustness exercises showing the insensitivity of our results to alternative specifications. Finally, Section 7 concludes.

2 WWII in Italy: a natural experiment setting

In this section we summarise the historical events of the WWII in Italy, which are crucial to understand the empirical framework of this study. The events of the so called *armistice*, under very mild assumptions discussed in Section 4, set up a scenario that can be considered as a natural experiment.

Despite WWII started in Sept 1939, Italy was a non-belligerent country until June 1940, when Mussolini declared war to Britain and France. From June 1940 until the end of summer 1943, Italy moved her armed troops mostly outside its national territory, the Italian territory was only modestly affected by war events. In fact, this period was marked by a relatively scarce number of casualties, which concentrated in the surroundings of strategic bombing targets, such as military and commercial harbors, significant industrial sites (i.e. metallurgic, transport and heavy machinery industries), or important railways (Baldoli and Knapp, 2011, 2012).

On September 3$^{\text{rd}}$ 1943, the armistice was secretly signed, but it was made public on the radio just on the 8$^{\text{th}}$, with the prime minister and the Italian royal family escaping from Rome on the morning of the 9$^{\text{th}}$.[1]

Only few weeks before that date, the Allies landed in Sicily (July 1943), in order to start

[1]The armistice was secretly signed in Santa Teresa Longarini district of Syracuse, 3 km away from Cassibile, from which the armistice was named (Vitali, 1980). It is recognised as the act by which the Kingdom of Italy ceased hostilities towards the Allies during World War II and the beginning of the Italian resistance against fascism. Since this act established its entry into force from the moment of its public announcement, it is commonly referred to as "September 8$^{\text{th}}$", when, at 18:30 Italian time, was announced earlier by the microphones of Radio Algiers by General Dwight Eisenhower and, just one hour later, at 19:42, confirmed by the proclamation of Marshal Pietro Badoglio via the Italian public broadcasting network (EIAR) microphones (**?**).

their campaign against axis forces (see Figure 1). Despite, the arrival of the Allied troops occured before the 8th of September, it is after the armistice that the Italian campaign changed radically, as well as the destiny of the Italian population. In fact, by September 8th1943 the Italian Army was left without instructions about the line of conduct to be taken against the German armed forces. In the days following the armistice most Italian servicemen were left without orders from higher echelons, due to Wehrmacht ceasing Italian radio communication. The dismantle of the Italian military troops was immediately evident, as well as the absence of a clear military strategy. There is abundant historical evidence reporting episodes of contradictory orders coming from higher echelons to the troops. For example, the 10th of September 1943, in Piombino, a small german flotilla, tried to enter the harbour of Piombino but was denied access by port authorities. In that case servicemen of the harbour received two contrasting orders, one from the Italian coastal forces commanded by a former fascist gerarca granting the access, while and another one from the naval commander denying the access to the port. Also the civilian population had no information about the evolution of the events. Given the unforeseen circumstances that brought the royal family to sign the armistice, the events of September 1943 were very difficult to predict, ruling out any possibility of strategic reaction by civilians. According to Strazza (2010), very frequently the information about the arrival of military troops did not spread across neighbouring villages Strazza (2010). Moreover, according to Baldoli and Knapp (2011), national evacuation plans did not exist. On top of that, from a logistics point of view, moving across provinces was extremely difficult, since railroads and main transportation networks were destroyed by the Allies tactical bombing (Baldoli and Knapp, 2012).

The post September 1943 period exposed the Italian territory to two major types of adversities: *i)* general armed conflicts between the armies with an indirect impact on the civilians, and *ii)* Nazi violence aimed directly at the civilian population and resistance fighters.

Concerning the general armed conflict, the post armistice period was characterised by war events of different scale, ranging from quick victories and movements of the frontline entailing relatively limited casualties, to long stalemates associated with a sizeable number of fatalities.

Figure 2 reports a detailed description of the heterogeneous distribution of civilian casualties over time and space. The army movements started rapidly in the Southern Italy, and then got stuck along the Winter line, experiencing a very long stalemate, which caused huge losses among civilians.[2]

The Nazi violence episodes were aimed at both the resistance fighters and at civilian populations. The rise of the underground resistance was not coordinated at the national level. Especially in the first period the nascent movement was formed by independent operating groups led by previously outlawed political parties or by former officers of the Royal Italian army. The first major act of resistance by the Italian population against the german occupation was in the city of Naples that was liberated by a chaotic popular rebellion. Nazi violence was rooted in racial expansionist ideology aiming at disrupting social dimension of the country and its geographical boarders. The research highlights the connection between the violence against defenceless civilians and the military strategic goals set by the German army in Italy. The violence to some extent was perpetrated against groups of resistance fighters, but more importantly it was aimed at the Italian population, exploiting women, children and economic resources, recurring to land desertification, roundups and deportations of civilians to labour camps. In numerous occasions the violence had a cleansing character, where German troops organised massacres of the civilians around the defensive and withdrawal lines. Although the numbers of casualties involved were not high, these massacres were characterised by intense violence witnessed by the civilian population. The character of the massacres was heterogeneous, involving rapes, murderers, torture, fires, explosions, deportations and robberies. In few occasions they were of particular violence for their victims, who involved children and women. Based on the time-line of these events, synthesised in Figure 2, we develop an identification strategy, accounting for the aforementioned exogenous events.

[2]The Winter (or Gustav) Line, though ultimately broken, effectively slowed the Allied advance for months between December 1943 and June 1944. Major battles in the assault on the Winter Line at Monte Cassino and Anzio alone resulted in 98,000 Allied casualties and 60,000 Axis casualties.

Figure 1: WWII fronts in Italy



Figure 2: Average in-utero province war victims per 100,000

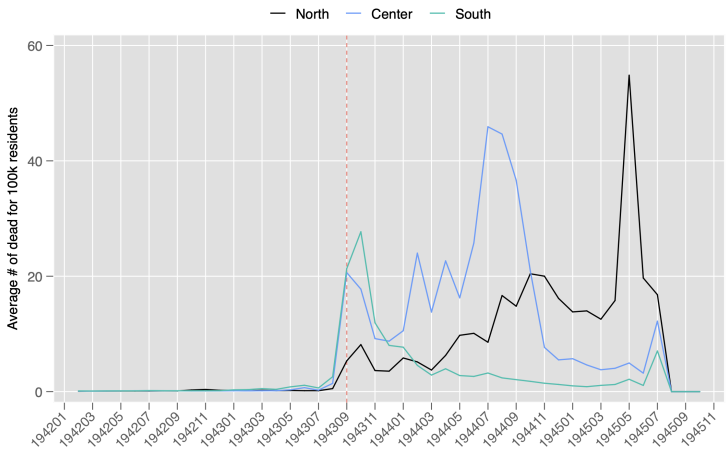
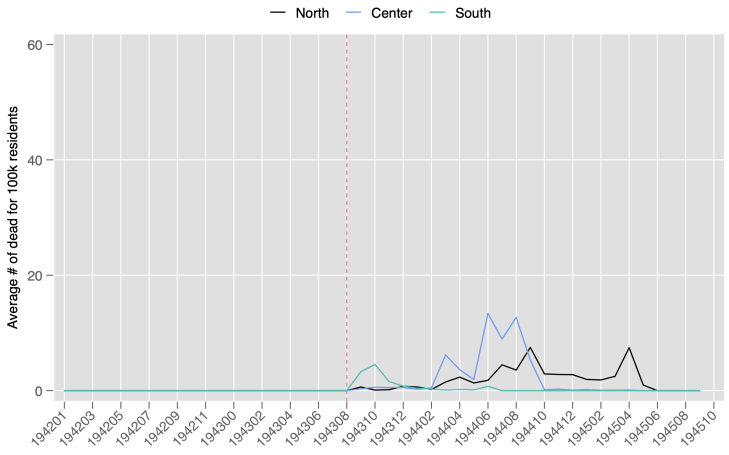


Figure 3: Average in-utero province nazi massacre victims per 100,000



3 Data

We assemble an individual-level dataset, combining individual administrative data on labour outcomes with historical data on the Nazi violence and on the intensity of war faced while in utero. This section introduces the data sources.

3.1 Nazi violence data

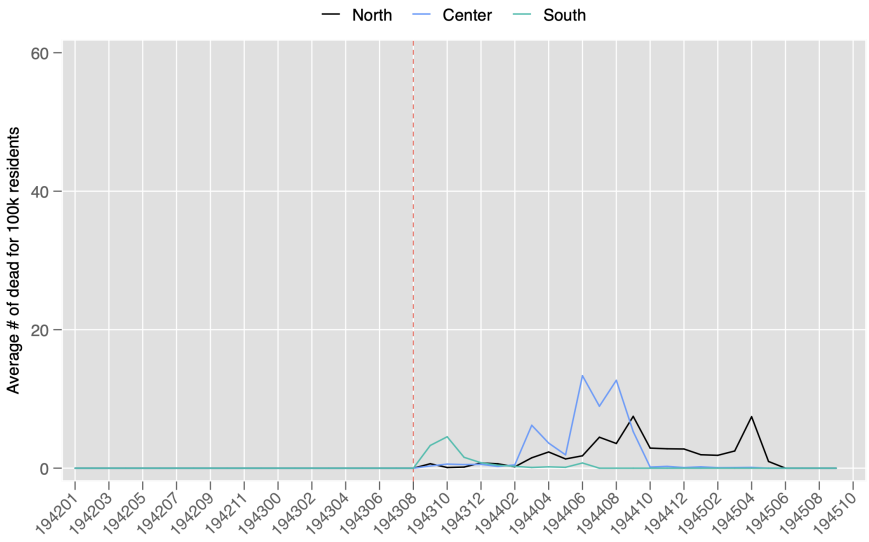
Following a joint historical Italian-German commission, a German-Italian Fund for the future' was created by the German Foreign Ministry. Under this fund, the INSMLI (National Institute for the History of the Italian Liberation Movement and the ANPI (National Association of Italian Partisans) promoted a project whose aim was to give a complete picture of the violence perpetrated against civilians by the German army and its allied Fascists in Italy between 1943 and 1945. The project gave rise to the Atlas of Nazi and fascist massacres, including a database and a wide collection of materials (documents, pictures, videos) on all the episodes.

The database lists and analyses all raids involving murders of civilians and resistance fighters killed in Italy after September 8, 1943 both by German soldiers and soldiers of the Italian Social Republic outside of the armed fights. These range from the first murders in the South to the withdrawal massacres committed in the days after the Liberation in Piedmont, Lombardy and Trentino Alto Adige region.

The chronological and geographical data processing resulted in an accurate reconstruction of the Nazi war in Italy, which finally connects modes, authors, times and places of the violence against the innocents all over the country. The database counts more then 5,000 episodes, describing the precise timing and place (day/municipality), the amount and type of victims (by age and gender), the violence type (type of murder, type of additional violence, such as robbery, property destruction, sexual violence) and the perpetrators type. [3] As before, the treatment is formulated by month/province of death of the victims, and is depicted in Figure 4. Overall, there were 20k victims distributed across 2000 municipalities, with an average size

[3]The information about the project, the database and access possibilities is available at `http://www.straginaziFasciste.it/?page_id=9&lang=en`.

Figure 4: Average province nazi massacre victims per 100,000



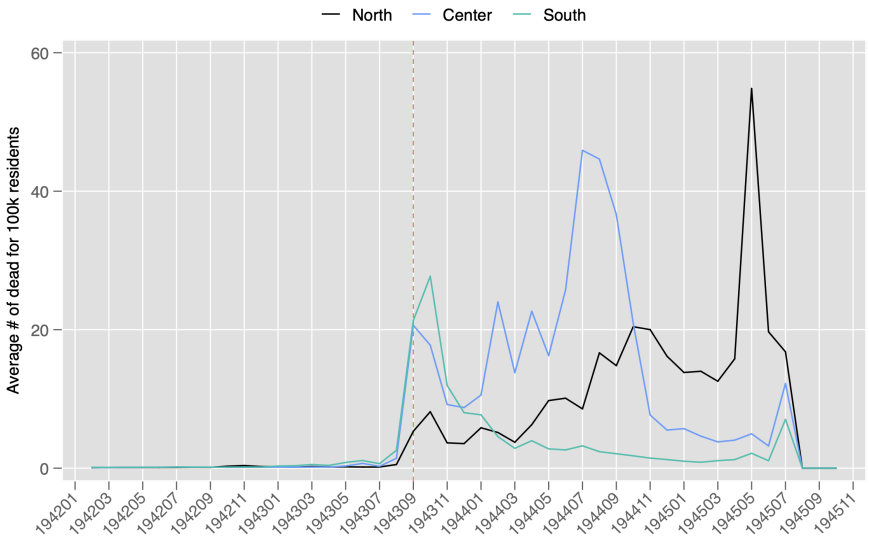
of 7 victims x episode.

In our analysis we concentrate on violence episodes occurring in smaller cities with resident population under 500 thousand. The nature of war in large cities was to some extent different then other places. On the one hand, raids in large cities had a more frequent and violent nature, hence excluding them should allows us also to net out possible physical harm. On the other hand, shock dispersion in large municipalities was substantial, hence the municipality level identification is less efficient in large urban centers. On top of that, large cities, due to political and strategical reasons, witnessed other adverse WWII conditions such as nutritional shortages, mass destruction and bombing. Finally, we additionally isolate events with high stress dose by focusing on violence whose victims were mainly women and children.

3.2 General war data

The general WWII intensity in Italy is obtained from the official publication "Morti e dispersi per cause belliche negli anni 1940-45" (The dead and the missing due to war causes between

Figure 5: Average province war victims per 100,000



1940-1945) ISTAT (1957), collected by the Italian National Institute of Statistics (ISTAT).[4] The data refer to the number of war victims, according to province and month of death.[5] Figure 5 provides a detailed description of the heterogeneous distribution of civilian casualties over time and space. There were around 200k dead and dispersed on the Italian territory.

In order to control for living conditions in the Italian provinces we use current data on average gross income from the Italian Ministry of Economics. We also account for the health conditions across Italian regions during war by collecting ISTAT data for two cause specific mortality rates (Mortality rates for pregnancy, delivery and puerperium, and Mortality rate for early childhood diseases). Finally, based on the data on the toll of war victims, for each combination of province and year/month of birth, we construct an indicator of early childhood exposure to war intensity in the first and second year of life from birth month in each province

[4]The publication is available and accessible either at the ISTAT archives, or online at `http://lipari.istat.it/digibib/causedimorte/IST3413mortiedispersipercausebellicheanni1940_45+OCRottimizz.pdf`.

[5]The analysis is framed in the todays administrative province division of the country, while ISTAT data report the intensity of war according to the historical administrative provinces. In the recent decades some provinces changed their structure by incorporating/separating some of their municipalities. We thus account for these changes and adopt a re-weighting procedure which follows the portions of population which change their province reference.

in order to net out the possible impact of war after the in-utero phase.

3.3 Longitudinal employer data

Data are provided by Istituto Nazionale della Previdenza Sociale (INPS), through the vistinps scholar programme, the largest social security and welfare institute in Italy and one of the most important on a European level. [6] It insures both private sector employees and self-employed workers and all companies operating in Italy are registered with INPS. Additionally, INPS provides both social security and welfare services.

INPS provides employer-employee matched data for the universe of Italian workers in the private sector (excluding agriculture) for a period that last from 1974 to 2018. To understand the dimension of the data, in 2015 INPS data records around 18 million of different jobs for a pool of 14 million workers in the private sector, out of the total of 22 million workers employed in 2015 in Italy (ISTAT, 2019). Moreover, it also provides information on the universe of Italian retirees and pensioners from 1995 onward, which allow to observe working careers that developed outside the private market. Additionally, the data contain information on disability pensions and other source of social benefits. Overall, for all beneficiaries, representing of the Italian population, INPS provides information on place and date of birth date, labour market entry date, and death date.

For all private employees, INPS data provide a longitudinal employment and earnings histories for workers from 1974 onward, together with information on their qualifications, types of contract, unemployment spells, sick leave periods and a range of additional information. It also provides municipality and exact date of birth which we use as a crosswalk for the WWII in utero exposure data. An important characteristics of the INPS data is its full time and space coverage for genuinely all non-public work contracts. As a result, the dataset allows us to track all the mobility of workers within Italy, independently of the employers. The only caveat of the data is the impossibility to distinguish individuals who earn zero earnings in any given year because of unemployment or because of their exit from the non-public labour

[6] The Institute has a budget (of approximately 400 billion Euro between receipts and payments) which is second only to state budget.

market force. We thus test indirectly if the treatment variable is correlated with out-of-private market labour force mobility, by examining whether individuals exposed at in utero stress are more likely to work in the public sector. We find no evidence in favor of differential mobility across sectors.

3.4 Data selection

Based on the historical events presented in Section 2, in our base specification, we we concentrate on males born in the war era (years 1942, 1943, 1944 and 1945). We focus on males only, since the female labour force representation among the Italian cohorts considered is much less represented (around 30% of the total labour force) and is distributed much more heterogeneously across the Italian territory. We then additionally restrict this sample to individuals born between January 1943 and May 1944, all conceived before the event of Armistice. The conception time delimited to the pre-armistice period allows us to compare individuals whose parents had the same prior about the pregnancy evolution, but whose actual pregnancy evolution, due to specific time-space heterogeneity, exposed them at different levels of stress.

As already mentioned, we concentrate on municipalities with teh number of residents lower then 500 thousands (by todays' population counts), which due to political reasons, repeated violence episodes, food shortages and shock dispersion witnessed confounding and multiple adversities in teh war era. In a set of robustness checks, we additionally limit the upper limit imposed on the sie of municipalities to 250 thousand.

We determine the intrauterine period for each male individual observed in the INPS data by counting 9 months backwards from their month/year of birth. Following the same approach, we additionally single out each of the three gestational trimesters [7]

[7]It is well known that the hypothesis of 9-month-pregnancies induces measurement errors in case of premature births, as emphasized by Currie and Rossin-Slater (2013). In case of shorter pregnancies, the actual exposure to treatment is lower, and the resulting estimate of treatment likely to be biased, since the overestimation of exposure period is likely to be correlated with higher probability of negative health outcomes typical of the preterm babies. In general, preterm pregnancies in our historical setting are likely to represent a narrower issue, since premature live births were rare during the WWII times. According to Kopp and Krakow (1983) early follow up studies done between 1920 and 1949 show that child weight at birth is the main parameter to consider for survivorship. In fact, until the WWII neonatology was not able to save pre-term babies weighting less than 1.8 kg, which was usually achieved not before the 31st week of pregnancy.

Moreover, for each individual we trace his/her municipality of birth.[8] Within the Nazi violence data we create an indicator dummy variable for the occurrence of at least one Nazi violence episode in the 9 months prior to birth for each year/month and municipality combination. We thus assign the dummy variable to each individual in the INPS data according to the municipality and birth date combination. We then create a similar variable for the general WWII intensity data, which in this case is a continuous variable describing the amount of war victims accumulated in the 9 months (or in each trimester) prior to the month of birth in each province. In this case the match with the INPS labour data is done on month and province of birth of each individual.

3.5 Outcome variables

Within INPS data, we create a set of labour-market outcomes: log annual earnings[9], weeks worked and blue collar status. Each outcome is computed at the moment of first employment, at the moment of last employment as well as at,distinct ages a: $a = 30$, 40, 50 and 60. For each a we construct the outcomes as the average (or median) of a, $a-1$ and and $a+1$ to minimize variance in the earnings distribution. In the preferred specification the missing records are kept at missing. For each individual, we additionally compute age at first employment, age at pension, the first amount of work pension, death event and occurrence of disabilities.

In terms of pensions we additionally study all retirement pensions, to both public and private labour market retirees. Moreover, the Italian pension scheme applicable to cohorts selected in our study is such that our workers retired receiving 80% of their last retribution received before retirement. The first pension allows us thus to analyze a synthetic measure of their working career result. Finally, in order to have a hint of the mechanism through which the labour market penalization operates we also study the disability status.

Finally, we also identify episodes of contract termination as a result of a mass layoff. We reconstruct all layoff episodes, by identifying a layoff in case of firms where $TotEmpl > 25$

[8]Italy is currently organised on 8090 municipalities, which represent the third-level administrative subdivision of 110 provinces of 20 regions.

[9]Earnings of each individual are expressed in 2005 euro, adjusted for inflation using the CPI index.

workers, and $TotEmpl_t/TotEmpl_{t-1} <.7$. We then match firm layoff episodes with individual contract termination between 1983 - 1998.

3.6 Descriptive statistics

The table refers to the pool of all private sector workers who over the period between 1974 and 2017 have been at least once insured with INPS. The statistics on the workers are computed as of 2017 and refer also to individuals who in the period between 1974-2017 deceased or left the non-public labour market. We observe each individual over an average of 15-year period of time, with a maximum of 42 years. The overall sample consists of 772,131 individuals, whoch we subsequently restrict 696,663 individuals, referring to those born in cities with the population size lower than 500 thousand residents. The sample size is the smallest for the selection relative to individuals present in the private sector data at the age of 30 and the age of 60. Nazi violence during the prenatal era struct 13% of the individuals, who on average saw 58 deaths per 100 thousand inhabitants in the province of residence of birth during their in-utero phase. In 2017, hence on average at the age of 73, 24% of the individuals were dead. Their real earnings grew from 15,679 to 18,676 euro, between the first and the last moment ween in the data. It peaked at the age of 50, reaching an average of 27,168 euro. At the market entry, 75% of them were blue collar, which decreased to 67% at the moment of market exit. On average, they retired at the age of 49, with their first monthly pension of 1,212 euro.

Table 1: Descriptive statistics - cohorts born between 1942-1945.

Variable	Mean	(Std. Dev.)	Min.	Max.	N
no. of years uniemens	15.37	(9.94)	1	42	772131
entry earnings	15678.55	(10992.82)	706.03	303961.84	772131
exit earnings	18675.52	(19215.01)	706.03	379860.53	772131
age 30 earnings	14183.97	(7791.04)	706.03	81240.10	368852
age 40 earnings	20382.47	(10547.39)	1285.37	158946.58	509906
age 50 earnings	27168.37	(18999.53)	2019.76	247775.91	428937
age 60 earnings	25404.52	(25267.1)	1130	379860.53	138405
Nazi violence	0.13	(0.34)	0	1	772131
WWII deaths	57.95	(85.55)	0	847.07	772131
death	0.24	(0.43)	0	1	772131
death year	2006.57	(8.27)	1977	2017	184034
year start work	1964.54	(8.05)	1959	2016	771863
entry blue collar	0.75	(0.44)	0	1	772131
exit blue collar	0.67	(0.47)	0	1	772131
blue collar at 30	0.72	(0.45)	0	1	368852
blue collar at 40	0.68	(0.46)	0	1	509906
blue collar at 50	0.66	(0.47)	0	1	428937
blue collar at 60	0.66	(0.47)	0	1	138405
disability	0.11	(0.31)	0	1	772131
work disability	0.09	(0.28)	0	1	772131
first pension	1212.37	(895.27)	340	62738	622354
age at retirement	48.79	(65.57)	34	75	627165

4 Identification Strategy

Our empirical identification strategy exploits the variation in type, space and time of the adversities of WWII over several months and across Italian municipalities. Since conflicts onset struck in September 1943, cohorts born just months apart experienced markedly different in-utero conditions. Nazi violence incidence varied widely and idiosyncratically across municipalities, hence similar pregnant mothers experienced strikingly different environments while pregnant. We use both cohort and geographic variation differences in the prenatal exposure to identify the differences in fetal exposure to stress. We restrict the sample to individuals born within a 9 month window around the Armistice (01.1943-05.1944) in order to take advantage of unexpectedness of the increase in prenatal insults immediately after the armistice onset.

We employ a generalized difference-in-differences strategy, taking advantage of the unexpected outbreak of violent raids and war events occurred in heterogeneous timing after the Italian armistice from September 8^{th} 1943 onwards. This exogenous source of variation represents an unambiguous discontinuity in the evolution of the prenatal era circumstances that breaks the distribution of WWII intensity over time (months) and across space (municipalities).

We thus build the following baseline reduced form, evaluating whether the war adversities have likely caused maternal stress, which, in turn, can be shown to have affected the long-term health outcomes of the children:

$$y_{icpt} = \beta_0 + \beta_1 Nazi_{ct} + \beta_2 War_{pt} + \alpha_c + \gamma_t + \epsilon_{icpt} \tag{1}$$

where y_{icpt} represents the individual labour outcomes studied, $Nazi_{ct}$ is defined as an indicator dummy variable for individuals born in municipalities which had an episode of violence in the 9 months prior to the month of birth, War_{pt} is defined as a continuous measure of the number of war deaths in the province of birth per 1,000 residents in the 9 months prior to the month of birth. We include municipality fixed effects and time (year x month) fixed effects. As

customary, ϵ_{icpt} is a municipality clustered standard error term.

We also test for differential impact of exposure during different stages of gestation. According to (xxxx ADD REFERENCES) the central nervous system structures are formed in the first trimester of pregnancy, hence changes underlying mental illness, and later labour related outcomes are likely to occur in this period (Schulz, 2010). Moreover, violence stress is frequently found to give rise to post traumatic stress disorder (PTS) (Bhalotra et al., 2012) and strong anxiety feelings persist as long as 12 months. Therefore, first trimester is likely to alter the course of entire pregnancy. To gauge the trimester specific exposure, we augment the previous model specification by each pregnancy subinterval term in the following manner:

$$\begin{aligned} y_{icpt} =& \beta_0 + \beta_1 Nazi_{1,ct} + \beta_2 Nazi_{2,ct} + \beta_3 Nazi_{3,ct} + \\ & \beta_4 War_{1,pt} + \beta_5 War_{2,pt} + \beta_6 War_{3,pt} + \alpha_c + \gamma_t + \epsilon_{icpt} \end{aligned} \tag{2}$$

As largely discussed in the previous pages, WWII does not provide randomization in strict causal inference terms, but delivers a setting that can be considered satisfactory for the causal interpretation of the conflict intensity parameter. The war events exploited in this study evolved in an unpredictable way, with the royal family running away from Rome a day after the armistice proclamation being an important representation of the information asymmetry about the war events that civilians were exposed to. It is thus reasonable to consider local adverse conditions as exogenously assigned to the civilians, after controlling for time invariant heterogeneity at municipality level, and the inclusion of time-characteristics based on month/year fixed effects.

5 Identification issues and solutions

5.1 Is it stress?

In order to provide a clear interpretation of the evidence of our study, we need to pin down the stress channel through which we deem Nazi violence had impact on pregnant mothers. To

exclude that violence affected individuals through other channel rather than stress, we employ an external data source on clinical information regarding the impact of Nazi violence exposure in-utero. We use Health Search data, a nation wide representative sample of GP records on all their patients, diagnosis and prescriptions. Chronic patients refill their prescriptions at the GP practices and ATC (Anatomic Therapeutic Chemical classification) codes of drugs allow us to track organ systems that the drugs are issued for. We thus analyze different types of health expenditure in relation to Nazi violence in-utero exposure, and find that the only type of health expenditure on which we observe the the impact of Nazi raids pertains to the nervous system and mental disease.

Table 2: Effect of prenatal exposure to Nazi rides on distinct health expenditure type.

	Mental	Cardiovascular	Respiratory	Skin
Nazi violence	35.67751**	-1.30074	4.470346	1.996955
	3.01	-.055	.42	.88

Notes: *** p<0.01, ** p<0.05, * p<0.1, province and year/month fixed effects included, standard errors clustered at province level,

5.2 Selection issues at conception, in utero and in later life

Another quintessential caveat in the estimation of exposure to early life shocks on long term outcomes is selection. There are three phases of selection: conception, in-utero and later life. While we provide a set of estimates for the overall pool of all war exposed individuals (born in 1942, 1943, 1944 and 1945), in order to handle the first type of selection, our preferred model specification pertains to the 9-month window around the conflicts onset (January 1943 - May 1944). This sample selection allows us to deal with individuals all conceived before the Armistice. Individuals are likely to adjust their fertility choices to the underlying uncertainty about the future. Mothers may respond to the shock by deciding not to get pregnant or having difficulties getting pregnant. On the one hand, it is likely that mothers who decide to get pregnant in war times feature on average lower socio-economic status, which is also likely to determine worse childhood conditions, hence poorer health in the long-term and an overestimation of the causal effect. On the other hand, it might be that only families with

better socio-economic status decide to have children, being able to buffer the adverse effect of war. It is important to highlight that in our setting socio-economic characteristics, for which we cannot control, affect only the choice of having a baby and not the chance to get treated, which remains exogenous. In such case fertility choices based on the socioeconomic status would entail better long-term health outcomes among the treated, and an underestimation of the causal effect. Our identification strategy tries to downplay the importance of this issue by subsampling individuals born in a 9-month pre/post window, between January 1943 and May 1944, all conceived before the materialization of any sign of upcoming war on the national territories, between April 1942 and August 1943. This sample selection allows us to make a reasonable claim that *foetuses* in the two subgroups were conceived in a fairly similar environment as far as the lack of war is concerned, hence the fertility choice concerning their conception was made according to similar uncertainty priors.

Secondly, to deal with in-utero selection, we would ideally need birth records at the time of exposure, which are not available. We however retrieve regional level data on cause specific mortality for complications of pregnancy.

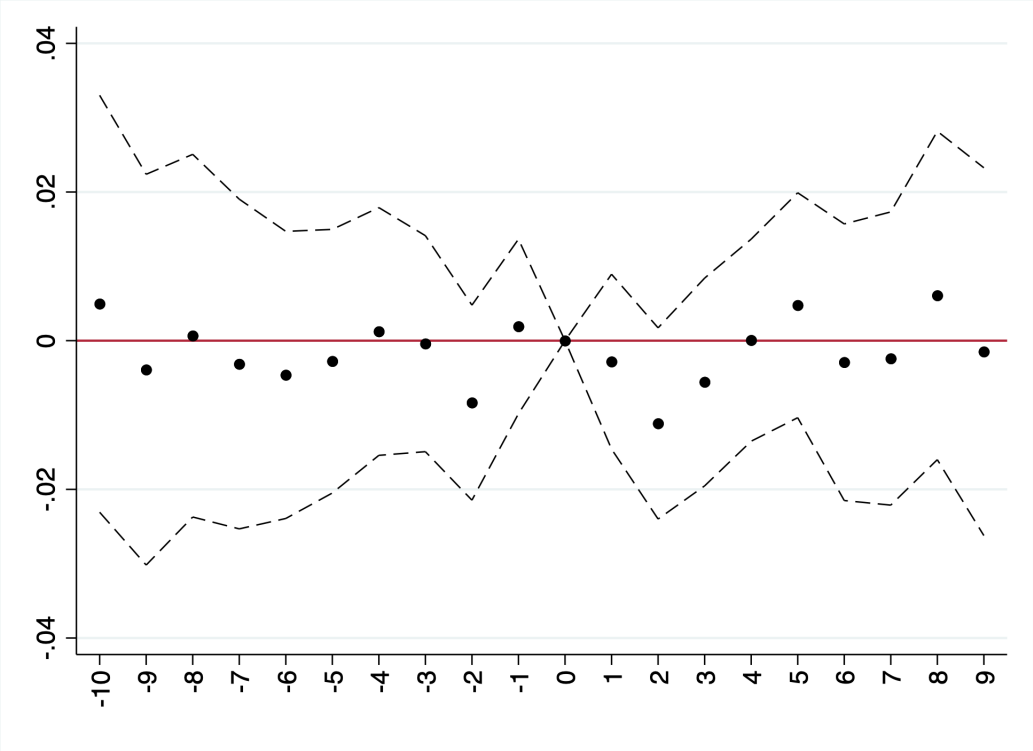
Table 3: Effect of WWII intensity and Nazi rides on mortality rate from pregnancy complications.

	mortality rate pregnancy complications
no. nazi massacres	-.00092
	(-0.21)
no. of WWII battlefront	.0247***
victims	(2.74)
r2	0.45
N	133

Notes: *** p<0.01, ** p<0.05, * p<0.1, controls include region and year fixed effects, standard errors clustered at region level,

While mortality due to pregnancy complications increases with WWII intensity, the effect of Nazi violence incidence is null.

For later life selection, we conduct a check for cohort composition and for attrition in INPS data. Our causal claims hinge on the capacity of our empirical analysis to rule out, or control for, the effect of selection of survivors in the treated sample. War events are likely to increment the likelihood of infertility, miscarriages, stillbirths or simply higher overall mortality, which



may distort the characteristics of the individuals in the treated sample (those exposed to war stress). Moreover, by analyzing the long-term impacts of war on the elderly, we are likely to be exposed at an even greater amount of survival distortion. Notwithstanding, the direction of this bias leads to an underestimation of health differential, due to the selection process occurring both during early and adult life, where the overall sample, and the treated individuals in particular, are expected to be on average healthier. We estimate an event study type specification, where we relate the probability of death of the timing of exposure to Nazi violence. The figure below is constructed alineating all Nazi violence episodes on a homogenous time scale testing for lags and leads effects, controlling for municipality and year/month fixed effects. The horizontal axis describes the distance in months between the first municipality level Nazi ride event and individual birth month, where individuals in period 0 experience the Nazi violence at the moment of birth, those in period -9 are born 9 months before the exposure, while those at 9 are born 9 months after the event, hence in the first month after conception. We find no effect of exposure to Nazi violence at any stage of early life on mortality.

We additionally check for selection and attrition, by checking for cohort composition size

within workers data at various age spells. We find no effect of on the composition of month cohorts according to the exposure timing.

Table 4: Event study of the municipality/month cohort sizes with respect to the first municipality level Nazi ride episode.

	in uni30	in uni40	in uni50	empl30	empl40	empl50	alive	dead40	dead50
t-7	-.02735	-.02749	-.06866	-.08539	-.08561	-.12515	-.18968	-.00773	.02592
	-.28777	-.28747	-.27646	-.40486	-.40646	-.42494	-.64284	-.00863	-.03993
t-6	.11522	.11744	.07658	.1102	.10734	.0738	.03177	-.00324	.02433
	-.26392	-.26369	-.25503	-.36824	-.36974	-.38315	-.57154	-.00832	-.03405
t-5	-.24098	-.24116	-.26413	-.25395	-.25813	-.28103	-.38738	.00064	.01174
	-.19808	-.19796	-.19159	-.28727	-.28868	-.30116	-.44675	-.00888	-.03038
t-4	.06128	.06248	.00622	.07924	.08128	.02842	-.03365	-.0052	.0336
	-.18875	-.18866	-.18162	-.24855	-.24979	-.25629	-.37347	-.00703	-.02527
t-3	.09898	.10354	.08619	.18068	.18204	.16	.17503	-.00505	.00599
	-.14283	-.14273	-.13941	-.1901	-.19099	-.1953	-.27936	-.00675	-.02061
t-2	.11248	.1178	.1097	.07077	.0727	.06164	.14466	-.00279	.01221
	-.14597	-.14569	-.14178	-.18151	-.18298	-.18039	-.23559	-.00699	-.0174
t	-.00525	-.00334	-.0012	.00288	-.00098	-.01167	-.0482	.00236	-.00076
	-.10238	-.10238	-.10063	-.11968	-.12042	-.12175	-.15955	-.00641	-.0164
t+1	.06281	.06241	.05174	.13046	.1248	.10973	.1093	.01051	.0206
	-.13544	-.1351	-.13063	-.16792	-.16821	-.17186	-.23772	-.00802	-.02051
t+2	.18641	.19234	.19028	.19373	.19711	.17865	.24301	.011	.02671
	-.17037	-.1704	-.16289	-.22727	-.22837	-.23441	-.34625	-.00772	-.02485
t+3	.0339	.03682	.0558	.04898	.04569	.04981	.08336	.01044	.00217
	-.20472	-.20478	-.19768	-.27089	-.27206	-.28483	-.42052	-.0084	-.0294
t+4	0.69901	0.69719	0.70412	0.94037	0.93393	0.91570	1.19221	0.01585	.02668
	-.3775	-.3773	-.36619	-.51108	-.51269	-.51678	-.72085	-.0089	-.03655
t+5	.38989	.38892	.41075	.53762	.53937	.54226	.65412	.01076	-.00001
	-.31836	-.31789	-.30678	-.41704	-.4188	-.43618	-.6383	-.01016	-.04037
t+6	.45144	.45353	.48074	.53515	.53303	.5339	.60028	.01298	-.00238
	-.37677	-.37665	-.36325	-.50136	-.50322	-.52173	-.77027	-.01038	-.0454
t+7	.26007	.25802	.29562	.31917	.31075	.31943	.43406	0.02239	-.00045
	-.37843	-.37799	-.36166	-.52181	-.52408	-.54862	-.83786	-.01077	-.05114
	-.80409	-.8038	-.76553	-1.15181	-1.15754	-1.22292	-1.89806	-.02133	-.11496
Obs.	25915	25915	25915	25915	25915	25915	25915	25915	25915

Notes: *** p<0.01, ** p<0.05, * p<0.1, municipality and year/month fixed effects included, standard errors clustered at municipality level,

5.3 Endogenous mobility and WWII events

Another issue we need to confront our setting with is endogenous mobility. As also explained in 2 section, the sudden and unexpected changes in war events and the nature of war described in the historical section rule out, to a reasonable extent, the issue of endogenous maternal mobility, hence also the possible issue of miss-classification in treatment. On the one hand, as discussed in Section 2, movements across Italian provinces during WWII were very limited due to the military strategies and to lack of consistent evacuation plans (Baldoli and Knapp 2012) Baldoli and Knapp (2012). Moreover, if any, the bias is again expected to underestimate the causal impact, given that the true control group (individuals whose mothers resided in low war adversity provinces both during pregnancy and at birth) is likely to be contaminated with treated individuals (individuals whose mothers resided in high war adversity provinces and at some point of the pregnancy moved to safer provinces). In fact, assigning individuals erroneously to a treatment according to the birthplace represents a typical case of misclassification in treatment, which leads to an underestimation (attenuation bias) of the treatment

effect (Lewbel, 2007). In our setting this bias is likely to increase with the length of the exposure to treatment, which we approximate on the basis of the information about municipality and month of birth.

Finally, in order to make sure that our results refer to Nazi violence only, we additionally provide estimates for individuals who were exposed at Nazi violence during their prenatal era, but not exposed at harsh WWII battlefront events. Moreover, since war may be responsible for the deterioration of living conditions straight after the actual events take place, it is important to disentangle the gestation exposure period from the very early childhood circumstances. In order to exclude possible confounding effects of early childhood exposure on top of Nazi violence, we estimate our model specification on individuals who were exposed at Nazi violence only in their prenatal era, with no subsequent violence of intense WWII events registered in their early childhood. Finally, we also isolate the most harsh violence episodes by concentrating on the sole Massacres which saw as victims children and women, representing high stress dose events.

6 Results

In this section we present the results obtained from the kep specification discussed before as well as from the alternative models. The outcomes examined are age at market entry (expressed in months), log earnings, blue collar status, managerial status, disability status, unemployment status, log first work pension, and death. The outcomes are examined at different moments of working career, namely the first and the last period when observed on the labour market, and at the age of 30, 40, 50 and 60, as well as at the moment of first work pension payment. While the information on outcomes relative to first and the last moment at the labour market is available for all individuals, age specific spells are characterized by a varying sample size. The sample size is the largest at the intersection relative to the age of 40, with a narrower samples seen at the age of 30 and 50, and the smallest number of individuals registered when 60.

6.1 Labour market entry

Results in the "wide" specification refer to the full sample, hence all individuals born in 1942, 1943, 1944 and 1945), while the "narrow" sample consists of individuals born in the 9-month-window before and after the conflicts outbreak. Other specifications are relative to alternative sample selection within the narrow window, where we: 1) exclude individuals exposed at Nazi violence in early childhood (first year of life), 2) exclude individuals exposed at intense WWII battlefront events during the in-utero phase, 3) single out only the extreme Nazi violence, where the victims of raids included children and women. As explained, each model specification includes municipality and month/year fixed effects, and controls for the overall WWII victims toll specific to each prenatal period in each province of individual birth. The standard errors are clustered at the municipality level.

For blue collar status and first earnings, we find greater magnitudes of penalties related to stressful events suffered in the narrow window, implying that a positive selective fertility at conception might have occurred, e.g. individuals conceived during the war era and born alive were on average endowed with better health or economic conditions, which in turn mitigated to a higher extent the adverse effects of in-utero exposure to stress. We also find a greater penalty associated with high dose of stress. In terms of the trimester specific analysis, we find that the first trimester exposure is associated with greater penalties, although the differences between them are narrow, and tend not to be statistically significant.

Table 5: Labour market entry outcomes.

	age entry	blue collar	log earnings
	wide window		
nazi massacre in	-9.746*	0.011***	-0.017***
9mths (municipality)	(-1.72)	(3.64)	(-4.73)
r2	0.011	0.066	0.106
N	696663	696663	696663
	narrow window		
nazi massacre in	-9.521*	0.014***	-0.021***
9mths (municipality)	(-0.67)	(3.13)	(-3.02)
r2	0.025	0.080	0.118
N	298510	298510	298510
	narrow window no early childhood violence		
nazi massacre in	1.218	0.019***	-0.019**
9mths (municipality)	(0.06)	(3.78)	(-2.16)
r2	0.031	0.082	0.124
N	237034	237034	237034
	narrow window no intense WWII in-utero		
nazi massacre in	-21.981	0.012*	-0.019**
9mths (municipality)	(-1.29)	(1.65)	(-2.11)
r2	0.036	0.093	0.131
N	207180	207180	207180
	narrow window women/children violence		
nazi massacre in	-4.532	0.024***	-0.024**
9mths (municipality)	(-0.17)	(3.07)	(-2.24)
r2	0.025	0.080	0.118
N	298510	298510	298510

Notes: *** p<0.01, ** p<0.05, * p<0.1, controls include WWII battlefront intensity, province and year/month fixed effects, standard errors clustered at province level,

Table 6: Labour market entry outcomes - by trimester analysis.

	age start work	blue collar entry	first earnings
	wide window		
nazi massacre in 1st	-14.457**	0.011**	-0.015***
trim (municipality)	(-2.34)	(2.21)	(-2.92)
nazi massacre in 2st	-7.900	0.005*	-0.014***
trim (municipality)	(-1.10)	(2.01)	(-3.58)
nazi massacre in 3st	5.657	0.008**	-0.013***
trim (municipality)	(0.87)	(2.41)	(-3.45)
r2	0.012	0.069	0.110
N	696660	696660	696660
	narrow window		
nazi massacre in 1st	-32.196**	0.018***	-0.027***
trim (municipality)	(-2.20)	(3.50)	(-3.00)
nazi massacre in 2st	1.553	0.015**	-0.023***
trim (municipality)	(0.13)	(2.22)	(-4.62)
nazi massacre in 3st	-2.699	0.014**	-0.017*
trim (municipality)	(-0.21)	(2.18)	(-1.99)
r2	0.025	0.080	0.119
N	298510	298510	298510

Notes: *** p<0.01, ** p<0.05, * p<0.1, controls include WWII battlefront intensity, province and year/month fixed effects, standard errors clustered at province level,

6.2 Labour market exit

Table 7: Labour market exit outcomes.

	exit blue	exit managerial	exit wage	unemployment
		wide window		
nazi massacre in	0.014***	-0.007***	-0.026***	0.000
9mths (municipality)	(4.76)	(-3.97)	(-5.10)	(0.20)
r2	0.073	0.035	0.076	0.048
N	696663	696663	696663	696663
		narrow window		
nazi massacre in	0.018***	-0.010***	-0.034***	0.004
9mths (municipality)	(3.90)	(-4.46)	(-3.44)	(1.50)
r2	0.088	0.051	0.091	0.062
N	298510	298510	298510	298510
		narrow window no early childhood violence		
nazi massacre in	0.021***	-0.013***	-0.040**	0.007
9mths (municipality)	(4.06)	(-4.63)	(-2.65)	(1.54)
r2	0.089	0.058	0.097	0.066
N	237034	237034	237034	237034
		narrow window no intense WWII in-utero		
nazi massacre in	0.020***	-0.010***	-0.031**	0.004
9mths (municipality)	(2.99)	(-2.83)	(-2.28)	(0.71)
r2	0.099	0.062	0.105	0.071
N	195412	195412	195412	195412

Notes: *** p<0.01, ** p<0.05, * p<0.1, controls include WWII battlefront intensity, province and year/month fixed effects, standard errors clustered at province level,

Table 8: Labour market exit outcomes - by trimester analysis.

	exit blue	exit managerial	exit wage	unemployment
		wide window		
nazi massacre in 1st	0.012***	-0.010***	-0.020***	-0.001
trim (municipality)	(3.24)	(-3.63)	(-3.65)	(-0.22)
nazi massacre in 2st	0.009**	-0.004**	-0.012*	-0.002
trim (municipality)	(2.61)	(-2.12)	(-1.81)	(-0.75)
nazi massacre in 3st	0.012***	-0.004**	-0.021***	0.001
trim (municipality)	(3.74)	(-2.21)	(-3.74)	(0.49)
r2	0.073	0.035	0.076	0.048
N	696663	696663	696663	696663
		narrow window		
nazi massacre in 1st	0.022***	-0.012***	-0.031**	0.002
trim (municipality)	(3.42)	(-3.59)	(-2.82)	(0.27)
nazi massacre in 2st	0.017***	-0.013***	-0.044***	-0.008*
trim (municipality)	(3.45)	(-5.03)	(-3.82)	(-1.98)
nazi massacre in 3st	0.020***	-0.009***	-0.030***	0.008**
trim (municipality)	(3.97)	(-3.53)	(-3.66)	(2.62)
r2	0.088	0.051	0.091	0.062
N	298510	298510	298510	298510

Notes: *** p<0.01, ** p<0.05, * p<0.1, controls include WWII battlefront intensity, province and year/month fixed effects, standard errors clustered at province level,

6.3 Age specific outcomes

Table 9: Labour market outcomes at the age of 30, 40, 50 and 60.

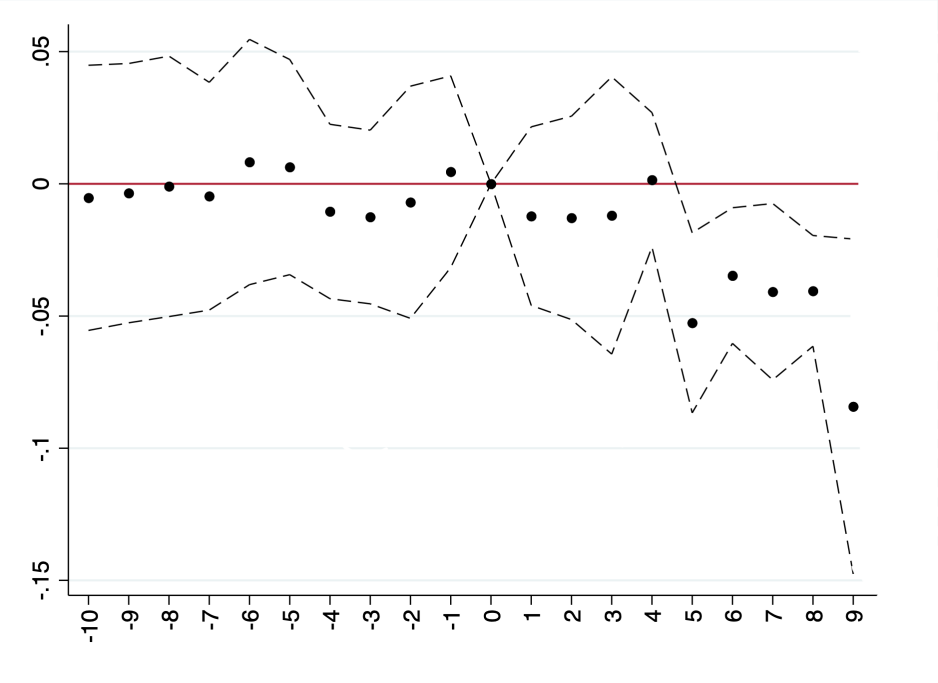
	earnings30	earnings40	earnings50	earnings60
		wide window		
nazi massacre in	0.004	-0.016***	-0.022***	-0.041***
9mths (municipality)	(0.67)	(-4.60)	(-4.36)	(-3.78)
r2	0.140	0.118	0.111	0.122
N	335464	460534	390549	121185
		narrow window		
nazi massacre in	0.008	-0.011	-0.028***	-0.055**
9mths (municipality)	(0.91)	(-1.52)	(-3.07)	(-2.92)
r2	0.167	0.137	0.133	0.166
N	188750	196521	166024	50221
		narrow window no early childhood violence		
nazi massacre in	0.005	-0.019	-0.045**	-0.066**
9mths (municipality)	(0.35)	(-1.48)	(-2.72)	(-2.39)
r2	0.176	0.144	0.142	0.178
N	145158	154972	130243	40749
		narrow window no intense WWII in-utero		
nazi massacre in	0.008	-0.016	-0.033***	-0.058**
9mths (municipality)	(0.57)	(-1.46)	(-2.58)	(-2.57)
r2	0.188	0.154	0.150	0.183
N	123529	128434	107333	32575

Notes: *** p<0.01, ** p<0.05, * p<0.1, controls include WWII battlefront intensity, province and year/month fixed effects, standard errors clustered at province level,

Table 10: Labour market outcomes at the age of 30, 40, 50 and 60 - by trimester analysis.

	earnings30	earnings40	earnings50	earnings60
		wide window		
nazi massacre in 1st	0.011*	-0.007	-0.018***	-0.013
trim (municipality)	(1.70)	(-1.26)	(-3.20)	(-0.73)
nazi massacre in 2st	-0.007	-0.016***	-0.020***	-0.049***
trim (municipality)	(-1.07)	(-3.02)	(-3.86)	(-4.46)
nazi massacre in 3st	-0.003	-0.010***	-0.010*	-0.024**
trim (municipality)	(-0.66)	(-3.08)	(-1.90)	(-2.07)
r2	0.144	0.126	0.122	0.130
N	335462	460531	390543	121179
		narrow window		
nazi massacre in 1st	0.009	-0.001	-0.035***	-0.127***
trim (municipality)	(0.78)	(-0.05)	(-6.73)	(-3.11)
nazi massacre in 2st	-0.013	-0.031***	-0.044***	-0.097***
trim (municipality)	(-1.11)	(-2.93)	(-4.73)	(-4.55)
nazi massacre in 3st	0.004	-0.003	-0.015	-0.034*
trim (municipality)	(0.44)	(-0.42)	(-1.43)	(-1.95)
r2	0.167	0.137	0.133	0.166
r2	0.144	0.126	0.122	0.130
N	188750	196521	166024	50221

Notes: *** p<0.01, ** p<0.05, * p<0.1, controls include WWII battlefront intensity, province and year/month fixed effects, standard errors clustered at province level,



We also estimate an event study type specification, where we relate the log earnings at the age of 50 to Nazi violence. The figure again is constructed alineating all Nazi violence episodes on a homogenous time scale testing for lags and leads effects, controlling for municipality and year/month fixed effects. The horizontal axis describes the distance in months between the first municipality level Nazi ride event and individual birth month, where individuals in period 0 experience the Nazi violence at the moment of birth, those in period -9 are born 9 months before the exposure, while those at 9 are born 9 months after the event, hence in the first month after conception. By construction, the estimates are relative to the sole exposed individuals, exploiting the time variation in exposure. It is however not directly comparable to the estimates presented in the table, which instead offer evidence relative to equation 2, where both exposed and non exposed individuals are included.

6.4 Additional outcomes

Disability refers to a status granted upon an INPS evaluation and subject to benefits. Work disability refers to a status acquired on the job. First pension refers to the amount paid by INPS as the first monthly work pension.

Table 11: Other labour market outcomes.

	disability	work disab	first pension (logs)
	wide window		
nazi massacre in	0.000	0.003*	-0.020***
9mths (municipality)	(0.28)	(1.97)	(-4.27)
r2	0.027	0.029	0.040
N	696663	696663	566993
	narrow window		
nazi massacre in	0.002	0.002	-0.021**
9mths (municipality)	(0.89)	(0.57)	(-2.45)
r2	0.040	0.041	0.054
N	298510	298510	242528
	narrow window no early childhood violence		
nazi massacre in	0.007**	0.001	-0.035**
9mths (municipality)	(2.13)	(0.18)	(-2.59)
r2	0.044	0.046	0.064
N	237034	237034	190460
	narrow window no intense WWII in-utero		
nazi massacre in	0.003	-0.003	-0.015
9mths (municipality)	(0.82)	(-0.69)	(-1.21)
r2	0.048	0.051	0.067
N	195412	195412	157107

Notes: *** p<0.01, ** p<0.05, * p<0.1, controls include WWII battlefront intensity, province and year/month fixed effects, standard errors clustered at province level,

6.5 What if lightening strikes twice

In this section we exploit a second random shock, a mass layoff episode, as a subsequent mediator of the early-life shocks. We identify episodes of contract termination as a result of a mass layoff. We rely on a standard layoff definition identifying a layoff episode as a reduction in total employment in firms larger than 25 workers in period t, by 30% with respect to period $t-1$. We then match firm layoff episodes with individual contract terminations in years between 1983 and 1998.

We first check if layoffs are orthogonal to Nazi violence and find no effect of Nazi stress exposure in utero on the probability of working in a firm which exercises a mass layoff.

Table 12: Effect of mass layoff on probability of working in the following period.

	positive earnings
nazi massacre in	-0.001
9mths (municipality)	(-1.56)
mass layoff	-0.190***
	(-41.50)
nazi ×	-0.031**
layoff	(-2.06)
r2	0.045
N	2548353

Notes: *** p<0.01, ** p<0.05, * p<0.1, controls include WWII battlefront intensity, province and year/month fixed effects, standard errors clustered at province level.

We then show in Table 12 that overall, individuals exposed at a layoff episode have a 20% reduction in probability of being employed in the period following the layoff. On top of that, those who underwent stress in the prenatal era, have an additional 3 % penalty. This finding is likely to shed light on why the earnings differential between the exposed and non exposed strengthens in time. Beyond confirming the robustness to the overall reading of the results presented in this study, this piece of evidence suggests that the gap between the

advantaged and the disadvantaged in utero, represents a permanent scar. The scar in case of an additional negative mediator effect such as career disruption, extends additionally the penalty and opening up even more the difference in outcomes.

6.6 Other evidence

We check for treatment effects in selection into private vs. public sector. Based on the source of last pension contributions, we assign each INPS beneficiary into private sector, public sector or self-employment. We find no patterns of selection into specific sectors following the in-utero exposure. Pooling together all pension types (public, private and self-employment), we find similar penalties for the exposed individuals as in the UNIEMENS sample. Similar to the UNIEMENS workers, we find no evidence of the impact of stress on mortality, disability and work related disability.

7 Conclusions

Exposure to stress in utero exerts a negative effect on long-run earnings and qualifications. The effect does not dissipate along the carrier, on the contrary, it increases with time. The effect is strongest in the first pregnancy months. Each additional shock throughout the working career causes a higher penalization for the disadvantaged individuals.

References

Aizer, A., L. Stroud, and S. Buka (2016). Maternal stress and child outcomes: Evidence from siblings. *Journal of Human Resources 51*(3), 523–555.

Almond, D. and J. Currie (2012). Killing me softly: The fetal origins hypothesis. *Journal of Economic Perspectives 25*(3), 153–72.

Baldoli, C, A. and R. Knapp (2011). Overy, bombing, states and peoples: in western europe 1940-1945. *7*(10), 882–892.

Baldoli, C, A. and R. Knapp (2012). Forgotten blitzes: France and italy under allied air attack, 1940-1945. *7*(10), 882–892.

Black, S., P. Devereux, and K. G. Salvanes (2016). Does grief transfer across generations? bereavements during pregnancy and child outcomes. *American Economic Journal: Applied Economics 8*(1), 193–223.

Currie, J. and M. Rossin-Slater (2013). Weathering the storm: Hurricanes and birth outcomes. *Journal of Health Economics 3*(32), 487–503.

ISTAT (1957). Morti e dispersi per cause belliche negli anni 1940-45. *Italian National Institute of Statistics Technical report*.

Kopp, C. B. and J. B. Krakow (1983). The developmentalist and the study of biological risk: A view of the past with an eye toward the future. *Child Development 5*(54), 1086–1108.

Lewbel, A. (2007). Estimation of average treatment effects with misclassification. *Econometrica 2*(75), 537–551.

Persson, P. and M. Rossin-Slater (2018). Family ruptures, stress, and the mental health of the next generation. *American Economic Review 108*(4-5), 1214–52.

Strazza, M. (2010). Senza via di scampo. gli stupri nelle guerre mondiali. *Italian National Institute of Statistics Collana:Un archivio della memoria*(18070).