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Maternal Stress and Offspring Lifelong Labor Market Outcomes *

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Abstract

This paper examines the effects of in-utero exposure to stress on lifelong labor market outcomes. We exploit a unique natural experiment that involved randomly placed Nazi raids on municipalities in Italy during WWII. We use administrative data on the universe of private sector workers in Italy and link this data to unique historical data with detailed information about war casualties and Nazi raids across space (Municipality) and time. We find that prenatal stress exposure leads to lower wage earnings when workers start their career, and that this effect persists until retirement. The earnings penalty is in large part due to the type of job that people hold and interruptions in their working career due to unemployment. We further show that workers exposed to in-utero stress face larger earnings reductions after job loss due to mass layoffs. This earnings loss deepens their relative disadvantage over time.

JEL Codes: I1, O1

Keywords: Early-life; Stress; Life-long earnings; mass layoff; dynamic complementarities.

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1 Introduction

There is abundant evidence that adverse events in the earliest stages of a person's life have lasting effects (Almond and Currie, 2012). The essential idea is that fetuses have specific developmental milestones, and that social and biological shocks can affect the development of vital organs (Kuzawa and Quinn, 2009). Therefore, a mother's exposure to adverse conditions during critical periods may compromise the health and human capital of her children later in life. A relevant shock in modern societies is stress, ranging from acute time-limited events to chronic stress.

There is an extensive body of biological and medical research on the effect of stress (Brunner, 1997; Marmot et al., 1991; Sapolsky et al., 2002, among others). Several studies show that the mother's hormonal response to anxiety and stress may have a profound impact on the neurological development of the fetus and can therefore lead to cognitive, emotional and mental problems as well as stress vulnerability later in life (Boersma and Tamashiro, 2014; Cotter and Pariante, 2002; den Bergh et al., 2005a; Marmot et al., 1991; Weinstock, 2005, and the literature reviewed in these papers).

Recent economic literature has given support to these claims by identifying causal effects of maternal stress during pregnancy on birth outcomes, educational attainment and mental health of the offspring (Aizer et al., 2016; Black et al., 2016; Camacho, 2008; Persson and Rossin-Slater, 2018; Quintana-Domeque and Rodenas-Serrano, 2017). So far, no studies have looked at life cycle labor outcomes; the aim of this paper is to fill this gap. Specifically, this paper studies two questions: *i)* If the health and ability gap between the advantaged and the disadvantaged begins in the earliest phases of life, how does it affect the future evolution of labor market outcomes during working careers? and *ii)* Do those exposed to stress in utero suffer disproportionally from job loss later in life? If dynamic complementarities ("skills begets skills", like in (Cunha and Heckman, 2007)) are relevant, negative shocks later in life may amplify the damage caused by in-utero exposure to stress.

The stress cues that we consider here are unexpected, violent raids by the Nazi regime in Italy during the second World War (WWII). On September 8, 1943 the Italian Kingdom ceased hostilities against the Allied forces. This act, known as the Armistice, initiated the Italian resistance against fascism. In response to the Armistice, the Nazis occupied Italy and started violent raids against the Italian population in order to spread terror and disrupt social life, and ultimately to discourage the Italian Partisan fighters. These raids were intentionally unpredictable and idiosyncratically distributed across time and space.

This quasi-experimental setting allows us to analyze differences in outcomes for cohorts born in municipalities right before and right after a Nazi raid, relative to the same cohorts which were not exposed to the violence. More specifically, in a generalized Difference-in-Differences (DiD) strategy we exploit the unexpected outbreak of violent raids and the spatial (municipalities) and time (months) variation after controlling for potentially confounding factors such as armed conflicts between German and Allied forces, municipality fixed effects, time fixed effects and regional trends. We also examine whether selective fertility, mobility and mortality could bias our estimates.

Our empirical analyses exploit and merge two data sources. The "Atlas of Nazi and Fascist massacres" lists and analyzes all raids involving civilians and resistance fighters killed in Italy after September 8, 1943 both by Nazi and fascist groups. This archive includes information about the type of assault, the exact location, and the age, gender and type of the victims. We link this dataset to administrative individual level data from "Istituto Nazionale della Previdenza Sociale" (INPS). INPS provides employer-employee matched data for the universe of Italian self-employed and private sector workers between 1974 and 2018. We limit our analysis to males since female labor force participation for cohorts born in 1943 was less than 30%. The data contain detailed working histories and information on earnings, occupation, employment status, pensions and the take up of social benefits. The data also contain date and municipality of birth. We enrich the two datasets with the official archive on WWII conflicts in Italy collected by Statistics Italy (ISTAT, 1957), which includes detailed information on the number of victims in WWII by province and month.

Our analyses show that workers exposed to raids in utero have lower earnings when they enter the labor market. These negative income effects persist throughout their career, and even increase with age. Selection into the type of job is important: those exposed to in-utero stress are more likely to work in lower skilled blue collar jobs. We also find evidence that interrupted labor market careers due to unemployment may be important in explaining the large earnings penalty for older workers. We show that these results are primarily driven by exposure to stress in the first two trimesters.

We next look at job loss for older workers, motivated by evidence from the medical literature showing that prenatal stress (PNS) exposure may reduce stress coping abilities (Boersma and Tamashiro, 2014). Moreover, job loss may be an important contributor to the large earnings penalty for older workers. We find that the earnings penalty for workers who lose their job due to a mass layoff is larger for those who were exposed to prenatal stress. This additional earnings loss increases with age. Finally, in line with the existing literature (e.g. Sullivan and von Wachter, 2009), we find that mass layoff episodes at older ages are likely to increase mortality, but this effect is the same across individuals regardless of exposure to in-utero stress.

Several features distinguish our study from previous work. First, our setting features a unique natural experimental design with desirable properties. The raids we consider lasted only one or two days, were unexpected and varied widely and idiosyncratically across municipalities and time. As a result, otherwise similar mothers experienced strikingly different environments while pregnant. This design and the availability of detailed administrative data allows us to control for several confounding effects due to other war related events, selective fertility and selective survival, and, therefore, to estimate relevant and precise causal treatment effects. Previous studies on maternal stress exposure analyzing longer-term outcomes exploited deaths of family members or relatives and relied on comparisons between siblings within families (Black et al., 2016) or on the assumption of randomness in the exact timing of the birth (Persson and Rossin-Slater, 2018). In our case the idiosyncratic shocks are assigned at a more aggregate level (the municipality) and are beyond the individual's control. This setup makes the shocks more likely to be exogenous from an individual point of view and (hence) requires fewer additional assumptions.[1] Terrorist attacks in the studies of Quintana-Domeque and Rodenas-Serrano (2017) and Camacho (2008) are similar to our Nazi raids, but in these studies the shocks occur over a prolonged period of time, which leaves room for behavioral responses such as migration or fertility timing. More importantly, those two studies are limited to birth outcomes, leaving later life effects unexplored.

Second, our study starts where others have stopped, as we examine the consequences of in-utero

[1] See also the discussion in Persson and Rossin-Slater (2018) about endogenous subsequent fertility and how this may affect the estimates of Black et al. (2016). Note, however, that anticipation of actual bereavement may affect estimates in both studies. Some causes of deaths such as cancer are to some extent foreseeable and may therefore induce stress much earlier than the actual bereavement of a relative. This may influence fertility timing decisions.

stress exposure on outcomes throughout the offspring's entire labor market career using detailed administrative data. These outcomes include selection into the type of job (blue collar or white collar), level of earnings at the start of the labor market career, as well as the evolution of subsequent earnings, income from pension and the collection of social security benefits. Previous studies primarily looked at infant and childhood educational and health outcomes (e.g. Bundervoet and Fransen, 2018; Persson and Rossin-Slater, 2018; Quintana-Domeque and Rodenas-Serrano, 2017). The only exception is Black et al. (2016) who in addition to infant and childhood outcomes also study adult earnings for a single year when the subjects were between 25 and 43 years old.

Third, we examine whether those exposed in utero to stress are more sensitive to labor market shocks when they are older. The existing economics literature shows that displaced workers due to mass layoffs tend to experience significant long-term earnings losses as well as lower job stability, lower employment rates and earlier retirement (see Ruhm (1991), Chan and Stevens (2001) and the literature cited in these papers). On the health side, Sullivan and von Wachter (2009) find strong increases in mortality rates for male workers that persist up to 20 years after job displacement. For male workers with a strong attachment to the labor market Browning and Heinesen (2012) find effects on overall mortality and mortality caused by circulatory disease, effects on suicide and suicide attempts and on death and hospitalization due to traffic accidents, alcohol-related disease, and mental illness. On the other hand, Browning et al. (2006) find no effects on hospitalization for stress-related disease.

We revisit this issue by looking at the mortality and earnings effects of job loss later in life for those exposed to maternal stress. To the best of our knowledge there is only one paper that looks at this issue, but it examines whether the cognitive impact of adverse events later in life is stronger if people are born in a recession.(Berg et al., 2010). Our job displacement analyses speak to a small but growing literature that empirically addresses the issue of dynamic complementarities in skills (see for example Almond and Mazumder, 2013; Malamud et al., 2016). Dynamic complementarities in skills, as defined by Cunha and Heckman (2007), refer to the idea that human capital investments later in life are more productive when the initial stock of skills is higher. In our context, an adverse shock early in life may exacerbate the adversity of later life shocks.

Our results support the idea that maternal stress exposure impairs the offspring's cognitive and/or stress management skills, leaving them more vulnerable to stressful and challenging events later in life. Indeed, these results suggest that dynamic complementarities in skills may be important for the entire path of labor market earnings. These results are of direct policy relevance and may guide policy makers in allocating resources. Our results are relevant in situations where individuals are subject to violence related stress, for instance due to growing up in deprived areas. But they also extend to the current COVID-19 pandemic, which is associated with fear for the health and well-being of individuals, which in turn leads to a strong reduction in production and demand and, consequently, firm closures and mass layoffs. The associated stress affects all, but in particular those workers who have been exposed to stress in utero and the cohort that is currently in utero. Therefore, targeting healthcare and social support interventions to these groups should be high on the policy agenda.

Section 2 briefly reviews the literature on the effect of prenatal stress on health status; Section 3 describes specific aspects of WWII in Italy, relevant for the empirical design of our study; Section 4 presents the data; Section 5 discusses the empirical strategy. Section 6 presents our main results along with the results of additional analyses, sensitivity analyses and robustness checks. Section 7 presents several robustness checks and addresses selection issues.

2 Health effects of prenatal stress

The clinical literature offers exhaustive reviews of the damages related to stress, listing the effects of Prenatal stress (PNS) on mental, emotional and immunological functioning (de Kloet et al., 2005; Gitau et al., 2001; Hansen et al., 2000; Lederman et al., 2004; Leeners et al., 2007; Matthews, 2000; Mulder et al., 2002; Weinstock, 2001, among others). Maternal anxiety and PNS are likely to lead to abnormal activity of the Hypothalamic-Pituitary-Adrenocortical (HPA)-axis, which not only exposes the fetus to altered stress hormone levels, but they are also likely to increase the permeability of the placental barrier (den Bergh et al., 2005b). Additionally, gestational stress stimulates the production of Corticotropin-Releasing Hormone (CRH), which enters the placenta (Majzoub and Karalis, 1999). The hormonal effects are important, since the brain structures are formed during the prenatal period, and any interruption of this process is likely to result in long-term changes in brain morphology and function.[2] Furthermore, recent studies have demonstrated how PNS may modulate the stress-coping phenotype of the offspring, inducing a more extreme phenotype (Boersma and Tamashiro, 2014). PNS gives rise to innate differences in stress vulnerability, such as stress-coping styles. Moreover, Boersma and Tamashiro (2014) show that when the postnatal environment does not match the prenatal environment, these alterations may have pathological consequences.

Until recently, most studies on humans have been based on observational data and have primarily presented evidence on associations between stress exposure and health outcomes. These studies find that PNS exposure in humans is associated with various psychopathologies later in life such as memory problems, decreased learning function, depression and dementia (Checkley, 1996; Heffelfinger and Newcomer, 2001; Selten et al., 1999).

There are several economic papers that have tried to assess the causal effects of stress using natural experiments such as wars, famines and natural disasters as a source of exogenous variation in PNS. While these studies are valuable since they may identify long term effects of exposure to adverse events, it is challenging to attribute their estimates solely to stress. For instance, in studies based on natural disasters (Torche, 2011) exposure usually lasts for a long time and frequently entails abrupt deterioration of the health care system.

A small set of economic papers focuses on well-defined exposures to PNS due to terrorists attacks. Both Quintana-Domeque and Rodenas-Serrano (2017) and Currie and Schwandt (2016) find negative effects of terror-related PNS on birth weight.[3] Persson and Rossin-Slater (2018) use Swedish administrative data to show that maternal stress resulting from a death in the family during pregnancy affects her unborn child's well-being from birth to adulthood, with a particular emphasis on the child's mental health. They find that prenatal exposure to the death of a maternal relative increases take-up of ADHD medications during childhood and anti-anxiety and depression medications up to

[2]PNS has been studied in both humans and animals. Laboratory animals present a better controlled setting for studying the underlying mechanism of PNS on the offspring's health, and the literature has collected important contributions on this matter. In animal experiments, prenatal glucocorticoid administration retards brain weight at birth (Huang et al., 1999), delays neuronal maturation (Huang et al., 2001), affects synapse formation (Antonow-Schlorke et al., 2003), and may permanently alter brain structure (Matthews, 2000). (Uno et al., 1994) shows how glucocorticoid in monkeys resulted in neuronal degeneration of hippocampal neurons and reduced hippocampal volume. Gestational stress was also found to give rise to anxiogenic and depressive-like behavior in rats and nonhuman primates (Weinstock, 2005).

[3]The results of the latter study describing the events of 9/11 may conflate the effects of stress with environmental factors related to toxic dust in lower Manhattan.

age 30. Another important study is Aizer et al. (2016), who use sibling fixed effect estimations and find that prenatal exposure to elevated cortisol levels in utero adversely affects cognition, educational outcomes and behavioral and motor development in childhood. The existing findings in the economic literature are thus consistent with the vast body of psychiatric and neuro-biological studies that show persistent associations between PNS and the evolution of psychopathologies later in life.

3 WWII in Italy: a natural experiment setting

This section summarizes the historical events that occurred in Italy around September 8^{th} 1943, the date of the so called *Armistice*, and briefly describes the living conditions of the local population during WWII.

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

The Armistice ceased hostilities between the Kingdom of Italy and the Allies and began the German occupation and Italian resistance against fascism. The act was secretly signed on September 3^{rd} 1943, but was made public on the radio on September 8^{th} at 18:30 Italian time.[4]

Only a few weeks before September 8^{th} 1943, the Allies landed in Sicily (July 1943) to start their campaign against the German forces (see Figure 1). While the arrival of the Allied troops occurred before the 8^{th} of September, the Italian campaign changed radically after the Armistice. In fact, by September 8 the Italian Army was left without instructions about how to engage the German armed forces. In the days following the Armistice most Italian servicemen were left without orders

[4]The Armistice was secretly signed in Santa Teresa Longarini district of Syracuse, 3 km from Cassibile (Vitali, 1980). The act was announced a bit earlier than 18:30 hrs on Radio Algiers by a declaration of General Dwight Eisenhower. Just one hour later, at 19:42, it was confirmed by the proclamation of Marshal Pietro Badoglio via the Italian public broadcasting network EIAR, (Zangrandi, 1974). After the signing of the act the Royal family and the prime ministers fled from Rome in the morning of September 9^{th}.

from their commands, due to the German ‘Wehrmacht’ disrupting Italian radio communication. The dismantling 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 ranking officers.[5] The civilian population also had no information about the evolution of relations with Germany.

Given the unforeseen circumstances that evolved during the first few days of September 1943, the series of events that followed was very difficult to predict, ruling out strategic migration responses by civilians. According to Strazza (2010), very frequently the information about the arrival of military troops did not spread across neighbouring villages. Moreover, according to Baldoli et al. (2011), there were no national evacuation plans. From a logistics point of view, moving across provinces was extremely difficult, since railroads and main transportation networks had been destroyed by tactical bombing from the allied forces (Baldoli and Knapp, 2012).

The period after 8th September 1943 exposed the Italian territory to two major types of adversities: *i)* general armed conflicts between the Italian and German armies with an indirect impact on civilians, and *ii)* Nazi (and fascist) violence aimed directly at the civilian population and resistance fighters. Concerning the general armed conflict, the post Armistice period was characterized by military battles, ranging from quick victories and movements of the front-line entailing relatively limited casualties, to long stalemates associated with a sizable number of fatalities. The underground resistance was not coordinated at the national level. Especially immediately after the Armistice, 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, which was liberated by a chaotic popular rebellion. Figure 1 provides a detailed map of the WWII events over time and space. The army movements started in Southern Italy, moved north and then got stuck along the Winter line in December 1943. The seven month stalemate that followed caused huge losses among civilians.[6]

[5]For example, on 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. Servicemen received two contrasting orders, one from the Italian coastal forces commanded by a former fascist “Gerarca” granting access, while the naval commander denied access to the port.

[6]The Winter (or Gustav) Line, though ultimately broken, effectively slowed the advance of the allied forces for seven months between December 1943 and June 1944. Major battles in the assault on the Winter Line at Monte Cassino and

Figure 1: WWII fronts in Italy

Notes: The map comes from ANPI (Roma).

The violent Nazi episodes were intentionally unpredictable and idiosyncratically placed with the aim of disrupting civilian life in Italy and disseminating terror. Historical research highlights the connection between the violence against defenseless civilians and the military strategic goals set by the German army in Italy. The violence was perpetrated against groups of resistance fighters to some extent, but more importantly it was aimed at the Italian population. German terror acts included exploiting women, children and economic resources, land desertification, rounding up civilians and deporting them to labor camps. On numerous occasions the German troops organized massacres of civilians around the defensive and withdrawal lines. Although there were relatively few Italian casualties and the raids only lasted for a few days, these massacres were characterized by intense violence witnessed by the civilian population.

4 Data

The individual-level data-set we use is a combination of several administrative data-sets. To start with, we collect historical information on *i)* Nazi violence episodes and *ii)* the number of deaths and missing persons in Italy during WWII, in both the prenatal period and the first years of life for the universe of Italian individuals born during WWII. We refer to this dataset as "War data". The second data-set includes working career histories derived from longitudinal individual level administrative employer-employee data on labor market outcomes and mortality. We refer to this dataset as "labor data".

4.1 War data

4.1.1 Nazi violence data

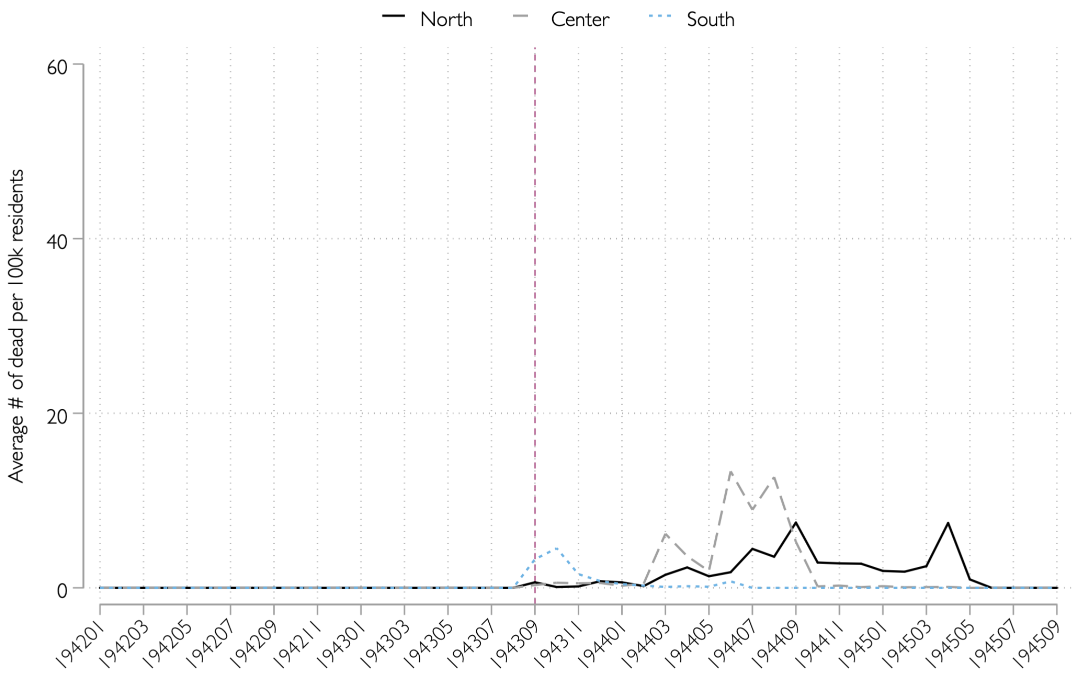
Following the recommendation of a joint Italian-German commission, the German Foreign Ministry created a German-Italian "Fund for the future". Under this fund, the INSMLI (National Institute for the History of the Italian Liberation Movement) and the ANPI (National Association of Italian Partisans) started a project whose aim was to provide a complete picture of the violence perpetrated against civilians by the German army and its fascists allies in Italy between 1943 and 1945. The

Anzio alone resulted in 98,000 Allied casualties and 60,000 German and fascist casualties.

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 violent episodes.[7]

The database lists and analyses all raids involving murders of civilians and resistance fighters killed in Italy after September 8$^{\text{th}}$, 1943 both by German soldiers and Italian fascists outside of formal military engagements. These events range from the first murders in the South to withdrawal massacres committed in the days after the Liberation (April 25$^{\text{th}}$ 1945) in the regions of Piedmont, Lombardy and Trentino Alto Adige.

Figure 2: Average Nazi raid victims per 100,000 residents by region



The database provides information on the number and type of victims by age and gender and the type of violence (murder and other violence, such as robbery, property destruction, sexual violence), the perpetrator's type (Germans or Italian fascist) and the precise timing (day) and place (municipality).[8] The database counts more then 5,800 episodes producing 20,000 victims distributed across 2,200 municipalities, with an average of 5 victims per episode. Table 1 reports descriptive

[7]This database is also used by Gagliarducci et al. (ming). Information about the project, the database and access possibilities is available at `http://www.straginaziFasciste.it/?page_id=9&lang=en`.

[8]This is the smallest administrative unit.

statistics on the number episodes, victims, duration of the Nazi raids and nature of victims involved by age and gender, while Figure 2 depicts the evolution of casualties due to violent raids by region, over time. [9] The casualties progress over time from South to North.

Table 1: Summary statistics on Nazi raids episodes between Sept 1943–May 1945 in all Italian municipalities.

	N. obs.	Mean	Std. Dev.	Min	Max	p1	p50	p99
Italy								
Length in days	5878	1.36	3.11	1	89	1	1	10
Number of victims	5878	4.14	15.03	0	770	1	1	42
Proportion of women (%)	5878	8.07	23.59	0	100	0	0	100
Proportion of children (%)	5878	1.38	9.41	0	100	0	0	50
Proportion of men (%)	5878	90.55	25.77	0	100	0	100	100
Proportion of partisans (%)	5878	33.93	51.66	0	100	0	0	100
Proportion of searches (%)	5878	29.52	45.62	0	100	0	0	100
Proportion of retaliations (%)	5878	16.77	37.37	0	100	0	0	100
Northwest								
Length in days	1376	1.57	4.24	1	72	1	1	15
Number of victims	1376	3.89	6.45	1	97	1	2	33
Proportion of women (%)	1376	5.31	18.39	0	100	0	0	100
Proportion of children (%)	1376	0.76	7.1	0	100	0	0	33.33
Proportion of men (%)	1376	93.93	19.92	0	100	0	100	100
Proportion of partisans (%)	1376	53.92	69.3	0	100	0	66.67	100
Proportion of searches (%)	1376	44.48	49.71	0	100	0	0	100
Proportion of retaliations (%)	1376	19.77	39.84	0	100	0	0	100
Northeast								
Length in days	2033	1.38	3.42	1	89	1	1	9
Number of victims	2033	4.51	19.01	0	770	1	2	46
Proportion of women (%)	2033	6.85	21.87	0	100	0	0	100
Proportion of children (%)	2033	1.38	9.34	0	100	0	0	50
Proportion of men (%)	2033	91.77	24.39	0	100	0	100	100
Proportion of partisans (%)	2033	47.91	47.34	0	100	0	40	100
Proportion of searches (%)	2033	31.68	46.53	0	100	0	0	100
Proportion of retaliations (%)	2033	19.28	39.46	0	100	0	0	100
Center								
Length in days	1512	1.26	1.92	1	29	1	1	8
Number of victims	1512	4.68	17.94	1	391	1	1	58
Proportion of women (%)	1512	9.42	25.3	0	100	0	0	100
Proportion of children (%)	1512	1.31	8.77	0	100	0	0	50
Proportion of men (%)	1512	89.27	27.14	0	100	0	100	100
Proportion of partisans (%)	1512	16.14	35.58	0	100	0	0	100
Proportion of searches (%)	1512	22.22	41.59	0	100	0	0	100
Proportion of retaliations (%)	1512	12.3	32.86	0	100	0	0	100
South								
Length in days	957	1.22	1.7	1	28	1	1	8
Number of victims	957	2.84	7.03	1	125	1	1	27
Proportion of women (%)	957	12.5	29.49	0	100	0	0	100
Proportion of children (%)	957	2.39	12.78	0	100	0	0	100
Proportion of men (%)	957	85.12	32.13	0	100	0	100	100
Proportion of partisans (%)	957	3.6	17.88	0	100	0	0	100
Proportion of searches (%)	957	14.94	35.67	0	100	0	0	100
Proportion of retaliations (%)	957	14.21	34.93	0	100	0	0	100

Notes: The numbers refer to an overall sample of 5,878 episodes between Sept 1943–May 1945 occurring in all Italian municipalities.

[9] Our analyses in section 6 are based on a sub-sample covering the period September 1943–May 1944. The summary statistics of the Nazi raids over this period are reported in Table B1 of Appendix B.

4.1.2 General data on deaths and missing persons during WWII in Italy

This dataset was reconstructed from an official archive publication "Morti e dispersi per cause belliche negli anni 1940–45" (The dead and the missing due to war causes between 1940–1945) ISTAT (1957), collected by the Italian National Institute of Statistics (ISTAT).[10] The dataset includes the number of victims of armed conflicts by province and month. Besides casualties, these fights generally entailed destruction of houses, factories and infrastructure. We use this mortality information to control for the intensity of battles between the Allied Forces and the Germans.[11]

As described in the previous section, the allied forces started in the South in September 1943 and gradually progressed up to the Northern territories, ultimately freeing Italy in May 1945. The progressive movement of the allied forces is reflected in the location of casualties over time (Figure 3). In accordance with the evolution of the Nazi massacres (Figure 2) the figure clearly shows how the peaks in mortality move over time, starting from the South, moving to the Center and finally to the North.

4.2 The labor data

The longitudinal employer-employee data are provided by the Instituto Nazionale della Previdenza Sociale (INPS[12]), the largest social security and welfare institute in Italy and one of the largest administrative bodies at the European level. INPS oversees all private sector (except agricultural) employees and private sector employers operating in Italy during the years 1974–2018.

4.2.1 Matched employer-employee data

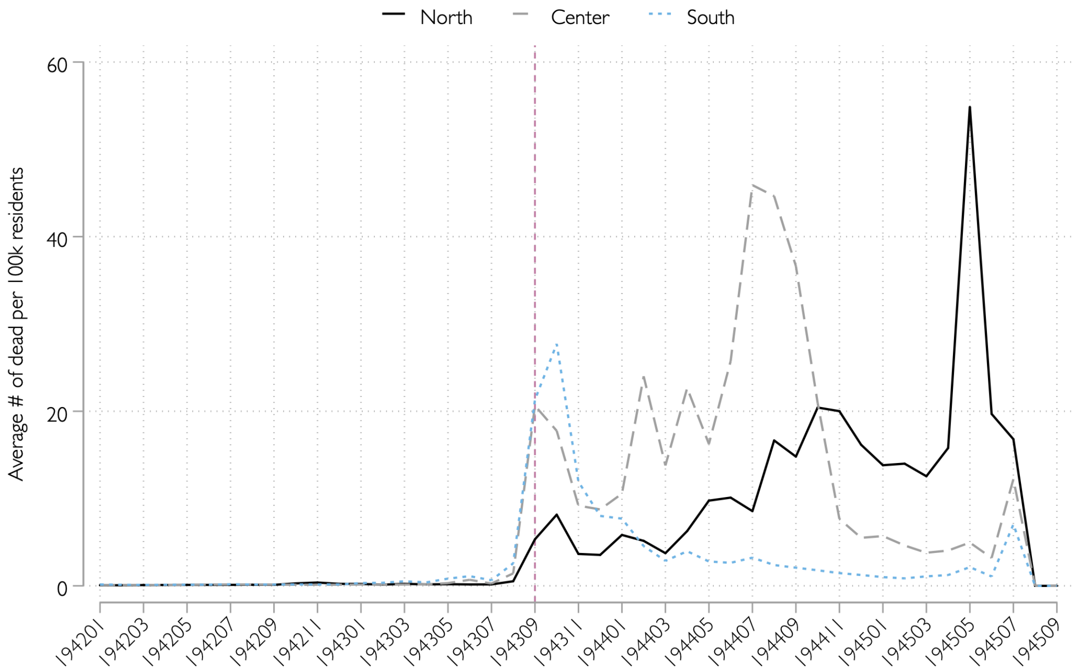
INPS provides longitudinal employment and earning histories from 1974, along with information on the employee's occupation and contract type. An important characteristic of the INPS data is its full coverage for all private sector work contracts in Italy over the period 1974–2018. As a result, the

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

[11] The analysis is framed in the current administrative province division of the country, while ISTAT data report the intensity of war according to the historical administrative provinces. In recent decades some provinces changed their structure by incorporating/separating some of their municipalities. We take these changes into account when calculating the monthly province mortality rates.

[12] https://www.inps.it/nuovoportaleinps/default.aspx?itemdir=47212

Figure 3: Macro area average WWII victims per 100,000 residents



dataset allows us to track the labor market mobility of workers over their entire working career, independent of their employer. Additionally, INPS is a matched employer-employee database, including information on all private sector employers in addition to employee information. Importantly, for all Italian residents INPS has information on labor market entry date, date of death, and municipality and month of birth. The information on municipality and date of birth allows us to assign Nazi raids to each individual.

One limitation of the private sector data provided by INPS is that it does not include all jobs. Therefore, zero earnings in any given year could be due to moving out of the labor market or being employed in a different sector.[13] INPS also includes detailed information on pensions and social security benefits claims for individual workers, which can make it easier to interpret zero earnings.

[13]The status of non employment, in the INPS private sector data (excluding agriculture) can have different meanings. The data do not distinguish between unemployment and inactivity. Moreover, an individual can be non employed and thus unidentified in the INPS data if s/he is self employed or employed in the agriculture or public sector. In this regard INPS data are similar to other administrative employer-employee data.

4.2.2 Pension and benefits data

From 1995 onward INPS also provides information on the universe of Italian pensioners. These data include information about when retirees collect their individual pensions, pension benefits, and the number of years an individual has contributed to the pension system.

The cohorts in our study (see Figure 4 below) have defined benefit (DB) pensions.[14] Additionally, INPS collects social security premiums for the provision of Disability Insurance (DI), Unemployment Insurance (UI) and Welfare. With this information we can infer whether individuals have faced interruptions during their labor market careers.

4.3 Data selection

We link the Nazi violence data to the individual INPS data through the municipality and month and year of birth for workers. For the general WWII data, we link records at the province and month of birth level.

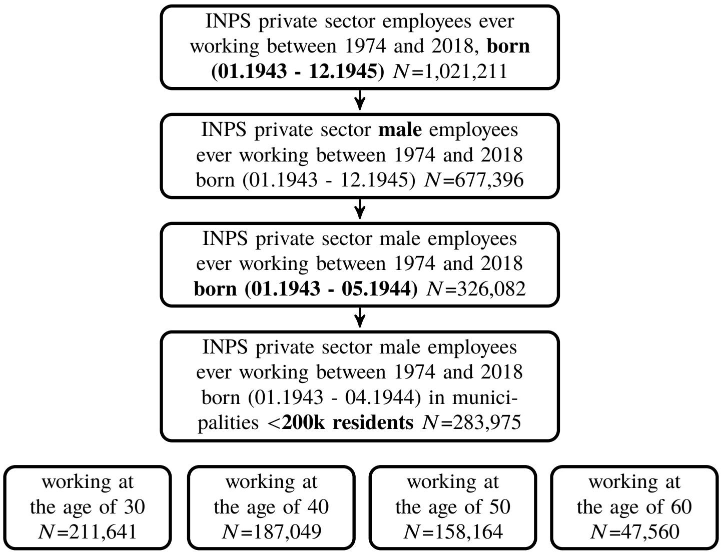
We made some sample restrictions to obtain the dataset used in the analyses. Figure 4 shows our selection process. We first select all employees in the private sector who worked at some point between 1974 and 2018 and were born in the WWII era, loosely defined as the period between January 1943 and May 1945 (for a total of 1,012,211 individuals).

We restrict our sample to males, since female labor force participation was very low (less than 30%) for our cohorts.[15] This results in 677,396 individuals. Given that the sudden onset of Nazi raids was triggered by the Armistice made public on September 8, 1943 (see Section 3), we restrict our sample to individuals born in a 9 month window around the Armistice (i.e. born between January 1943 and May 1944). Figure 5 contains a timeline of the events and the monthly cohort selection. This sample restriction rules out selective fertility timing behavior. Those born less than 9 months

[14] Prior to 1996 the Italian system was financed by general taxation (PAYG). In 1996, the Dini Reform transformed the scheme into a funded national defined contribution system (NDC). The new system was phased in through a long transitory process but only for workers who had worked fewer than 18 years by the end of 1995. The NDC system is applied pro-rata for all worker contributions paid after 2011. The cohorts in our sample are all born around 1943 (see the data selection section below). Therefore more than 99% of our sample is retired by 2011 and consequently falls under the DB scheme.

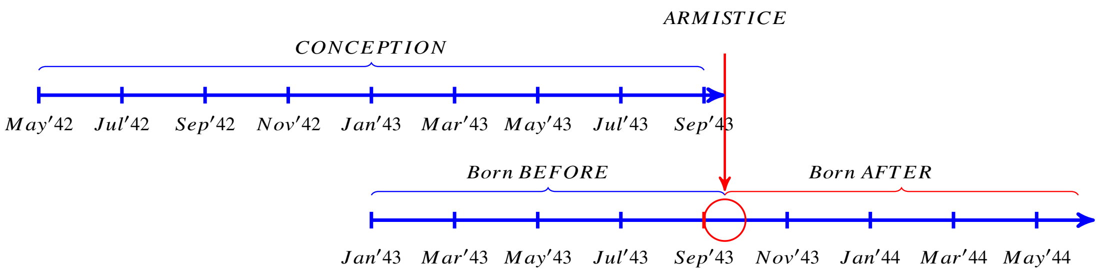
[15] Exposure to stress early in life may also affect fertility behavior, which in itself may effect labor force participation decisions by females. This would make the results difficult to interpret.

Figure 4: INPS data selection



after September 8th were (potentially) exposed to in-utero stress, while others (born in the 9 months preceding September 8th) were not. While this timing restriction rules out fertility timing responses, it does not rule out mortality selection effects in utero (still births) and between birth and the first time that we observe these individuals in 1974 in our administrative INPS data set (we return to this question in a later section). This last selection results in a sample of 326,082 individuals.

Figure 5: Cohort selection and timing of events.



Another confounding factor to consider is that the nature of the war was different in large cities. On the one hand, raids in large cities generally involved more victims and nearby areas were more likely to hear about these raids. This complicates the distinction between the treated and control subjects. Besides, large cities experienced other adverse WWII conditions such as nutritional shortages, mass destruction and bombing. These other hardships may confound our estimates and may also affect the interpretation of the effects of the Nazi raids. We therefore further restrict our sample to municipalities with less than 200 thousand residents.[16]

This final selection reduces our sample to 283,975 males born in 7,507 municipalities. In this sample 22,194 (7.8%) individuals were exposed in utero to maternal stress (PNS) due to 1,603 raids. The raids lasted 1.4 days on average, with an average of 3.3 victims per raid, and 89% of the victims were men.[17]

4.4 Exposure variables

For each individual in our sample we determine the intra-uterine period by counting 9 months backwards from the exact date of birth. We use the estimated date of conception to define the three gestational trimesters.[18]

For each individual in the INPS data we trace his/her municipality of birth and use the Nazi violence data to create an indicator for exposure. This indicator equals 1 if the municipality of birth had at least one Nazi violence episode in the 9 months preceding the birth. Analogously we construct a war intensity variable from the general war data set (see Section 4.1). This variable is defined by the number of war victims in the province of birth due to armed fights between allied forces and the German troops in the 9 months prior to the month of birth. For the purposes of interpretation, we standardize this variable into a z-score with mean zero and standard deviation of one.

4.5 Outcome variables

Using individual level INPS working histories, we create two key annual labor outcomes: log earnings and an indicator for manual (unskilled) labor.[19] We define our outcomes at different points in time/ages: the timing of the first employment, the timing of the last employment, as well as at

[16]We use current population counts, as complete municipality level census data are not available for the WWII period.

[17]See also Table B1 in Appendix B.

[18]Premature births may be relevant in the context of stress. Experiencing stress leads to increased levels of corticotropin-releasing hormone (CRH). High CRH-levels prepare the fetus for a preterm birth. If stressed mothers did have shorter pregnancies, the one sided measurement error is likely to lead to biased estimates since the error may be correlated with higher chances of negative health outcomes, typical for preterm babies. In Section 7 we check that our results are robust to alternative gestational periods.

[19]The blue collar indicator is derived from a hierarchical categorical variable that takes on 5 values: 1) Manual; 2) Skilled non-manual; 3) Professional; 4) Managerial; 5) Apprentice. Earnings are expressed in 2005 euros, adjusted for inflation using the CPI index. In the analysis all missing records are kept as missing.

distinct ages a, (for $a = 30, 35, 40, 45, 50, 55, 60$ and 65). To minimize the variance in earnings, for each a we take the average of the earnings at $(a-1)$, a and $(a+1)$. For each individual we construct binary indicators for mortality, unemployment benefit and disability benefit receipt. We also examine the effect of in-utero stress exposure on two retirement outcomes: the age at retirement and the pension benefit level. The pension benefit level is a function of work experience, age when the benefit is claimed, and average earnings over the last working years.

We exploit the employer-employee nature of our data to identify episodes of job loss due to a mass layoff at the firm.[20] In line with the literature (Sullivan and von Wachter, 2009), we define a mass layoff as an event where firms with more than 25 workers reduce their total work force by more than 30% in a given year. We link these mass layoffs to individual workers at these firms whose contract terminates as a result of the mass layoff.

4.6 First descriptive evidence based on the INPS data

Table 2 provides some initial evidence from relevant information in our database. About 8% of the individuals in our sample were exposed to in-utero stress, earnings gradually increase with age, and about three quarters of all workers were blue collar when they entered the labor market. This last group declines over time, suggesting that these workers leave the labor market more quickly than other workers. Figure 6 plots this phenomenon by treatment status. Similarly, figure 7 describes the pattern of earnings across the working career by treatment status. Table 2 shows that 22% of workers have collected a disability benefit at some point during their working career, that at the end of our observation period about 82% of the male workers had retired, and that the average age at retirement is about 58 years old.

Table 3 provides the first descriptive evidence of the PNS effect (i.e. prenatal exposure to a Nazi raid) on some outcomes. Columns 1 and 2 report the mean labor market outcome variables for individuals born in municipalities that did not experience a Nazi raid during our observation period, while columns 3 and 4 report the same information for individuals born in a municipality that did

[20]Job loss due to a mass layoff is arguably more exogenous. See Sullivan and von Wachter (2009) for a discussion.

Table 2: Descriptive statistics for INPS men born between January 1943 and May 1944

	Mean	Std. Dev.	N. obs.
Nazi violence in utero	0.08	(0.27)	283,975
War victims in utero (# for 100k province pop.)	57.08	(69.38)	283,975
First year of earnings	1977	(6.61)	283,975
Last year of earnings	1994	(9.77)	283,975
Number of years with positive earnings	16	(9.89)	283,975
Earnings at 30	15014	(8123)	211,641
Earnings at 40	19999	(10240)	187,049
Earnings at 50	26582	(18626)	58,164
Earnings at 60	24712	(24785)	47,560
Blue collar at 30	0.79	(0.41)	211,641
Blue collar at 40	0.71	(0.46)	187,049
Blue collar at 50	0.68	(0.47)	158,164
Blue collar at 60	0.68	(0.47)	47,560
Ever disabled	0.11	(0.31)	283,975
Ever unemployed	0.22	(0.42)	283,975
Ever unemployed due to mass layoff	0.10	(0.27)	283,975
Retired	0.82	(0.39)	283,975
Retirement age	58	(5.39)	232,035
First monthly retirement pension	1173	(859)	232,035
Dead	0.24	(0.43)	283,975

Notes: The numbers refer to a sample of 283,975 males working in the private sector, born in municipalities with fewer than 200,000 residents between January 1943 and May 1944. Wages and pensions are expressed in 2005 euros.

Table 3: Descriptive Difference-in-Differences statistics of the sample of INPS men born between January 1943 and May 1944

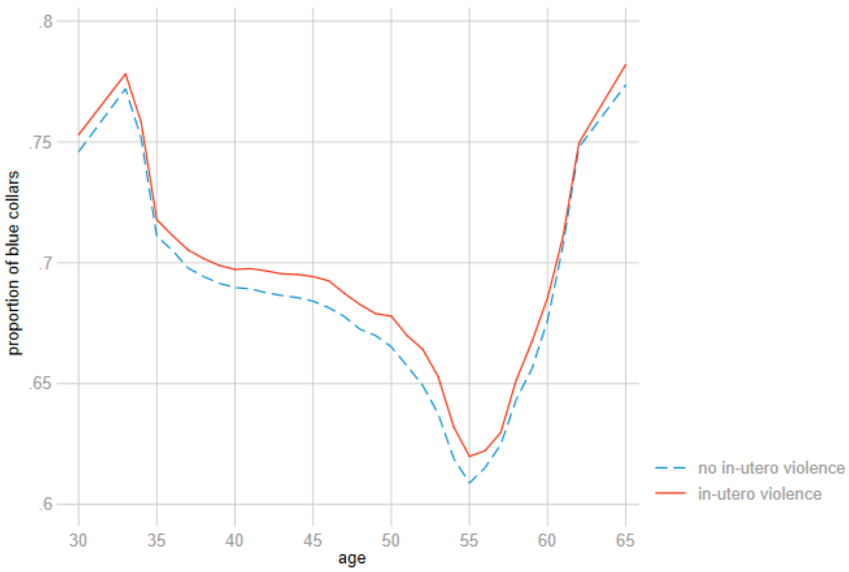
	Control		*Treated*		*DiD*
	Before (A)	*After* (B)	*Before* (C)	*After* (D)	$(D-C)$ $-(B-A)$
earnings at 30	14579	15212	15183	15531	-285**
earnings at 40	19683	19917	20768	20622	-381**
earnings at 50	26049	26206	28721	27764	-1113***
earnings at 60	23865	24180	27689	26387	-1617***
blue collar at 30	0.8	0.8	0.73	0.77	0.04***
blue collar at 40	0.72	0.71	0.66	0.67	0.02***
blue collar at 50	0.69	0.69	0.63	0.64	0.01***
blue collar at 60	0.69	0.68	0.62	0.65	0.04***
Ever disabled	0.11	0.10	0.10	0.10	0.01*
Ever unemployed	0.23	0.23	0.21	0.21	0.01*
Ever unemployed due to mass layoff	0.1	0.1	0.09	0.09	0
Retired	0.82	0.82	0.82	0.82	0.00
Age at retirement	57.79	57.64	57.99	57.9	0.06*
First retirement monthly pension	1231	1254	1354	1348	-29*
Dead	0.21	0.19	0.21	0.19	0
Observations	103,818	118,557	29,743	31,857	

Notes: The numbers refer to the sample of males working in the private sector between 1974 and 2017 born in January 1943–May 1944 in municipalities with fewer than 200,000 residents. The sample size varies according to outcome and when it was measured. Wages and pensions are expressed in 2005 euros. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

experience a raid. The distinction between before and after is based on the date of the Armistice (September 8, 1943).[21] Finally, column 5 provides a simple difference-in-differences of the variable means. The results in Column 5 agree with our priors (see Figures 6 and 7): exposure leads to lower earnings and a higher likelihood of being in a blue collar job. Column 5 also shows that exposed individuals are more likely to experience a disability and an unemployment spell during their working career. Interestingly, exposed individuals also retire a bit later.

These initial descriptive analyses are interesting since they provide preliminary, tentative evidence of the effect of in-utero exposure to a Nazi raid on a range of outcomes. However, these analyses do not exploit the exact timing of the Nazi raids, nor do they control for other circumstances and municipality and time specific fixed effects. These effects may confound our preliminary reading of the evidence. In what follows, we present our empirical model and identification strategy, which are designed to overcome these concerns.

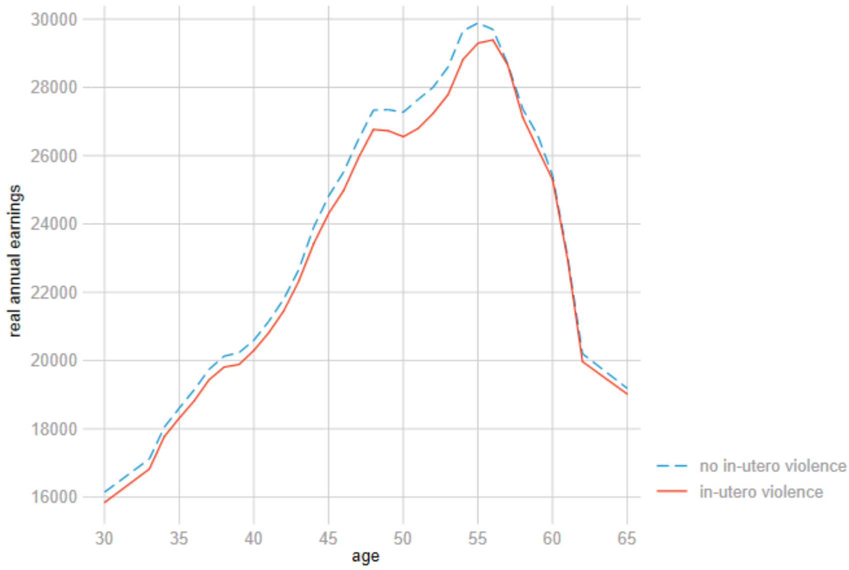
Figure 6: Proportion of blue collar workers by treatment status



Notes: The numbers refer to 283,975 men working in the private sector between 1974 and 2017, born between January 1943 and May 1944 in municipalities with fewer than 200,000 residents, selected from the initial sample of 772,131 men workers born in 1942-1945. The figure pictures differences between individuals exposed to in-utero raid and those not exposed to any raid, controlling for municipality fixed effects.

[21] Note that the table presents simple, raw calculations that do not take into account the exact timing of the Nazi raids in each municipality.

Figure 7: Wages by treatment status



Notes: The numbers refer to 283,975 men working in the private sector between 1974 and 2017, born between January 1943 and May 1944 in municipalities with fewer than 200,000 residents, selected from the initial sample of 772,131 men workers born in 1942–1945. The figure shows differences between individuals exposed to in-utero raids and those not exposed to any raid, controlling for municipality fixed effects. Annual earnings are expressed in 2005 euros.

5 Empirical model and identification strategy

In section 3 we provided historical information about the circumstances following the Armistice. To briefly recap: the Nazi responded by occupying Italy and performing violent raids. These raids were intentionally unpredictable and idiosyncratically placed across time and space. Our empirical analyses exploits the random variation in violent raids to assess the effect of prenatal stress exposure on labor market outcomes. Specifically, our baseline specification is the following generalized Difference-in-Differences model (DiD):

$$y^a_{imt} = \beta^a_0 + \beta^a_1 Nazi_{mt} + \beta^a_2 War_{pt} + \alpha^a_m + \gamma^a_t + \delta^a_{tr} + \epsilon^a_{imt}, \tag{1}$$

where y^a_{imt} represents the labor outcome for individual i, born in municipality m at time t, at age a (for a=30, 35, 40, 45, 50, 55, 60). For notational convenience we suppress subscripts p for the municipality's province (103 in total) and r for the municipality's region (20 in total). $Nazi_{mt}$ is an indicator for whether an individual's municipality of birth had at least one episode of Nazi violence in the 9 months before birth. Similarly, War_{pt} is the number of war deaths in the province of birth p per 1,000 residents in the 9 months prior to the month of birth. We include municipality (α_m) fixed effects, $year \times month$ fixed effects (γ_t), and region specific time trends (δ_{tr}). ϵ^a_{imt} is an idiosyncratic error term. In all our analyses we cluster standard errors at the municipality level.

The coefficient β_1 is identified by comparing individuals exposed in utero to a Nazi raid to those who were exposed in the first year of life and those born in municipalities that were not subject to any Nazi raid. The margin that we use to identify our treatment effect is different from Persson and Rossin-Slater (2018), who exploit random variation in the timing of bereavement of bereaved mothers. More precisely, they compare outcomes for children born to mothers who loose one of their parents or a close relative just prior to or just after the birth of the child.[22] Quintana-Domeque and Rodenas-Serrano (2017) use a margin that is comparable to ours. They exploit variation in time and area of residence to examine the effect of terrorist attacks on birth outcomes.[23]

Some comments are in order before we present our estimation results. The coefficient β_1 measures the effect of the Nazi raid over and above general war effects (controlled for by War_{pt}). Any effect of Nazi raids on our outcome measures can be interpreted as the impact of prenatal exposure to stress. In order to validate this claim we use information from an external data base: the Health Search/CSD Patient Database. This is a nation-wide representative sample of Italian adult patients, containing patient electronic clinical records (ECRs). The records include the so-called ATC (Anatomic Therapeutic Chemical) drug classification codes. With this information we can compute drug expenditures for specific disease types at the patient level.[24] We find that Nazi raids have significant and sizable effects on health expenditure for diseases of the nervous system and mental disorders (see Table A1 in Appendix A). This finding is in line with the medical literature that finds strong associations between in-utero stress exposure and various psycho-pathologies later in life such as memory problems, decreased learning function, depression and dementia (Checkley,

[22]In our context this means estimating our model on treated municipalities only. We present estimates of this model in Section 7

[23]However, they use data over a prolonged period of time (1980–2003), which could lead to anticipation and subsequent behavioral (fertility and mobility) responses.

[24]More information about this database and the Italian health care system can be found in Appendix A.

1996; Heffelfinger and Newcomer, 2001; Selten et al., 1999).

The assumption that Nazi raids are randomly assigned is crucial in order to identify the treatment effect. Including municipality fixed effects controls for time invariant differences between the treated and control municipalities. In Appendix C we further examine the assumption of random assignment. In Section 7 we also present falsification tests that support the assumption of (conditional) random assignment.

Our main results in Section 6 are based on the basic model (Equation 1), but we also consider the results of two alternative specifications. First, medical studies point at differential effects by trimester of exposure. The first trimester may be particularly critical since this is when the central nervous system is formed (Schulz, 2010). For example, Torche (2011) finds stronger effects of first trimester exposure to maternal stress on birth outcomes. On the other hand the review by King et al. (2012) suggests that there is no single period of vulnerability, but that the effects vary by trimester of exposure. We therefore also estimate versions of Equation 1 that allow for separate exposure effects by pregnancy trimester:

$$\begin{aligned} y^a_{imt} = &\beta^a_0 + \beta^a_{1,1} Nazi_{1,mt} + \beta^a_{1,2} Nazi_{2,mt} + \beta^a_{1,3} Nazi_{3,mt} + \\ &\beta^a_{2,1} War_{1,pt} + \beta^a_{2,2} War_{2,pt} + \beta^a_{2,3} War_{3,pt} + \alpha^a_m + \gamma^a_t + \delta^a_{tr} + \epsilon^a_{imt}. \end{aligned} \tag{2}$$

We infer birth trimesters assuming a 9 month gestation period. Medical studies have shown that prenatal stress increases the risk of pre-term birth, defined as a gestational period less than 37 weeks (see e.g. Lilliecreutz et al. (2016)). Our exposure measures by pregnancy trimester are therefore likely to be measured with error. This holds in particular for exposure in the first two trimesters. Specifically, those assigned to exposure in the first trimester may not have been exposed in utero, and those assigned to the second trimester may have been exposed in the first trimester. We address the sensitivity of our estimates to alternative assumptions about the length of the gestational period in Section 7.

Second, Nazi raids may give rise to elevated cortisol levels and/or trauma for the parent(s) that persists for a longer period (Post Traumatic Stress Syndrome, PTSS) and may influence parental behavior and parenting skills when the child grows up (Akresh et al., 2012). This hypothetical additional effect has important implications for the parameter estimates of Equation 1 and 2. The estimate of in-utero exposure in Equation 1 will in this case include both a biological and a behavioral (PTSS) effect. [25] To examine the relevance of this effect we could also estimate models that include effects for exposure to a Nazi raid in the child's first and second year of life. Note, however, that this analysis is equivalent to a test of the common trends assumption. We therefore report the results of this event study in Section 7.

6 Results

In this section we present the main results based on Equation 1 and its extensions (Equation 2). The outcomes are defined at age 30/31 (when we observe individuals for the first time in 1974) and at later ages. We examine the effects on earnings (in logarithms), blue collar status, disability and unemployment benefit receipt, retirement age, pension benefits (in logarithms) and mortality. We present the results of these analyses in subsection 6.1. Previous work has shown that prenatal stress (PNS) gives rise to innate differences in stress vulnerability, and that those exposed to PNS may have worse stress coping mechanisms (den Bergh et al., 2005a). We therefore examine the effects of a job loss due to a mass layoff in subsection 6.2.[26] In all the analyses we use robust standard errors clustered at the municipality level. [27]

6.1 Labor market outcomes across the life cycle

We conduct our baseline analysis on a sample of 283,720 individuals born in the 9 month window around the date of the Armistice (Jan 1943–May 1944). For these cohorts we observe outcomes registered over the period 1974 (aged 30–31) to 2018 (aged 74–75). We include month × year fixed effects, municipality fixed effects, and 20 region specific time trends in all our baseline specifications.

[25]The estimate of in-utero Nazi exposure in Equation 1 may include effects of PTSS lasting after the pregnancy. Additionally, a part of the reference group, namely those who are exposed to a Nazi raid post-birth, is also affected by PTSS due to the Nazi raid. Further, regarding Equation 2, the estimate of the first trimester exposure $\beta_{1,1}$ will in this case also pick up PTSS induced elevated cortisol levels of mothers in the second and third trimester. Similarly, $\beta_{1,2}$ may also include third trimester effects.

[26]In section 7 we address several issues related to identification concerns.

[27]Spatial correlation may be relevant. One could take this to some extent into account by clustering the standard errors at the province level. This hardly affected the standard errors

Table 4 shows estimates from the main model (Equation 1) for wage earnings. By the age of 30 we observe an earnings penalty of about 2% for those who are exposed to prenatal stress. The earnings penalty is slightly smaller at ages 40 and 45, but widens at ages 50 and older. At age 60 the earnings penalty increases to about 5.5%. Table 4 thus shows that there is already an earnings penalty when workers enter the labor force, and this effect does not dissipate over time. Notably, the general war effects (WWII casualties) also show significant effects, mainly at older ages.

Table 4: Age specific earnings.

	Earnings at 30	Earnings at 35	Earnings at 40	Earnings at 45	Earnings at 50	Earnings at 55	Earnings at 60
Nazi raid	-0.0218***	-0.0238***	-0.0155**	-0.0177**	-0.0268***	-0.0254**	-0.0551***
in utero	(0.0080)	(0.0075)	(0.0076)	(0.0077)	(0.0085)	(0.0120)	(0.0194)
WWII casualties (SD)	-0.0065**	-0.0073**	-0.0046	-0.0053	-0.0052	-0.0115**	-0.0118*
in utero	(0.0032)	(0.0031)	(0.0030)	(0.0034)	(0.0036)	(0.0048)	(0.0064)
R^2	0.1514	0.1437	0.1391	0.1432	0.1348	0.1418	0.1712
N	211,641	207,420	187,049	170,775	158,164	101,081	47,560
Time FEs	YES	YES	YES	YES	YES	YES	YES
Municipality FEs	YES	YES	YES	YES	YES	YES	YES
Reg trends	YES	YES	YES	YES	YES	YES	YES

$^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

Notes: The samples refers to individuals born in the 9 month window around the Armistice (Jan 1943–May 1944) in municipalities with fewer than 200,000 residents. The outcome is age specific earnings between the ages of 30 and 60. All regressions include month × year fixed effects, municipality fixed effects, and 20 region specific time trends. Robust standard errors (in parentheses) are clustered at the municipality level.

The persistent differences in earnings for those exposed in utero to a Nazi raid could be due to differences in the type of jobs that people hold. Aizer et al. (2016) finds strong effects of prenatal stress on offspring cognition and education. This finding could imply that those exposed to in-utero stress may sort into lower skilled jobs. However, sorting into lower skill jobs cannot explain the stark increase in the earnings penalty later in the working career. A possible reason for this dynamic might be related to interrupted working careers, for instance, due to unemployment or disability. An alternative explanation is that in Italy people start retiring from age 55, and by age 60 only 22% of 60 year old males are still at work. This retirement effect may be selective with respect to earnings and the type of job. Italian pension benefits that apply to this cohort were based on earned wages for the last few years of a worker's career. As such, those with higher incomes, mostly white collar jobs, may have opted for retirement at earlier ages. Indeed, the earnings penalty at the last measured earnings before retirement is smaller than the penalty at age 60 (-0.0281 (s.e. 0.0090)).

Table 5: Age specific blue collar status.

	Blue collar at 30	Blue collar at 35	Blue collar at 40	Blue collar at 45	Blue collar at 50	Blue collar at 55	Blue collar at 60
Nazi raid	0.0222***	0.0028	0.0176***	0.0172***	0.0184***	0.0277***	0.0370***
in utero	(0.0055)	(0.0067)	(0.0063)	(0.0062)	(0.0065)	(0.0072)	(0.0109)
WWII casualties (SD)	0.0030	0.0039**	0.0062***	0.0048**	0.0050**	0.0064**	0.0027
in utero	(0.0019)	(0.0019)	(0.0022)	(0.0023)	(0.0024)	(0.0030)	(0.0039)
R^2	0.0885	0.0965	0.1036	0.1107	0.1160	0.1658	0.1976
N	211,641	207,420	187,049	170,775	158,164	101,081	47,560
Time FEs	YES	YES	YES	YES	YES	YES	YES
Municipality FEs	YES	YES	YES	YES	YES	YES	YES
Reg trends	YES	YES	YES	YES	YES	YES	YES

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: The samples refers to individuals born in the 9 month window around the Armistice (Jan 1943–May 1944) in municipalities with fewer than 200,000 residents. The outcome is age specific earnings between the ages of 30 and 60. All regressions include month × year fixed effects, municipality fixed effects, and region specific time trends. Robust standard errors (in parentheses) are clustered at the municipality level.

To start investigating sorting by employment type, Table 5 shows the effects of exposure to Nazi raids on blue collar status. Those exposed to in-utero stress are more likely to work in blue collar jobs. As in Table 4 the strongest effects are found at age 60. This supports the idea that those retiring prior to age 60 are more likely to hold white collar jobs (with on average higher earnings). In Table 6 we present the results for the number of unemployment spells, disability, the age at retirement, pension benefits and mortality. The number of unemployment spells refers to the number of times an individual claimed an unemployment benefit prior to age 60. Similarly, disability and mortality are defined as having ever received a disability insurance benefit before age 60 and the probability of dying before age 60, respectively. Pension benefit is defined as the first monthly pension benefit (in logarithms) at retirement.

Table 6 shows a small and insignificant effect for disability (insurance receipt), but a sizeable and marginally significant effect for the number of unemployment spells prior to age 60. The effect on unemployment spells prior to age 60 suggests that at least part of the earnings penalty at age 60 for those exposed to stress in utero may be due to interruptions in their labor market careers. The pronounced earnings penalty at age 60 (see Table 5) could also be due to later retirement for lower earning (blue collar) workers. Table 6 supports this idea, although the coefficient on retirement age is not significant. Pension benefits are related to average earnings; as expected, those exposed to in-utero stress also have lower pension benefits upon retirement. Finally, Table 6 shows that there is no differential mortality by treatment status.

Table 6: Other outcomes before age 60.

	Disability before 60	Dead before 60	Age at retirement	First pension (log)	No. Unemployed before 60
Nazi raid	0.0007	-0.0028	0.0595	-0.0209**	0.0309*
in utero	(0.0018)	(0.0028)	(0.0562)	(0.0093)	(0.0159)
WWII casualties (SD)	0.0033***	0.0006	0.0117	-0.0051	-0.0025
in utero	(0.0012)	(0.0009)	(0.0192)	(0.0034)	(0.0083)
R^2	0.0413	0.0627	0.1970	0.055	0.0535
N	283,720	283,720	227,987	227,987	283,720
Time FEs	YES	YES	YES	YES	YES
Municipality FEs	YES	YES	YES	YES	YES
Reg trends	YES	YES	YES	YES	YES

$^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

Notes: The sample refers to 283,720 individuals born in the 9 month window around the Armistice (Jan 1943–May 1944) in municipalities with fewer than 200,000 residents. Outcomes include Disability, Mortality and Unemployment registered before they turn 60. Age at retirement and first pension are the first outcome occurring between 1974 and 2017. All regressions include month × year fixed effects, municipality fixed effects, and region specific time trends. Standard errors (in parentheses) are clustered at the municipality level.

In-utero exposure to stress leads to an earnings penalty. This gap is already apparent when workers start their careers (here defined at the age of 30), it persists as people age, and becomes even stronger at age 60. Selection into the type of job appears to be an important factor: those exposed to in-utero stress are more likely to work in lower skilled blue collar jobs. This finding is in line with the findings of Aizer et al. (2016) and Persson and Rossin-Slater (2018), who find that in-utero exposure to stress impacts cognition and mental health at younger ages. We find evidence that the pronounced earnings penalty at age 60 may be due to interrupted labor market careers due to unemployment. Specifically, job loss later in the life cycle may lead to substantial earnings losses. We further investigate this channel in subsection 6.2.

In section 5, we introduced an alternative model specification (Equation 2) that allowed for differential effects of in-utero exposure to stress by trimester of exposure. However, estimates of Equation 2 should be interpreted with caution. We defined trimesters by assuming a 9 month (40 week) gestation period. Prenatal stress exposure is known to be associated with increased risks of preterm births (defined as a gestational period of less than 37 weeks). Therefore, we may measure the trimesters with error. This potential error is particularly concerning for those exposed in the first two trimesters; some of those assigned to the second trimester may actually have been exposed in the first trimester and, more importantly, some of those assigned to be exposed in the first trimester may not have been exposed in utero.[28]

Table 7 presents the results for earnings singling out Nazi raid episodes by each trimester of gestation. Across all ages, the most substantial effects are for exposure in the second trimester. At younger ages first trimester exposure effects are small and have large standard errors, but the effects become stronger and standard errors shrink for older workers. Prenatal stress may lead to preterm delivery, and some of those assigned to exposure in the first trimester may not have been exposed in utero. This may explain the relatively large standard errors for the first trimester effects. Reasoning along the same line, second trimester effects may pick up some of the first trimester's importance. Therefore, in the presence of such measurement errors it is difficult to interpret the effects for the first and second trimester. [29]

[28]In section 7 we explore the robustness of our estimates under different assumptions about the length of gestational period.

[29]King et al. (2012) find that there is no single period of vulnerability, but that the consequences of exposure differ by trimester. They find evidence that exposure in the first trimester is related to head circumference, cognitive development at age 2 and symptoms of autism at age 6. Exposure in the second trimester is related to cognitive and language development at age 2 and exposure in the third trimester is related to motor development.

Table 7: Age specific wages by trimester of exposure.

	Earnings at 30	Earnings at 35	Earnings at 40	Earnings at 45	Earnings at 50	Earnings at 55	Earnings at 60
Nazi raid	-0.0098	-0.0088	-0.0081	-0.0003	-0.0380*	-0.0445*	-0.1131***
trim1	(0.0196)	(0.0171)	(0.0172)	(0.0180)	(0.0198)	(0.0263)	(0.0368)
Nazi raid	-0.0363***	-0.0324***	-0.0340***	-0.0268***	-0.0429***	-0.0333**	-0.0932***
trim2	(0.0119)	(0.0110)	(0.0101)	(0.0101)	(0.0119)	(0.0149)	(0.0238)
Nazi raid	-0.0209***	-0.0230***	-0.0070	-0.0165**	-0.0173**	-0.0285**	-0.0347*
trim3	(0.0080)	(0.0076)	(0.0082)	(0.0079)	(0.0086)	(0.0125)	(0.0206)
WWII casualties (SD)	-0.0087***	-0.0061**	-0.0023	-0.0042*	-0.0029	-0.0053	-0.0119**
trim1	(0.0026)	(0.0025)	(0.0022)	(0.0024)	(0.0025)	(0.0034)	(0.0050)
WWII casualties (SD)	-0.0055**	-0.0038*	-0.0035*	-0.0052**	-0.0026	-0.0065*	-0.0063
trim2	(0.0023)	(0.0020)	(0.0020)	(0.0024)	(0.0024)	(0.0033)	(0.0043)
WWII casualties (SD)	-0.0006	-0.0034	-0.0017	-0.0007	-0.0030	-0.0062**	-0.0047
trim3	(0.0023)	(0.0022)	(0.0022)	(0.0023)	(0.0026)	(0.0031)	(0.0048)
R^2	0.1516	0.1437	0.1393	0.1432	0.1350	0.1420	0.1713
N	211,641	207,420	187,049	170,775	158,164	101,081	47,560
Time FEs	YES	YES	YES	YES	YES	YES	YES
Municipality FEs	YES	YES	YES	YES	YES	YES	YES
Reg trends	YES	YES	YES	YES	YES	YES	YES

$^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$

Notes: The samples refers to individuals born in the 9 month window around the Armistice (Jan 1943–May 1944) in municipalities with fewer than 200,000 residents. The outcome is age specific earnings between the ages of 30 and 60. All regressions include month × year fixed effects, municipality fixed effects, and region specific time trends. Robust standard errors (in parentheses) are clustered at the municipality level.

Table 8: Age specific blue collar status by trimester of exposure.

	Blue collar at 30	Blue collar at 35	Blue collar at 40	Blue collar at 45	Blue collar at 50	Blue collar at 55	Blue collar at 60
Nazi raid	0.0229**	0.0063	0.0122	0.0172	0.0181	0.0206	0.0122
trim1	(0.0101)	(0.0124)	(0.0136)	(0.0122)	(0.0127)	(0.0160)	(0.0223)
Nazi raid	0.0237***	0.0113	0.0322***	0.0189**	0.0221**	0.0329***	0.0440***
trim2	(0.0072)	(0.0089)	(0.0088)	(0.0089)	(0.0096)	(0.0098)	(0.0140)
Nazi raid	0.0211***	0.0028	0.0152**	0.0162**	0.0166**	0.0264***	0.0377***
trim3	(0.0060)	(0.0070)	(0.0069)	(0.0065)	(0.0069)	(0.0074)	(0.0116)
WWII casualties (SD)	0.0012	0.0037***	0.0041***	0.0041**	0.0047***	0.0037*	0.0060*
trim1	(0.0013)	(0.0014)	(0.0016)	(0.0017)	(0.0017)	(0.0022)	(0.0031)
WWII casualties (SD)	0.0028**	0.0036***	0.0039***	0.0029*	0.0024	0.0033*	0.0036
trim2	(0.0013)	(0.0014)	(0.0015)	(0.0016)	(0.0016)	(0.0020)	(0.0027)
WWII casualties (SD)	0.0008	0.0006	0.0027*	0.0019	0.0023	0.0034	-0.0018
trim3	(0.0013)	(0.0013)	(0.0015)	(0.0016)	(0.0017)	(0.0021)	(0.0028)
R^2	0.0886	0.0966	0.1037	0.1107	0.1161	0.1659	0.1978
N	211,641	207,420	187,049	170,775	158,164	101,081	47,560
Time FEs	YES	YES	YES	YES	YES	YES	YES
Municipality FEs	YES	YES	YES	YES	YES	YES	YES
Reg trends	YES	YES	YES	YES	YES	YES	YES

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: The samples refers to individuals born in the 9 month window around the Armistice (Jan 1943–May 1944) in municipalities with fewer than 200,000 residents. The outcomes is the fraction of workers in blue collar jobs between the ages of 30 and 60. All regressions include month × year fixed effects, municipality fixed effects, and region specific time trends. Robust standard errors (in parentheses) are clustered at the municipality level.

6.2 What if lightening strikes twice?[30]

The stressful event that we consider here is losing one's job. Recent medical studies (see e.g. Boersma and Tamashiro, 2014) have demonstrated that prenatal stress exposure may affect the ability of offspring to deal with stress. In our context, this finding indicates that those exposed to stress in utero may suffer more from stressful events later in life. Besides a direct medical effect, stress vulnerability may also affect job search behavior. Stress vulnerability may affect reservation wages, social networks and the effectiveness of job search strategies. This may in turn explain the large earnings penalty at age 60.

We examine the effects of job loss due to a mass layoff on subsequent earnings and mortality for those exposed to stress in utero. We focus on contract terminations due to mass layoffs because these job separations are more likely to be exogenous at the individual level. While this explanation is plausible, we need to verify that these separations are actually exogenous.

The literature on the effects of job loss due to mass layoffs shows that displaced workers tend to experience significant long-term earnings losses (Jacobson et al., 1993; Ruhm, 1991), lower employment rates (Chan and Stevens, 2001), strong increases in mortality rates for male workers, persisting up to 20 years after job displacement (Sullivan and von Wachter, 2009) and suicide rates and hospitalization due to traffic accidents, alcohol-related diseases, and mental illness (Browning and Heinesen, 2012).[31] Our results are also relevant for an emerging literature that aims to empirically address the issue of dynamic complementarities in skills (Almond et al., 2018; Malamud et al., 2016). We turn to the question of dynamic complementarities at the end of this subsection.

Following Sullivan and von Wachter (2009) we use the standard mass layoff definition: a reduction of at least 30% in total employment between period $t-1$ and t in a firm with more than 25 workers. We match these firm layoff events with individual contract terminations. We match these data for the period from 1983 (aged 40) to 2004 (aged 61).[32]

We need to examine first whether the allocation of mass layoffs to individual workers is indepen-

[30]Adapted text from Almond et al. (2018), section 5.

[31]On the other hand Browning et al. (2006) find no effects of job displacements on stress related diseases. In this study, they do not restrict their sample to workers with a strong attachment to the labor market.

[32]Participation rates for older workers declines rapidly after age 60. At age 60 only about 22% of male workers are still at work (47,560 out of 211,642).

dent from our exposure variable (being exposed in utero to a Nazi raid). Table B2 of Appendix B shows that these layoffs are independent from our exposure variable: there are no effects of in-utero exposure (or general WWII effects) on the probability of working in a firm that experiences a mass layoff. We can therefore proceed and extend the main specification Equation 1 into a more general model that can be used to assess the causal effect of a job loss due to a mass layoff (LO) for workers who have been exposed to in-utero stress. We use the specification:

$$y^a_{imt} = \beta^a_0 + \beta^a_1 Nazi_{mt} + \beta^a_2 LO^a_{it} + \beta^a_3 LO^a_{it} * Nazi_{mt} + \beta^a_4 War_{pt} + \alpha^a_m + \gamma^a_t + \delta^a_{tr} + \epsilon^a_{imt}, \tag{3}$$

where LO^a_{it} is an indicator that equals 1 if an individual i, born at time t loses his job due to a mass layoff at age $a-1$. The other regressors are the same as in Equation 1. As in Equation 1, β^a_1 measures the effect of in-utero exposure to a Nazi raid. The parameter β^a_2 is the effect of the mass layoff reported in other studies on the effects of mass layoffs on earnings (Jacobson et al., 1993; Ruhm, 1991) and mortality (Sullivan and von Wachter, 2009). The triple difference-in-differences parameter β^a_3 measures the (additional) effect of a job loss due to a mass layoff for those who have been exposed in utero to stress. Finally, β^a_4 measures the general WWII effects. When our outcome is earnings, we measure the effect of the mass layoff on next year's earnings. For mortality, we look at the effect of the mass layoff on the probability of dying within the next 10 years.

Table 9 presents the estimates of Equation 3 on next year's earnings from age 45 to 60. The effect of a Nazi raid (β^a_1, see the first row) is remarkably similar to the general treatment effects presented in Table 4, hinting that there is no sorting of individuals exposed in utero to a Nazi raid into firms that experience a mass layoff. Our estimates indicate that a layoff (β^a_2) results in an immediate earnings loss of about 31–34%. This estimate is in line with the evidence of Couch and Placzek (2010), who find effects for the US of about 32–33%. Jacobson et al. (1993) find immediate losses of more than 40%. Interestingly, the estimates of β^a_3 show that those who experienced a mass layoff and have been exposed in utero to maternal stress have an additional earnings penalty at ages 45, 50 and 55 of about 8%, 9% and 15%, respectively. [33] The results in Table 9 tell us that workers not exposed to prenatal

[33]Note that the effect at age 55 rather than at age 60 is most relevant for the large earnings penalty at age 60 (see Table

stress face immediate earnings losses of about 31–34%, while this effect can increase to more than 47% for those exposed to stress in utero.

Table 9: Effect of mass layoff on log earnings in the following year.

	age 45	age 50	age 55	age 60
Nazi raid	-0.0244***	-0.0247***	-0.0233*	-0.0519***
in utero (β_1^a)	(0.0079)	(0.0083)	(0.0123)	(0.0197)
Layoff (β_2^a)	-0.3359***	-0.3234***	-0.3350***	-0.3157***
	(0.0139)	(0.0120)	(0.0163)	(0.0234)
Layoff × Nazi raid	-0.0781*	-0.0996**	-0.1436**	-0.0445
in utero (β_3^a)	(0.0474)	(0.0490)	(0.0625)	(0.0709)
WWII casualties (SD)	-0.0063*	-0.0063*	-0.0097*	-0.0058
in utero (β_4^a)	(0.0034)	(0.0033)	(0.0050)	(0.0070)
R^2	0.1500	0.1427	0.1682	0.2006
N	155,587	145,885	85,302	39,325
Time FEs	YES	YES	YES	YES
Municipality FEs	YES	YES	YES	YES
Reg trends	YES	YES	YES	YES

$^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$

Notes: The samples refers to individuals born in the 9 month window around the Armistice (Jan 1943–May 1944) in municipalities with fewer then 200,000 residents, and refers to individuals who had positive earnings in the period prior to the layoff event. The estimates are carried out for the overall sample as well as for each age specific outcome between the ages of 45 and 60. All regressions include month × year and municipality fixed effects as well as region specific time trends. Standard errors (in parentheses) are clustered at the municipality level.

Finally, we also examine whether a mass layoff leads to increased mortality rates within 10 years following the mass layoff. We report the results of this analysis in Table 10 and show that, in line with the literature (Sullivan and von Wachter, 2009), a job loss leads to increased 10-year mortality rates, but we do not find evidence of additional effects for those who are exposed to in-utero stress.

Taken together, the results in this section indicate that being exposed to prenatal stress leaves a permanent scar that leads to worse labor market outcomes and ultimately lower pensions in retirement. These negative outcomes are due both to the type of job and to interruptions during the working career. Our results on the effects of a mass layoff for earnings indicates that stress vulnerability may

4)

Table 10: Effect of mass layoffs on probability of death in 10 years.

	age 45	age 50	age 55	age 60
Nazi raid	0.0004	-0.0032	-0.0083*	-0.0110
in utero (β_1^a)	(0.0026)	(0.0032)	(0.0044)	(0.0082)
Layoff (β_2^a)	0.0056	0.0042	0.0155**	0.0187*
	(0.0039)	(0.0042)	(0.0067)	(0.0108)
Layoff× Nazi raid	0.0051	-0.0153	0.0102	-0.0335
in utero (β_3^a)	(0.0153)	(0.0139)	(0.0243)	(0.0295)
WWII casualties (SD)	-0.0004	-0.0006	0.0007	-0.0005
in utero (β_4^a)	(0.0008)	(0.0011)	(0.0015)	(0.0026)
R^2	0.0422	0.0461	0.0662	0.1106
N	170,830	158,232	101,124	47,582
Time FEs	YES	YES	YES	YES
Municipality FEs	YES	YES	YES	YES
Reg trends	YES	YES	YES	YES

$^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

Notes: The sample refers to individuals born in the 9 month window around the Armistice (Jan 1943–May 1944) in municipalities with fewer than 200,000 residents, and refers to individuals who had positive earnings in the period prior the layoff event. The estimates are carried out for the overall sample as well as for each age specific outcome between the ages of 45 and 60. All regressions include month × year and municipality fixed effects as well as region specific time trends. Standard errors (in parentheses) are clustered at the municipality level.

be an important mediator in explaining the long term negative effects on labor earnings. In particular, those exposed in utero to stress face higher earnings penalties at later ages after job loss.

Our results showing that a bad start amplifies the negative effects of later life employment shocks suggest that dynamic complementarities in skills may be important. Dynamic complementarities in skills, as defined by Cunha and Heckman (2007) and Almond and Mazumder (2013), refer to the idea that investments made in later periods are more productive when the baseline stock of skills is higher. Assessing this claim empirically is difficult. As argued by Almond and Mazumder (2013), causal inference on dynamic complementarities requires "lightning to strike twice", i.e. it requires exogenous variation in the baseline stock as well as exogenous variation in the second shock later in life. Such situations are rare in practice, but our empirical design combined with an extremely long follow up period allows us to address dynamic complementarities in skills.[34]

7 Robustness checks

7.1 Some sensitivity checks and additional analyses

We perform additional analyses, sensitivity tests and falsification exercises to examine that our model assumptions hold and to see whether our results on earnings, blue collar status and layoffs are stable under alternative specifications.

Testing for common trends. A crucial assumption underlying estimates of Equation 1 is that, in the absence of the intervention, the outcomes for the control group and the (future) treatment group would run in parallel (the common trends assumption). From an empirical perspective this boils down to examining later life outcomes for those born prior to September 8, 1943 in control and treated municipalities. We include differential effects for exposure in utero, in the first year of life, and in the second year of life in Equation 1. In this specification the effects are relative to individuals born in municipalities that did not experience a Nazi raid. For these analyses we extended the time window to the left, i.e. the time window is [-24,9], where zero is September 8th, 1943.

This extension of the model also (implicitly) tests for the relevance of changed parental behavior

[34]However, as noted by Malamud et al. (2016), exogeneity of the two shocks is a necessary, but not sufficient condition to asses whether dynamic complementarities are important. Parents may respond with investments in their children to counter the negative effects of a bad start. This may confound the test for dynamic complementarities in skills.

due to post traumatic stress. Post traumatic stress syndrome (PTSS) may also influence parental behavior and parenting skills when the child grows up (Akresh et al., 2012). In the presence of PTSS effects, one would expect to also find evidence of adverse effects of exposure in the first and/or second year of life. Absence of first year and second year effects may therefore suggest that it is primarily a biological effect, rather than an effect from altered parental behavior post-birth that drives the findings of our main model (Equation 1).[35]

Table 11 reports the results for earnings by age. The top panel (A) reports estimates of the baseline model based on Equation 1 and for the window [-9,9] (first row) and the extended time window [-24,9] (second row of Panel A). Panel B presents estimates of the event study that also includes the effects of the first and second years of exposure. The results in panel A show that the coefficients of interest are hardly affected and that the parameter estimates are slightly larger and more precise when we extend the time window to the left. Importantly, the first and second year effects in Panel B are small and not significantly different from zero, indicating that we cannot reject the assumption of common pre-trends. We have also tested the common trend assumption for all other outcomes (blue collar status, disability, mortality, retirement, unemployment and the mass layoff effects). We report these results in Tables B3, B4 and B5 of Appendix B. We cannot reject the common trends assumption for any of our outcome variables.

In Appendix B we present figures that summarize the results of a more flexible specification for earnings and blue collar status that allows for effects by each month of exposure (Figures B1–B6). The results confirm that we cannot reject the assumption of common trends.

Treatment time variation only. We also estimated our main model (Equation 1) on a sub-sample of municipalities that experienced a Nazi raid. In these analyses we only exploit the random timing of Nazi raids (rather than the random timing and location). This set-up is equivalent to the design of Persson and Rossin-Slater (2018). We report the results of this exercise for earnings, blue collar status and mass layoff effects in Table B6, B7 and B8 of Appendix B. Our results for the age specific effects for earnings change in this specification. In particular, the large and significant effects at later ages

[35]It could also be the case that children aged 1 or 2 are directly affected by witnessing the violent raids of the Nazis. Such trauma effects are likely to be more relevant for older children.

Table 11: Testing the common trend assumption: results of an event study for earnings.

	Earnings at 30	Earnings at 35	Earnings at 40	Earnings at 45	Earnings at 50	Earnings at 55	Earnings at 60
			Panel A				
Baseline model [-9,9] window							
Nazi raid	-0.0218***	-0.0238***	-0.0155**	-0.0177**	-0.0268***	-0.0254**	-0.0551***
in utero	(0.0080)	(0.0075)	(0.0076)	(0.0077)	(0.0085)	(0.0120)	(0.0194)
Baseline model [-24,9] window							
Nazi raid	-0.0224***	-0.0278***	-0.0199***	-0.0235***	-0.0316***	-0.0219**	-0.0502***
in utero	(0.0073)	(0.0064)	(0.0061)	(0.0065)	(0.0075)	(0.0099)	(0.0161)
			Panel B				
First and Second year effects model [-24,9] window							
Nazi raid	-0.0177**	-0.0337***	-0.0137**	-0.0206***	-0.0342***	-0.0225*	-0.0372**
in utero	(0.0084)	(0.0076)	(0.0067)	(0.0069)	(0.0081)	(0.0121)	(0.0190)
Nazi raid	0.0131	-0.0057	0.0038	0.0027	-0.0003	0.0038	0.0205
1st year	(0.0092)	(0.0067)	(0.0058)	(0.0063)	(0.0072)	(0.0112)	(0.0188)
Nazi raid	0.0229	0.0011	0.0043	0.0037	-0.0015	0.0013	0.0042
2nd year	(0.0177)	(0.0068)	(0.0060)	(0.0064)	(0.0073)	(0.0111)	(0.0183)
WWII casualties (SD)	-0.0063**	-0.0048*	-0.0044*	-0.0037	-0.0029	-0.0065*	-0.0062
in utero	(0.0026)	(0.0026)	(0.0023)	(0.0026)	(0.0028)	(0.0036)	(0.0049)
WWII casualties (SD)	-0.0040*	-0.0004	-0.0019	-0.0024	-0.0011	-0.0043	-0.0072
1st year	(0.0023)	(0.0022)	(0.0020)	(0.0023)	(0.0024)	(0.0033)	(0.0052)
WWII casualties (SD)	0.0016	-0.0028	-0.0019	-0.0025	-0.0053**	-0.0028	-0.0050
2nd year	(0.0024)	(0.0023)	(0.0021)	(0.0022)	(0.0024)	(0.0033)	(0.0053)
R^2	0.1391	0.1371	0.1240	0.1324	0.1152	0.1173	0.1357
N	376,895	386,610	354,811	319,900	299,326	191,942	89,267
Time FEs	YES	YES	YES	YES	YES	YES	YES
Municipality FEs	YES	YES	YES	YES	YES	YES	YES
Reg trends	YES	YES	YES	YES	YES	YES	YES

$^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$

Notes: The top row of panel A refers to individuals born in the 9 month window around the Armistice (Jan 1943–May 1944) in municipalities fewer than 200,000 residents, while the bottom row of panel A and panel B refer to individuals born in the 24 months prior Armistice or in the 9 months after Armistice (Sept 1941–May 1944). The columns refer to each age specific outcome between the ages of 30 and 60. All regressions include month × year and municipality fixed effects as well as region specific time trends. Standard errors (in parentheses) are clustered at the municipality level.

are smaller and insignificant. We observe a similar pattern for the earnings effects of a mass layoff (Table B8). The results in the top row of this table are very similar to the earnings effect in Table B6, but the earnings penalty of a mass layoff for those who are exposed in utero to maternal stress are much larger. For blue collar status (B7) the coefficients remain significant and are comparable to the results of the main model (Table 5). These results show that ignoring additional (cross sectional) variation in location can have substantial effects on the parameter estimates and standard errors.

A more flexible regional trend specification. In Table B9 of Appendix B we report the results of our main model, but we replace the regional time trends with a more flexible specification using a set of region by birth month indicators. For earnings, the coefficients of interest are hardly affected (see Table 4). A similar analyses for blue collar status (Table B10) and mass layoff effects (Table B11) shows that those coefficients are also virtually identical to the results of the main model (Table 5 and 9).

Changes in the duration of the gestation. As a further sensitivity test, we explored the effect of changes in the gestation period. In section 4.4 we derived the exposure variable assuming a nine month gestation period. Maternal stress increases the likelihood of a preterm birth (Lilliecreutz et al., 2016). With shorter gestational periods some of those exposed may in fact have been conceived after September 8, 1943, or may have been exposed post birth. This may lead to biased parameter estimates. To examine the sensitivity of our results to the assumed nine month gestation period, we estimated the main model assuming both an eight month and a seven month gestation period. [36] We report the results of this exercise in Tables B12, B13 and B14 of Appendix B. By reducing the gestation period, the coefficients slightly increase in size and significance. The definition of trimesters is less clear with shorter gestational periods. We therefore did not pursue the effects of eight and seven months gestation on trimester specific effects.

Capping municipality size. We also explored the role that municipality size may play in our results. We obtained the previous results for municipalities with less than 200,000 inhabitants. The nature of the war was different in larger cities. Larger cities generally, due to political and strategic reasons, suffered more from mass destruction of buildings, bombings and nutritional shortages. This

[36] We trim the data from the right; for an eight month gestation period we take births in the [-9,8] window around September 8, 1943. For a seven month gestation period we take the [-9,7] window around September 8, 1943.

additional suffering may imply that, for larger municipalities, the general war effects dominate stress effects induced by the Nazi raids. We plotted the coefficients of in-utero exposure to a Nazi raid and the general war effects for earnings by varying municipality size in Figures B7 and B8. Figure B7 show that the effects of in-utero exposure to a Nazi raid are smaller and insignificant for larger municipalities. For general war effects (B8) we see the opposite pattern. The results for blue collar status (available upon request) show a similar dynamic, where the inclusion of large cities increases the effects of general WWII intensity but decreases the effects of Nazi raids.

Falsification tests. Finally, we further verify our identification assumptions and demonstrate the statistical power of our inferences by conducting falsification tests where we assign a pseudo-treatment. We randomly assign the Nazi raids to each individual in our sample. If, after conditioning on municipality fixed effects, systematic differences remain in the assignment of the treatment status across municipalities, then this would show up in the distribution of our estimates. If our identification strategy is valid, the estimates using these pseudo-samples should be centered around zero. Figures B9 and B10 plot the distribution of the t-statistics from 5,000 estimated pseudo-treatment effects for earnings at age 30 and blue collar status at age 30, respectively (results for other ages as well as for the effects of a mass layoff are available upon request). As expected, both distributions are centered around zero and the t-stats of our main analyses (indicated with the vertical dotted red line) are in the far end of the left (earnings) and right (blue collar status) tails of the distributions. The share of t-values that (in absolute terms) exceeds the t-statistics from our main model is less than 1%.

7.2 Dealing with selection effects

Our identification strategy assumes that individuals did not anticipate the series of events that unfolded soon after the Armistice, and that Nazi raids were randomly assigned. With respect to the random assignment of raids, in all analyses we include municipality fixed effects (in addition to time fixed effects and region specific time trends), so our results only require the weaker assumption of conditional random assignment. The results in Appendix C and the falsification test in subsection 7.1 provide additional evidence that this is a credible identifying assumption.

The no anticipation assumption is required to exclude endogenous fertility and residential mobility responses. The historical background information provided in Section 3 makes it plausible that individuals did not anticipate the series of events that followed the Armistice, and that the assumption of no anticipation is plausible. We support these assumptions with additional information and examine other possible sources of selection bias.

Selection issues at conception, in utero and in later life

Confounding due to selection can take place at several stages: *i)* at conception; *ii)* in-utero; *iii)* later in life before we observe the individuals in 1974; *iv)* after 1974. With respect to selection at conception, our sample pertains to all births in the 9-month window around the Armistice (January 1943–May 1944). This restriction guarantees that our sample includes all births conceived prior to the unexpected proclamation of the Armistice. This universal coverage supports our claim that *fetuses* in the two subgroups were conceived in a similar environment and that fertility choices were made with similar priors. Still, one might argue that conception rates varied with socio-economic status. In our setting socio-economic characteristics, for which we cannot control, affect only the choice of having a child and not the likelihood of treatment, which is still exogenous from the individual's point of view.[37]

To deal with in-utero selection, we ideally would have birth records at the time of exposure. Unfortunately, these are not available. However, we are able to retrieve regional data on cause specific mortality for complications during pregnancy or at birth in the WWII era. If this mortality rate is higher in periods of a Nazi raid, then this implies that fewer women give birth in these periods. This may bias parameter estimates if these women are systematically different from women who give birth. We regressed regional mortality rates due to pregnancy complications from 1941 to 1946 on the number of Nazi raids and WWII casualties. We ran both regressions at the province level after controlling for year and region fixed effects. We find some effects of general WWII intensity, but no effect of the incidence of Nazi violence on mortality due to complications during pregnancy (see Table B15 of Appendix B).

[37]Since we control for municipality fixed effects, this would imply that there might be within municipality heterogeneity with respect to socio-economic status in conception choices. If this is an important source of bias, then one would expect to find sizeable changes in the parameter estimates when we vary the sample with respect to the size of the municipality. We do not see this pattern in the estimates in Figures B7 and B8.

Selection after conception can take place in utero (miscarriages), between birth and the first time that we observe the individuals at later ages. Moreover, some individuals may leave the private sector after we first observe them in 1974 in their working histories. In order to test for these selection effects we estimated the following model:

$$CS^a_{mt} = \beta^a_0 + M_t + \beta^a_1 M_t * Nazi_{mt} + \beta^a_2 War_{pt} + \alpha^a_m + \delta^a_{tr} + \epsilon^a_t, \tag{4}$$

where CS^a_{mt} is the cohort size at age a (a = 30, 40, 60 and 70 years) in municipality m at time t (for t = Jan 1943, . . . , May 1944), M_t a month dummy, $Nazi_{mt}$ is an indicator that equals one if municipality m at time t was exposed to a Nazi raid and War_{pt} is the number of war casualties in province p at time t. α^a_m, δ^a_{tr} and ϵ^a_{mt} are municipality fixed effects, region specific time trends and an idiosyncratic error term. Our main interest is the coefficient β^a_1, associated with the interaction $M_t \times Nazi_{mk}$. A significant negative effect indicates a smaller cohort size in the months that a municipality was subject to a Nazi raid (and vice versa). We report this coefficient in Table B16 of Appendix B. Our results show that the cohort size of a municipality is not significantly different in the months when a Nazi raid took place. We also find no general WWII effects on cohort sizes of our sample in the 9 month window around the Armistice. These findings suggests that selection effects for reasons *i)-iv)* are negligible.

Selective mobility

The unexpected declaration of the Armistice is likely to rule out endogenous residential mobility in the short time window that we use in our analyses. Moreover, movements across Italian provinces during WWII were very limited due to the destruction of railroads and transport networks (Baldoli and Knapp, 2012). To empirically test for selective mobility responses we select all municipalities that experienced a Nazi raid in the 9 months following the Armistice and estimate a model that relates the cohort size of a municipality to leads and lags of exposure to the Nazi raid. A reduction in monthly cohort sizes prior to the actual Nazi raid may indicate that civilians are moving out of municipalities that were (later) subject to a Nazi raid. A cohort size reduction in the months preceding a Nazi raid would also suggest non-random locations for the Nazi raids. In Figures B11 and B12 we report coefficients of the leads and lags of the municipality's exposure to the Nazi raid. The figures give no indication that the cohort size varied before (or after) the month that the municipality was subject to a Nazi raid. This suggests that residential mobility in affected municipalities was limited.

8 Discussion and conclusions

This paper is the first to carefully examine the impact of prenatal stress exposure on long term labor market outcomes. Previous work in medical sciences, epidemiology and economics has documented negative effects of prenatal stress exposure on birth outcomes, mental health, cognition and education. These findings plausibly translate into worse labor market outcomes throughout an individual's working career. To test whether these plausible negative effects are empirically relevant, we exploit a unique natural experiment that involved short lived, randomly placed violent Nazi raids across municipalities after September 8^{th}, 1943, when the Italian Kingdom ceased hostilities against the Allied forces in WWII. We use administrative data on the universe of private sector workers in Italy and link these data to unique historical databases with detailed information about war casualties and the assignment of the Nazi raids across space (Municipality) and time.

We use a generalized Difference-in-Differences model to examine the effect of in-utero stress exposure on later life earnings, occupation, pensions, retirement, mortality, disability and unemployment outcomes. We find that prenatal stress exposure leaves a permanent scar on individuals, reducing earnings by about 2% at the start of their working careers. This effect increases to about 5.5% at age 60. These lower earnings translate into lower pensions. The lower earnings are partly due to the type of jobs that people hold and interruptions in their working careers due to unemployment.

Medical studies have documented strong associations between in-utero stress and stress vulnerability later in life. We find that a bad start (i.e. prenatal stress exposure) exacerbates the negative effects of later life job loss on earnings, deepening the negative impact on earnings at later ages. The effects of job loss on earnings are substantial, between 31–34% for all workers and up to 47% for workers exposed to prenatal stress.

The results for job loss due to a mass layoff also suggest that dynamic complementarities in skills (Cunha and Heckman, 2007) are important. Dynamic complementaries refer to the idea that investments in later life are more productive when the initial stock of skills is higher. Conversely, as we find, a bad start may amplify adverse effects of negative shocks later in life. The empirical literature on dynamic complementarities is thin. This is largely due to data requirements. As argued by Almond and Mazumder (2013), empirical work in this area requires exogenous variation in the baseline stock of skills as well as exogenous variation in a second shock later in life. Our unique empirical design, aided by the very long follow up period and detailed data, satisfies this condition.

Our findings are in accordance with results from earlier economic studies that focused on cognition, years of education and mental health. It is, however, difficult to directly relate the findings of these studies to our findings for earnings, blue collar worker status and other labor outcomes. Unlike previous studies, our stress measure is meaningfully different from the bereavement measure used in previous work. According to several clinical studies and disaster research, a stressor is likely to be more severe in cases of interpersonal events (rape, assault, torture), as compared to a non-interpersonal occurrences (accident, illness, bereavement) (Breslau et al., 1998; Green et al., 2000; Krupnick et al., 2004; Shalev and Freedman, 2005). Persson and Rossin-Slater (2018) report increases in salivary cortisol levels of 2.75 nmol/l in bereaved groups, while Heissel et al. (2018) find an increase of 3.58 nmol/l from community violent crime exposure in adolescents.

In addition to a different stressor, the outcomes in our study are also different from the outcomes in previous work, and the subjects involved are much older. For our set of older males we find (see Appendix A, Table A2) that those aged 60–65, who were exposed in utero to stress, are about 3.6% more likely (about 13% increase) to purchase drugs for treating diseases of the nervous system and mental disorders, and they have 12.8% higher medical expenditures. Persson and Rossin-Slater (2018) find a 13 and 8% increase in the likelihood of consuming prescription drugs for anxiety and depression.

Mean expenditures on drugs for the nervous system and mental disorders are 22 euros per patient per year. Italy counts about 17.9 million people aged 60 or older, resulting in total expenditures for drugs of the nervous system and mental disorders of about 393 million euro per year for this age group. This quick calculation shows that additional medical expenses due to prenatal stress can be substantial. The total cost associated with prenatal stress is of course substantially higher since this cost also include other medical costs, reduced earnings and increased social security spending. Using our point estimates for drug expenditures and earnings at age 60, we find an implied elasticity of earnings at age 60 with respect to medical expenditures for diseases of the nervous system and mental disorders of about 0.43.[38]

What about external validity? Most studies in the literature on long run effects of adversities early in life concern historical, "exotic", situations and events (Almond et al., 2018). This paper is no exception. We feel, however, that the context in this paper extends to settings observed today, especially developing countries. Unfortunately, war related conflicts that involve aggression and violence against the civilian population remain relevant today. Stressful conditions are not limited to war like situations, but also hold for deprived neighborhoods, where crime rates, unemployment and poverty rates are high. Further, the current COVID-19 pandemic is associated with fear and stress for the health, economic security and well-being of individuals and their families. In all these circumstances, chronic or acute stress plays an important role for the current cohort as well as for cohorts in utero.

Our finding that those exposed to prenatal stress enter the labor market in worse jobs and are more vulnerable to adversities later in life suggests that there can be an important role for public policy. It also suggests that programs targeted at vulnerable families, in particular (pregnant) women and children, can be very effective in mitigating the negative effects of a bad start and the consequences of adversities later in life.

[38]The reduction in log earnings at age 60 equals 5.5%, while the relative change in expenditures equals 12.8%.

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A Appendix: Is it stress?

In-utero stress exposure is proxied by being in utero at the time of a Nazi raid in a municipality where the individual was born. We use information of an external data base on health expenditures to validate our interpretation of the effects of this proxy. The Health Search/CSD Patient Database is a nation-wide representative sample of Italian adults, containing electronic patient level clinical records (ECRs) on diagnosis and prescriptions collected by General Practitioners (GPs).[39] The dataset also includes the patient's date and municipality of birth. Following a GP visit patients obtain their prescriptions which include information on diagnosis and types of drugs classified using the ATC (Anatomic Therapeutic Chemical) drug classification codes. With this information we can track the specific medical condition for which the drugs is prescribed and next compute drug expenditures for a specific disease at the patient level.

From the database we select all individuals born in the nine month time window surrounding September 8, 1943 (see Section 4) and compute annual patient level disease specific expenditures. These disease specific expenditures are then regressed on an indicator for in-utero exposure to Nazi raid, controlling for age, WWII casualties at the province level, GP fixed effects, Municipality fixed effects and regional trends.

The results of this regression are displayed in table A1. The table shows that the Nazi raids have a sizeable and significant effect on health expenditure for diseases of the nervous system and mental disorders (column 1). The coefficient of 14.7 amounts to a 12.8 percentage increase of annual individual expenditures on drugs for diseases of the nervous system and mental disorders (mean expenditures are 115 euro). This finding is in line with the medical literature that documents strong associations between stress exposure and various psycho-pathologies later in life such as memory problems, decreased learning function, depression and dementia (Checkley, 1996; Heffelfinger and Newcomer, 2001; Selten et al., 1999). Important for our study, the findings give credit to our interpretation of the effect of the Nazi raids as being primarily a stress effect.

[39]The database involves the ECRs of patients of a group of 900 GPs, representative of the Italian population, covering 1.8 million patients between 2004-2018. As Italian residents receive primary care services for free and are assigned to GPs on the basis of geographical proximity, the data are free of selection issues in the choice of GPs. Moreover, any healthcare service utilisation in Italy is subject to prescription or referral by a GP.

Table A1: Effect of prenatal exposure to Nazi raids on health expenditure types.

	(1) Neuro/ Mental.	(2) Cardio/ Diabetic	(3) Respir. syst.	(4) Hormone syst.	(5) Neoplasms	(6) Skin	(7) Musculo/ skeletal
Nazi raid	14.68607**	-.8137636	.2053017	.0604989	-6.213564	-1.089221	-1.453942
in utero	7.027016	3.318328	.395272	.5167893	4.91778	.7845821	1.146788
WWII casualties (SD)	-2.4008	-.1176307	-.0043448	.8692845	-4.107407	-.2607241	-.8289242*
in utero	2.271634	1.307562	.0833904	.6241549	3.653428	.2934628	.4665404
Time FEs	YES	YES	YES	YES	YES	YES	YES
Municipality FEs	YES	YES	YES	YES	YES	YES	YES
Reg trends	YES	YES	YES	YES	YES	YES	YES
GP FEs	YES	YES	YES	YES	YES	YES	YES
AGE	YES	YES	YES	YES	YES	YES	YES
N	82,299	82,299	82,299	82,299	82,299	82,299	82,299

$^{***}p < 0.01,^{**}\ p < 0.05,^{*}\ p < 0.1$

Notes: The sample of 82,299 refers to individuals born in the 9 month window around the Armistice (Sept 1941–May 1944) and corresponds to patients managed by 468 GPs between 2004 and 2010. The expenditure refers to annual outpatient drug expenditure expressed in euro. All regressions control for WWII intensity and include month × year, municipality, GP, age and wave fixed effects as well as region specific time trends. Standard errors are clustered at the municipality level.

Table A2: Effect of prenatal exposure to Nazi raids on probability of consuming various drug treatment types.

	(1) Neuro/ Mental.	(2) Cardio/ Diabetic	(3) Respir. syst.	(4) Hormone syst.	(5) Neoplasms	(6) Skin	(7) Musculo/ skeletal
Nazi raid	.0363725*	.009888	.0025408	-.0028159	-.0011372	-.0059475	-.031536
in utero	.0206139	.0165968	.0058697	.010909	.0042813	.00794	.0207016
WWII casualties (SD)	-.0069781	-.0038541	.0002067	-.0047323	-.0033508*	-.0028436	.0004971
in utero	.0062256	.0055323	.0015742	.0038085	.0018029	.0032363	.0079697
Time FEs	YES	YES	YES	YES	YES	YES	YES
Municipality FEs	YES	YES	YES	YES	YES	YES	YES
Reg trends	YES	YES	YES	YES	YES	YES	YES
GP FEs	YES	YES	YES	YES	YES	YES	YES
AGE	YES	YES	YES	YES	YES	YES	YES
N	82,299	82,299	82,299	82,299	82,299	82,299	82,299

$^{***}p < 0.01,^{**}\ p < 0.05,^{*}\ p < 0.1$

Notes: The sample of 82,299 refers to individuals born in the 9 month window around the Armistice (Sept 1941–May 1944) and corresponds to patients managed by 468 GPs between 2004 and 2010. The dependent variable is an indicator dummy for positive expenditure referring to annual outpatient drug expenditure expressed in euro. All regressions control for WWII intensity and include month × year, municipality, GP, age and wave fixed effects as well as region specific time trends. Standard errors are clustered at the municipality level.

B Appendix: Additional results

B.1 Additional descriptive statistics

Table B1: Descriptive statistics on Nazi raids episodes between Sept 1943 - May 1944 in municipalities with fewer than 200,000 residents.

	N. obs.	Mean	Std. Dev.	Min	Max	p1	p50	p99
Italy								
Length in days	1603	1.4	2.67	1	54	1	1	14
Number of victims	1603	3.27	7.35	1	130	1	1	29
Proportion of women (%)	1603	9.53	25.99	0	100	0	0	100
Proportion of children (%)	1603	1.89	11.49	0	100	0	0	80
Proportion of men (%)	1603	88.58	28.57	0	100	0	100	100
Proportion of partisans (%)	1603	17.2	36.45	0	100	0	0	100
Proportion of searches (%)	1603	30.88	46.21	0	100	0	0	100
Proportion of retaliations (%)	1603	10.67	30.88	0	100	0	0	100
Northwest								
Length in days	248	1.77	3.43	1	36	1	1	21
Number of victims	248	4.9	9.26	1	97	1	2	51
Proportion of women (%)	248	5.74	19.52	0	100	0	0	100
Proportion of children (%)	248	.4	6.35	0	100	0	0	0
Proportion of men (%)	248	93.86	20.41	0	100	0	100	100
Proportion of partisans (%)	248	42.81	46.71	0	100	0	0	100
Proportion of searches (%)	248	51.61	50.08	0	100	0	100	100
Proportion of retaliations (%)	248	15.73	36.48	0	100	0	0	100
Northeast								
Length in days	181	1.67	4.64	1	54	1	1	29
Number of victims	181	3.41	10.42	1	130	1	1	32
Proportion of women (%)	181	7.45	25.2	0	100	0	0	100
Proportion of children (%)	181	.68	7.57	0	100	0	0	19.35
Proportion of men (%)	181	91.87	26.34	0	100	0	100	100
Proportion of partisans (%)	181	35.01	45.98	0	100	0	0	100
Proportion of searches (%)	181	32.04	46.79	0	100	0	0	100
Proportion of retaliations (%)	181	17.68	38.26	0	100	0	0	100
Center								
Length in days	394	1.36	2.2	1	27	1	1	11
Number of victims	394	3.24	5.11	1	46	1	1	29
Proportion of women (%)	394	6.4	20.66	0	100	0	0	100
Proportion of children (%)	394	1.68	10.67	0	100	0	0	66.67
Proportion of men (%)	394	91.92	23.63	0	100	0	100	100
Proportion of partisans (%)	394	22.9	40.63	0	100	0	0	100
Proportion of searches (%)	394	45.94	49.9	0	100	0	0	100
Proportion of retaliations (%)	394	8.63	28.12	0	100	0	0	100
South								
Length in days	780	1.24	1.84	1	28	1	1	8
Number of victims	780	2.74	6.67	1	125	1	1	25
Proportion of women (%)	780	12.81	29.75	0	100	0	0	100
Proportion of children (%)	780	2.75	13.64	0	100	0	0	100
Proportion of men (%)	780	84.45	32.71	0	100	0	100	100
Proportion of partisans (%)	780	2.04	13.78	0	100	0	0	100
Proportion of searches (%)	780	16.41	37.06	0	100	0	0	100
Proportion of retaliations (%)	780	8.46	27.85	0	100	0	0	100

Notes: The numbers refer to an overall sample of 1,603 episodes between Sept 1943 – May 1945 occurring in Italian municipalities with resident population under 200,000.

B.2 Layoffs assignment

Table B2: Layoff event random assignment robustness check.

	layoff at anytime	layoff at 45	layoff at 50	layoff at 55	layoff at 60
Nazi raid	-0.0013	0.0001	-0.0018	-0.0006	0.0017
in utero	(0.0027)	(0.0018)	(0.0024)	(0.0023)	(0.0026)
WWII casualties (SD)	0.0000	-0.0004	-0.0009	0.0001	0.0001
in utero	(0.0013)	(0.0009)	(0.0011)	(0.0009)	(0.0011)
R^2	0.0426	0.0454	0.0513	0.0528	0.0666
N	283,741	187,135	170,830	158,232	101,124
Time FEs	YES	YES	YES	YES	YES
Municipality FEs	YES	YES	YES	YES	YES
Reg trends	YES	YES	YES	YES	YES

$^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

Notes: The samples refers to individuals born in the 9 month window around the Armistice (Sept 1941–May 1944) in municipalities with fewer than 200,000 residents, and refer to individuals who had positive earnings in the period prior the layoff event. The estimates are carried out for the overall sample as well as for each age specific outcomes between the ages of 45 and 60. All regressions include month × year and municipality fixed effects as well as region specific time trends. Standard errors (in parentheses) are clustered at the municipality level.

B.3 Common trends test

Table B3: Testing the common trend assumption: results of an event study for blue collar status.

	Blue collar at 30	Blue collar at 35	Blue collar at 40	Blue collar at 45	Blue collar at 50	Blue collar at 55	Blue collar at 60
			Panel A				
Baseline model [-9,9] window							
Nazi raid	0.0222***	0.0028	0.0176***	0.0172***	0.0184***	0.0277***	0.0370***
in utero	(0.0055)	(0.0067)	(0.0063)	(0.0062)	(0.0065)	(0.0072)	(0.0109)
Baseline model [-24,9] window							
Nazi raid	0.0312***	0.0068	0.0217***	0.0218***	0.0234***	0.0272***	0.0360***
in utero	(0.0045)	(0.0054)	(0.0051)	(0.0051)	(0.0056)	(0.0061)	(0.0089)
			Panel B				
First and Second year effects model [-24,9] window							
Nazi raid	0.0291***	0.0053	0.0224***	0.0248***	0.0266***	0.0299***	0.0423***
in utero	(0.0052)	(0.0060)	(0.0061)	(0.0060)	(0.0064)	(0.0079)	(0.0116)
Nazi raid	-0.0013	-0.0001	0.0041	0.0075	0.0076	0.0076	0.0116
1st year	(0.0047)	(0.0048)	(0.0057)	(0.0055)	(0.0057)	(0.0071)	(0.0104)
Nazi raid	-0.0043	-0.0039	-0.0032	0.0002	0.0003	-0.0007	0.0047
2nd year	(0.0048)	(0.0042)	(0.0049)	(0.0053)	(0.0055)	(0.0069)	(0.0105)
WWII casualties (SD)	0.0018	0.0032*	0.0041**	0.0028	0.0018	0.0033	0.0003
in utero	(0.0017)	(0.0017)	(0.0018)	(0.0019)	(0.0019)	(0.0024)	(0.0032)
WWII casualties (SD)	0.0018	0.0008	0.0032*	0.0035**	0.0017	0.0044**	0.0041
1st year	(0.0015)	(0.0016)	(0.0018)	(0.0018)	(0.0018)	(0.0021)	(0.0032)
WWII casualties (SD)	0.0041**	0.0041***	0.0024	0.0043**	0.0031*	0.0055**	0.0061*
2nd year	(0.0017)	(0.0015)	(0.0018)	(0.0018)	(0.0018)	(0.0023)	(0.0032)
R^2	0.0805	0.0818	0.0876	0.0936	0.0997	0.1408	0.1645
N	376,895	386,610	354,811	319,900	299,326	191,942	89,267
Time FEs	YES	YES	YES	YES	YES	YES	YES
Municipality FEs	YES	YES	YES	YES	YES	YES	YES
Reg trends	YES	YES	YES	YES	YES	YES	YES

$^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

Notes: The top row of panel A refers to individuals born in the 9 month window around the Armistice (Sept 1941–May 1944), while the bottom row of panel A and panel B refer to individuals born in the 24 month prior Armistice to 9 month after Armistice window (Sept 1941 - May 1944), the columns refer to each age specific outcomes between the ages of 30 and 60. All regressions include month × year and municipality fixed effects as well as region specific time trends. Standard errors (in parentheses) are clustered at the municipality level.

Table B4: Testing the common trend assumption: results of an event study for other outcomes.

	Disability before 60	Dead before 60	Age at retirement	First pension (log)	No. Unemployed before 60
			Panel A		
Baseline model [-9,9] window					
Nazi raid	0.0007	-0.0028	0.0595	-0.0209**	0.0309*
in utero	(0.0018)	(0.0028)	(0.0562)	(0.0093)	(0.0159)
Baseline model [-24,9] window					
Nazi raid	0.0010	-0.0007	0.0452	-0.0224***	0.0241*
in utero	0.0025)	(0.0024)	(0.0471)	(0.0081)	(0.0135)
			Panel B		
First and Second year effects model [-24,9] window					
Nazi raid	-0.0011	0.0006	0.0295	-0.0212**	0.0166
in utero	(0.0017)	(0.0028)	(0.0557)	(0.0102)	0.0152
Nazi raid	-0.0017	0.0017	0.0263	0.0014	-0.0158
1st year	(0.0015)	(0.0024)	(0.0476)	(0.0086)	(0.0122)
Nazi raid	-0.0029*	0.0021	-0.1292**	0.0011	0.0062
2nd year	(0.0017)	(0.0027)	(0.0562)	(0.0091)	(0.0140)
WWII casualties (SD)	0.0003	0.0006	0.0320*	-0.0060*	0.0021
in utero	(0.0006)	(0.0009)	(0.0179)	(0.0031)	(0.0068)
WWII casualties (SD)	-0.0005	0.0000	0.0117	-0.0042	0.0075
1st year	(0.0006)	(0.0009)	(0.0186)	(0.0034)	(0.0057)
WWII casualties (SD)	0.0011**	-0.0002	0.0325*	-0.0040	0.0088*
2nd year	(0.0005)	(0.0008)	(0.0244)	(0.0030)	(0.0049)
R^2	0.1391	0.0200	0.1339	0.0461	0.0745
N	521,486	521,486	445,813	445,813	521,486
Time FEs	YES	YES	YES	YES	YES
Municipality FEs	YES	YES	YES	YES	YES
Reg trends	YES	YES	YES	YES	YES

$^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$

Notes: The top row of panel A refers to individuals born in the 9 month window around the Armistice (Sept 1941–May 1944), while the bottom row of panel A and panel B refer to individuals born in the 24 month prior Armistice to 9 month after Armistice window (Sept 1941 - May 1944), the columns refer to each outcome separately. All regressions include month × year and municipality fixed effects as well as region specific time trends. Standard errors (in parentheses) are clustered at the municipality level.

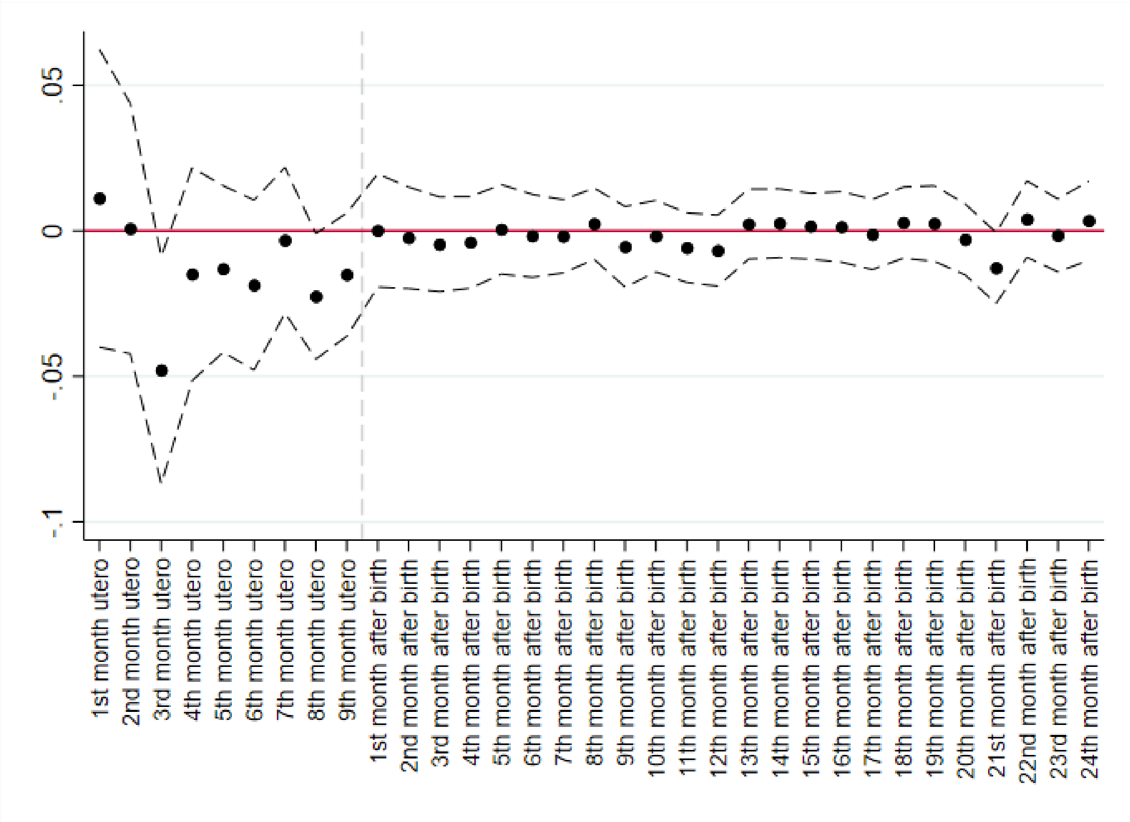
Table B5: Testing the common trend assumption: results of an event study for layoff effect on wages.

	age 45	age 50	age 55	age 60
		Panel A		
Baseline model [-9,9] window				
Nazi raid	-0.0244***	-0.0247***	-0.0233*	-0.0519***
in utero	(0.0079)	(0.0083)	(0.0123)	(0.0197)
Layoff × Nazi raid	-0.0781*	-0.0996**	-0.1436**	-0.0445
in utero	(0.0474)	(0.0490)	(0.0625)	(0.0709)
Baseline model [-24,9] window				
Nazi raid	-0.0267***	-0.0281***	-0.0195*	-0.0488***
in utero	(0.0066)	(0.0072)	(0.0102)	(0.0163)
Layoff × Nazi raid	-0.0702	-0.1417***	-0.1722***	-0.1151*
in utero	(0.0464)	(0.0486)	(0.0603)	(0.0662)
		Panel B		
First and Second year effects model [-24,9] window				
Nazi raid	-0.0244***	-0.0285***	-0.0161	-0.0373*
in utero	(0.0073)	(0.0082)	(0.0130)	(0.0221)
Layoff × Nazi raid	-0.0667	-0.1411***	-0.1735***	-0.1187*
in utero	(0.0469)	(0.0488)	(0.0602)	(0.0673)
Nazi raid	0.0007	0.0008	0.0066	0.0204
1st year	(0.0062)	(0.0069)	(0.0115)	(0.0194)
Layoff × Nazi raid	0.0256	-0.0305	-0.0302	-0.0830
1st year	(0.0344)	(0.0348)	(0.0440)	(0.0702)
Nazi raid	0.0054	-0.0035	0.0020	0.0076
2nd year	(0.0063)	(0.0068)	(0.0114)	(0.0194)
Layoff × Nazi raid	0.0242	0.0380	0.0110	0.0284
2nd year	(0.0348)	(0.0296)	(0.0522)	(0.0552)
R^2	0.1355	0.1207	0.1363	0.1582
N	292,821	275,090	165,434	73,987
Time FEs	YES	YES	YES	YES
Municipality FEs	YES	YES	YES	YES
Reg trends	YES	YES	YES	YES

$^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

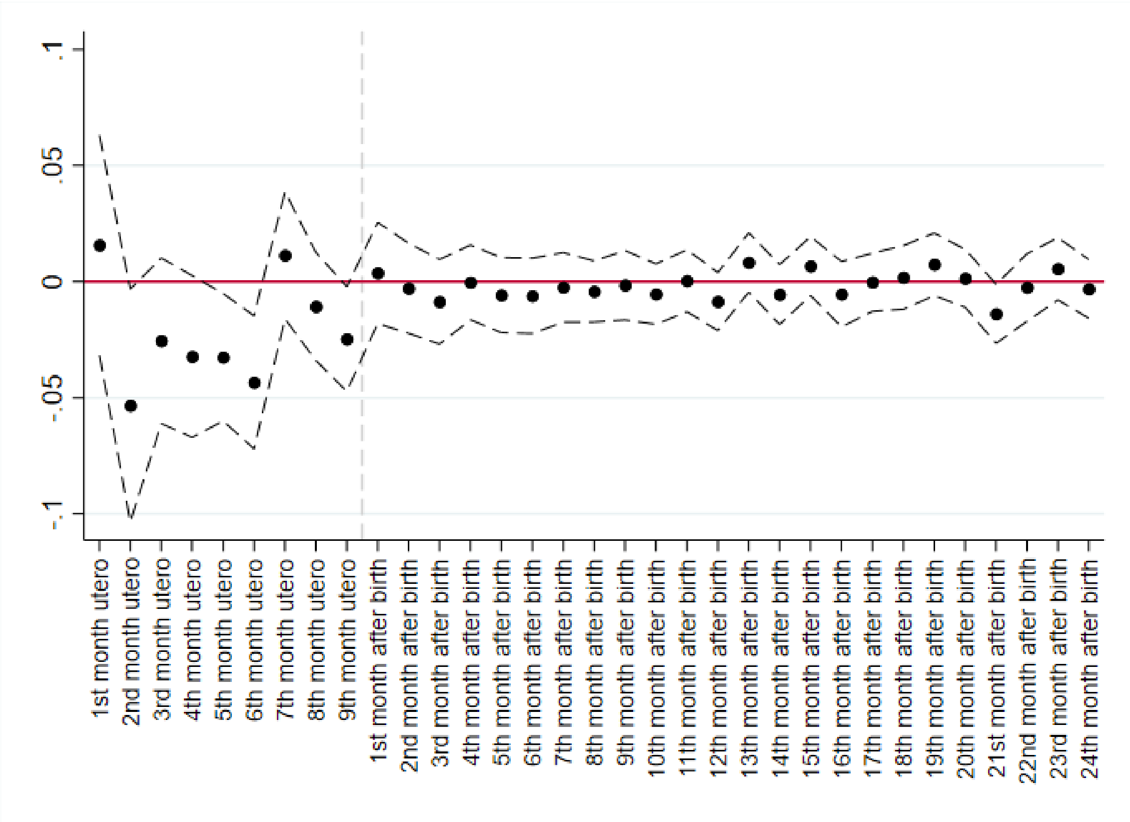
Notes: The top row of panel A refers to individuals born in the 9 month window around the Armistice (Sept 1941–May 1944), while the bottom row of panel A and panel B refer to individuals born in the window -24,9 month around Armistice (Sept 1941 – May 1944), the columns refer to the separate outcomes. All regressions include month × year and municipality fixed effects as well as region specific time trends. Standard errors (in parentheses) are clustered at the municipality level.

Figure B1: Month-by-month exposure to Nazi raids - earnings at the age of 30.



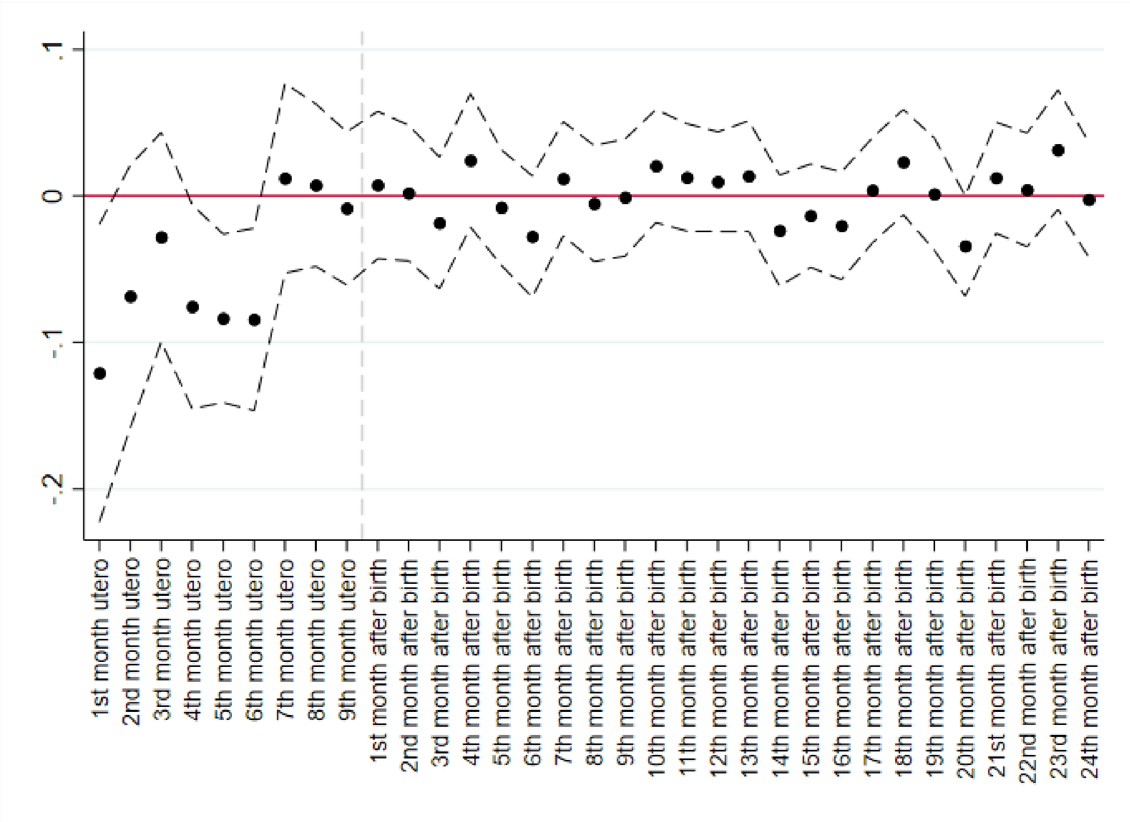
Notes: The figure shows coefficient estimates of the baseline model in the [-24,9] window around the Armistice, where the horizontal axis depicts the timing of each Nazi raid episode with respect to the affected individuals, and is constructed by taking lags and leads of exposure for each cohort/municipality of birth pair. As standard in the baseline specification, the model includes time fixed effects (year/month), municipality fixed effects and regional trends. The confidence interval bans corresponds to 95% threshold.

Figure B2: Month-by-month exposure to Nazi raids - earnings at the age of 50.



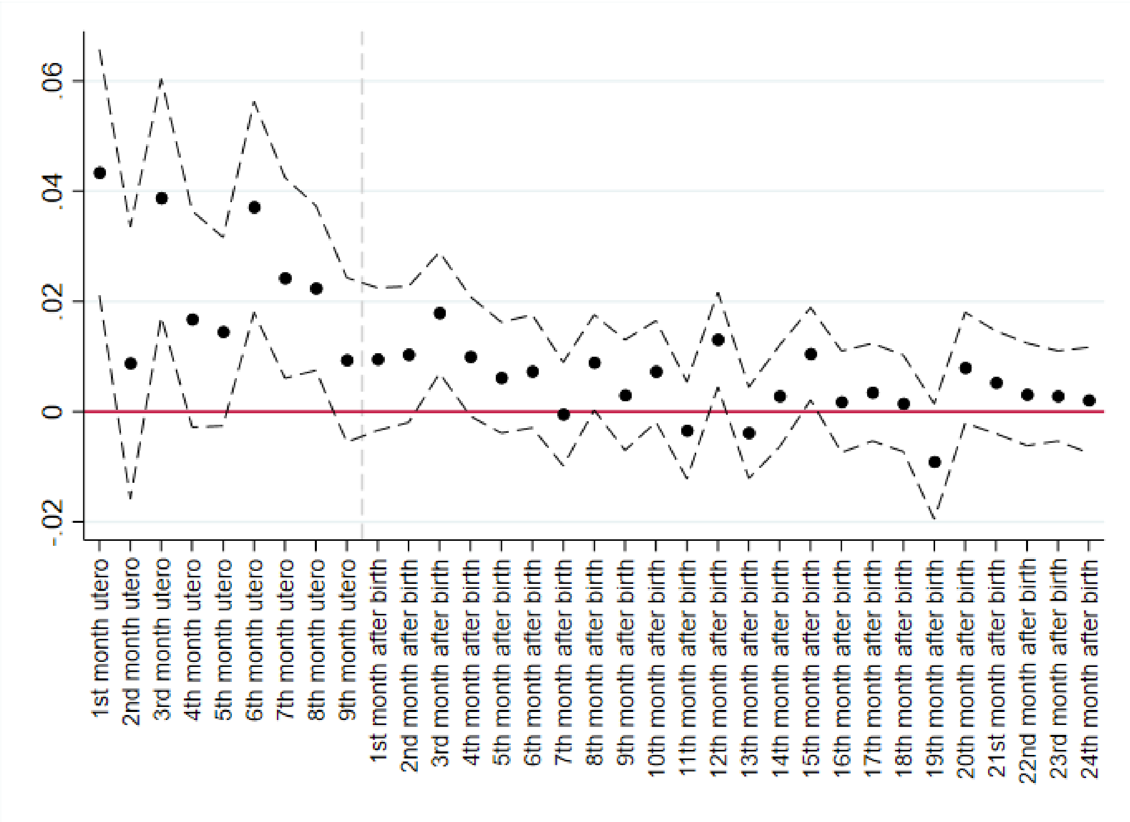
Notes: The figure shows coefficient estimates of the baseline model in the [-24,9] window around the Armistice, where the horizontal axis depicts the timing of each Nazi raid episode with respect to the affected individuals, and is constructed by taking lags and leads of exposure for each cohort/municipality of birth pair. As standard in the baseline specification, the model includes time fixed effects (year/month), municipality fixed effects and regional trends. The confidence interval bans corresponds to 95% threshold.

Figure B3: Month-by-month exposure to Nazi raids - earnings at the age of 60.



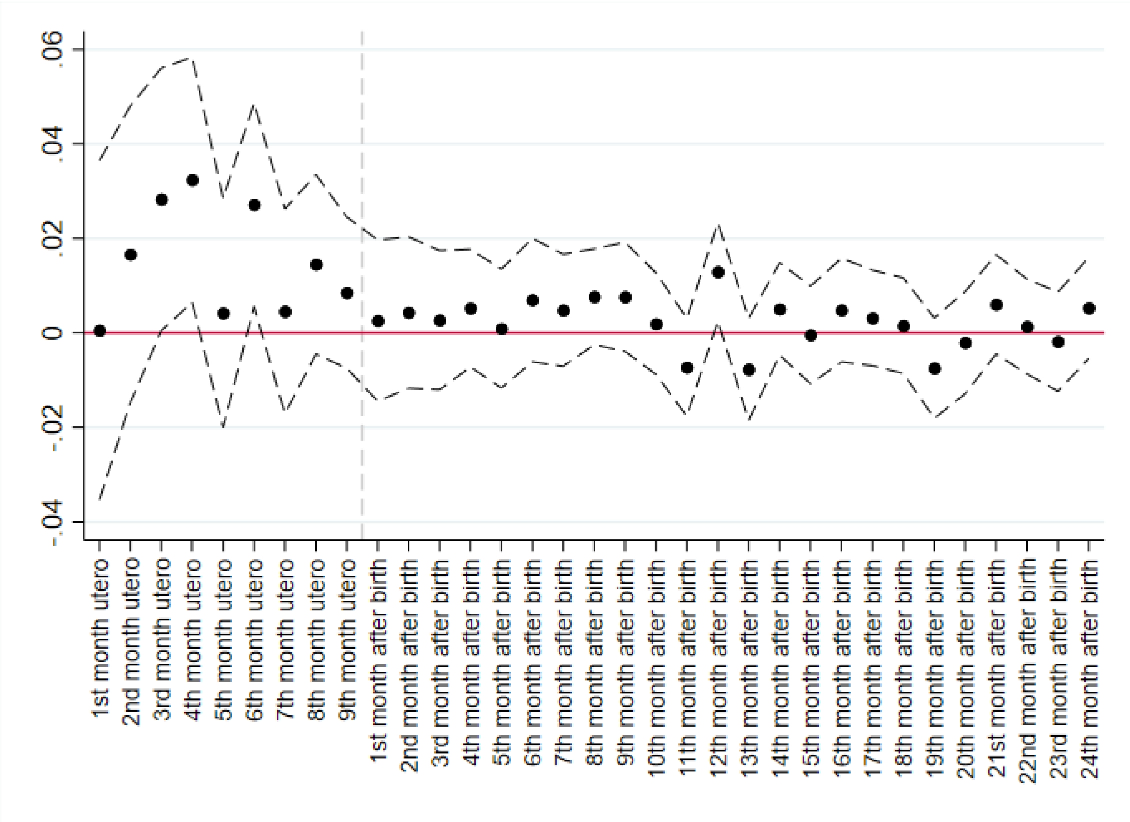
Notes: The figure shows coefficient estimates of the baseline model in the [-24,9] window around the Armistice, where the horizontal axis depicts the timing of each Nazi raid episode with respect to the affected individuals, and is constructed by taking lags and leads of exposure for each cohort/municipality of birth pair. As standard in the baseline specification, the model includes time fixed effects (year/month), municipality fixed effects and regional trends. The confidence interval bans corresponds to 95% threshold.

Figure B4: Month-by-month exposure to Nazi raids - blue collar status at the age of 30.



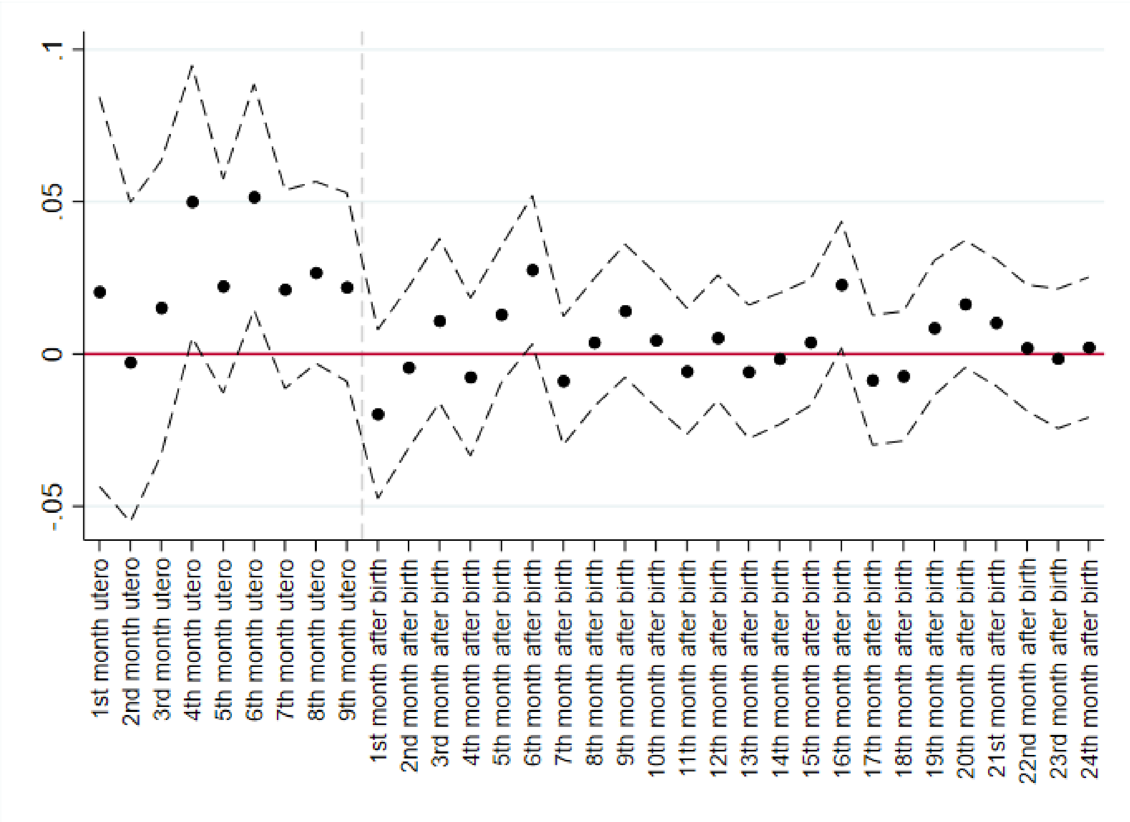
Notes: The figure shows coefficient estimates of the baseline model in the [-24,9] window around the Armistice, where the horizontal axis depicts the timing of each Nazi raid episode with respect to the affected individuals, and is constructed by taking lags and leads of exposure for each cohort/municipality of birth pair. As standard in the baseline specification, the model includes time fixed effects (year/month), municipality fixed effects and regional trends. The confidence interval bans corresponds to 95% threshold.

Figure B5: Month-by-month exposure to Nazi raids - blue collar status at the age of 50.



Notes: The figure shows coefficient estimates of the baseline model in the [-24,9] window around the Armistice, where the horizontal axis depicts the timing of each Nazi raid episode with respect to the affected individuals, and is constructed by taking lags and leads of exposure for each cohort/municipality of birth pair. As standard in the baseline specification, the model includes time fixed effects (year/month), municipality fixed effects and regional trends. The confidence interval bans corresponds to 95% threshold.

Figure B6: Month-by-month exposure to Nazi raids - blue collar status at the age of 60.



Notes: The figure shows coefficient estimates of the baseline model in the [-24,9] window around the Armistice, where the horizontal axis depicts the timing of each Nazi raid episode with respect to the affected individuals, and is constructed by taking lags and leads of exposure for each cohort/municipality of birth pair. As standard in the baseline specification, the model includes time fixed effects (year/month), municipality fixed effects and regional trends. The confidence interval bans corresponds to 95% threshold.

B.4 Alternative specification

B.4.1 Treatment timing variation only

Table B6: Age specific earnings, treated in utero versus treated after birth (no untreated municipalities).

	Earnings at 30	Earnings at 35	Earnings at 40	Earnings at 45	Earnings at 50	Earnings at 55	Earnings at 60
Nazi raid	-0.0210**	-0.0214**	-0.0079	-0.0196**	-0.0189*	-0.0149	-0.0202
in utero	(0.0094)	(0.0089)	(0.0090)	(0.0092)	(0.0100)	(0.0156)	(0.0280)
WWII casualties (SD)	-0.0114*	-0.0171***	-0.0147***	-0.0126**	-0.0137**	-0.0165*	-0.0138
in utero	(0.0059)	(0.0059)	(0.0053)	(0.0061)	(0.0069)	(0.0095)	(0.0132)
R^2	0.1268	0.1116	0.1066	0.1063	0.1042	0.1155	0.1518
N	93,778	91,319	82,300	75,373	70,457	43,375	18,945
Time FEs	YES	YES	YES	YES	YES	YES	YES
Municipality FEs	YES	YES	YES	YES	YES	YES	YES
Reg trends	YES	YES	YES	YES	YES	YES	YES

$^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

Notes: The samples refers to individuals born in the 9 month window around the Armistice (Jan 1943 – May 1944) only in the municipalities with fewer then 200,000 residents that receive a Nazi raid, and refer to age specific outcomes between the ages of 30 and 60. All regressions include month × year fixed effects, municipality fixed effects as well as region specific time trends. Robust standard errors (in parentheses) are clustered at the municipality level.

Table B7: Age specific blue collar status, treated in utero versus treated after birth (no untreated municipalities).

	Blue collar at 30	Blue collar at 35	Blue collar at 40	Blue collar at 45	Blue collar at 50	Blue collar at 55	Blue collar at 60
Nazi raid	0.0206***	0.0003	0.0173**	0.0200***	0.0193**	0.0303***	0.0474***
in utero	(0.0070)	(0.0077)	(0.0079)	(0.0075)	(0.0079)	(0.0097)	(0.0159)
WWII casualties (SD)	0.0048	0.0100***	0.0125***	0.0091*	0.0118**	0.0179***	0.0135
in utero	(0.0039)	(0.0037)	(0.0046)	(0.0049)	(0.0050)	(0.0066)	(0.0083)
R^2	0.0798	0.0886	0.0946	0.0994	0.1020	0.1484	0.1825
N	93,778	91,319	82,300	75,373	70,457	43,375	18,945
Time FEs	YES	YES	YES	YES	YES	YES	YES
Municipality FEs	YES	YES	YES	YES	YES	YES	YES
Reg trends	YES	YES	YES	YES	YES	YES	YES

$^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$
Notes: The samples refers to individuals born in the 9 month window around the Armistice (Jan 1943 – May 1944) only in the municipalities with fewer then 200,000 residents that receive a Nazi raid, and refer to age specific outcomes between the ages of 30 and 60. All regressions include month × year fixed effects, municipality fixed effects as well as region specific time trends. Robust standard errors (in parentheses) are clustered at the municipality level.

Table B8: Effect of layoff on wages, treated in utero versus treated after birth (no untreated municipalities).

	age 45	age 50	age 55	age 60
Nazi raid	-0.0239***	-0.0187*	-0.0110	-0.0197
in utero (β_1^a)	(0.0092)	(0.0096)	(0.0164)	(0.0277)
Layoff (β_2^a)	-0.2196***	-0.2856***	-0.2949***	-0.3135***
	(0.0221)	(0.0207)	(0.0308)	(0.0485)
Layoff × Nazi raid	-0.1946***	-0.1367***	-0.1815***	-0.0432
in utero (β_3^a)	(0.0497)	(0.0505)	(0.0674)	(0.0823)
WWII casualties (SD)	-0.0104**	-0.0099*	-0.0129	-0.0122
in utero (β_4^a)	(0.0047)	(0.0052)	(0.0089)	(0.0117)
R^2	0.1133	0.1121	0.1386	0.1723
N	69,346	65,211	36,922	15,932
Time FEs	YES	YES	YES	YES
Municipality FEs	YES	YES	YES	YES
Reg trends	YES	YES	YES	YES

$^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$
Notes: The samples refers to individuals born in the 9 month window around the Armistice (Jan 1943 – May 1944) only in the municipalities with fewer then 200,000 residents that receive a Nazi raid, and refer to age specific outcomes between the ages of 45 and 60. All regressions include month × year fixed effects, municipality fixed effects as well as region specific time trends. Robust standard errors (in parentheses) are clustered at the municipality level.

B.4.2 Region specific month times year dummy variables

Table B9: Age specific earnings - region/time fixed effects.

	Earnings at 30	Earnings at 35	Earnings at 40	Earnings at 45	Earnings at 50	Earnings at 55	Earnings at 60
Nazi raid	-0.0242***	-0.0235***	-0.0155**	-0.0185**	-0.0273***	-0.0227*	-0.0537**
in utero	(0.0082)	(0.0076)	(0.0078)	(0.0079)	(0.0086)	(0.0123)	(0.0200)
WWII casualties (SD)	-0.0080**	-0.0071*	-0.0059	-0.0058	-0.0054	-0.0110	-0.0061
in utero	(0.0037)	(0.0036)	(0.0055)	(0.0039)	(0.0041)	(0.0066)	(0.0086)
R^2	0.1533	0.1449	0.1409	0.1447	0.1365	0.1446	0.1760
N	211,641	207,420	187,049	170,775	158,164	101,081	47,560
Time FEs	YES	YES	YES	YES	YES	YES	YES
Municipality FEs	YES	YES	YES	YES	YES	YES	YES
Reg Time FEs	YES	YES	YES	YES	YES	YES	YES

$^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

Notes: The samples refers to individuals born in the 9 month window around the Armistice (Jan 1943 – May 1944) in municipalities with fewer then 200,000 residents and refer to age specific outcomes between the ages of 30 and 60. All regressions include month × year fixed effects, municipality fixed effects as well as region × month fixed effects. Robust standard errors (in parentheses) are clustered at the municipality level.

Table B10: Age specific blue collar status - region/time fixed effects.

	Blue collar at 30	Blue collar at 35	Blue collar at 40	Blue collar at 45	Blue collar at 50	Blue collar at 55	Blue collar at 60
Nazi raid	0.0207***	0.0082	0.0198***	0.0177***	0.0198***	0.0250***	0.0351***
in utero	(0.0050)	(0.0061)	(0.0062)	(0.0063)	(0.0066)	(0.0077)	(0.0119)
WWII casualties (SD)	0.0015	0.0022	0.0040**	0.0028	0.0025	0.0027	-0.0011
in utero	(0.0018)	(0.0018)	(0.0021)	(0.0022)	(0.0023)	(0.0028)	(0.0038)
R^2	0.0874	0.0957	0.1015	0.1086	0.1149	0.1640	0.1985
N	211,641	207,420	187,049	170,775	158,164	101,081	47,560
Time FEs	YES	YES	YES	YES	YES	YES	YES
Municipality FEs	YES	YES	YES	YES	YES	YES	YES
Reg Time FEs	YES	YES	YES	YES	YES	YES	YES

$^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

Notes: The samples refers to individuals born in the 9 month window around the Armistice (Jan 1943 – May 1944) in municipalities with fewer then 200,000 residents and refer to age specific outcomes between the ages of 30 and 60. All regressions include month × year fixed effects, municipality fixed effects as well as region × month fixed effects. Robust standard errors (in parentheses) are clustered at the municipality level.

Table B11: Effect of layoff on wages - region/time fixed effects.

	age 45	age 50	age 55	age 60
Nazi raid	-0.0243***	-0.0254***	-0.0230*	-0.0462**
in utero (β_1^a)	(0.0081)	(0.0084)	(0.0126)	(0.0204)
Layoff (β_2^a)	-0.3362***	-0.3236***	-0.3354***	-0.3169***
	(0.0139)	(0.0120)	(0.0163)	(0.0233)
Layoff × Nazi raid	-0.0781	-0.1000**	-0.1421**	-0.0493
in utero (β_3^a)	(0.0476)	(0.0488)	(0.0625)	(0.0708)
WWII casualties (SD)	-0.0044	-0.0056	-0.0079	0.0030
in utero (β_4^a)	(0.0034)	(0.0034)	(0.0054)	(0.0076)
R^2	0.1496	0.1442	0.1698	0.2051
N	155,587	145,885	85,302	39,325
Time FEs	YES	YES	YES	YES
Municipality FEs	YES	YES	YES	YES
Reg Time FEs	YES	YES	YES	YES

$^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$

Notes: The samples refers to individuals born in the 9 month window around the Armistice (Jan 1943 – May 1944) in municipalities with fewer then 200,000 residents and refer to age specific outcomes between the ages of 45 and 60. All regressions include month × year fixed effects, municipality fixed effects as well as region × month fixed effects. Robust standard errors (in parentheses) are clustered at the municipality level.

B.4.3 Shorter gestation

Table B12: Age specific earnings and shorter gestation period.

	Earnings at 30	Earnings at 35	Earnings at 40	Earnings at 45	Earnings at 50	Earnings at 55	Earnings at 60
9 month gestation period [-9,9] window							
Nazi raid	-0.0218***	-0.0238***	-0.0155**	-0.0177**	-0.0268***	-0.0254**	-0.0551***
in utero	(0.0080)	(0.0075)	(0.0076)	(0.0077)	(0.0085)	(0.0120)	(0.0194)
WWII casualties (SD)	-0.0065**	-0.0073**	-0.0046	-0.0053	-0.0052	-0.0115**	-0.0118*
in utero	(0.0032)	(0.0031)	(0.0030)	(0.0034)	(0.0036)	(0.0048)	(0.0064)
R^2	0.1514	0.1437	0.1391	0.1432	0.1348	0.1418	0.1712
N	211,641	207,420	187,049	170,775	158,164	101,081	47,560
8 month gestation period [-9,8] window							
Nazi raid	-0.0241***	-0.0277***	-0.0142*	-0.0198**	-0.0292***	-0.0249**	-0.0526***
in utero	(0.0085)	(0.0079)	(0.0081)	(0.0081)	(0.0092)	(0.0123)	(0.0200)
WWII casualties (SD)	-0.0050	-0.0055	-0.0034	-0.0039	-0.0049	-0.0124**	-0.0055
in utero	(0.0035)	(0.0035)	(0.0034)	(0.0037)	(0.0040)	(0.0052)	(0.0072)
R^2	0.1530	0.1453	0.1411	0.1456	0.1371	0.1451	0.1742
N	201,078	197,052	177,859	162,270	150,403	95,783	45,091
7 month gestation period [-9,7] window							
Nazi raid	-0.0216**	-0.0269***	-0.0154*	-0.0185**	-0.0265***	-0.0313**	-0.0636***
in utero	(0.0093)	(0.0087)	(0.0088)	(0.0088)	(0.0099)	(0.0132)	(0.0220)
WWII casualties (SD)	-0.0065*	-0.0068*	-0.0033	-0.0054	-0.0067	-0.0128**	-0.0079
in utero	(0.0037)	(0.0037)	(0.0037)	(0.0039)	(0.0043)	(0.0057)	(0.0080)
R^2	0.1545	0.1469	0.1430	0.1476	0.1393	0.1485	0.1788
N	189,423	185,605	167,716	152,960	141,929	89,947	42,377
Time FEs	YES	YES	YES	YES	YES	YES	YES
Municipality FEs	YES	YES	YES	YES	YES	YES	YES
Reg trends	YES	YES	YES	YES	YES	YES	YES

$^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$

Notes: The samples refers to individuals born 9 months prior and 9, 8 or 7 months post Armistice (Jan 1943 – May 1944) in municipalities with fewer then 200,000 residents and refer to age specific outcomes between the ages of 30 and 60. All regressions include month × year fixed effects, municipality fixed effects as well as region specific time trends. Robust standard errors (in parentheses) are clustered at the municipality level.

Table B13: Age specific blue collar status and shorter gestation period.

	Blue collar at 30	Blue collar at 35	Blue collar at 40	Blue collar at 45	Blue collar at 50	Blue collar at 55	Blue collar at 60
9 month gestation period [-9,9] window							
Nazi raid	0.0222***	0.0028	0.0176***	0.0172***	0.0184***	0.0277***	0.0370***
in utero	(0.0055)	(0.0067)	(0.0063)	(0.0062)	(0.0065)	(0.0072)	(0.0109)
WWII casualties (SD)	0.0030	0.0039**	0.0062***	0.0048**	0.0050**	0.0064**	0.0027
in utero	(0.0019)	(0.0019)	(0.0022)	(0.0023)	(0.0024)	(0.0030)	(0.0039)
R^2	0.0885	0.0965	0.1036	0.1107	0.1160	0.1658	0.1976
N	211,714	207,515	187,135	170,830	158,232	101,124	47,582
8 month gestation period [-9,8] window							
Nazi raid	0.0224***	0.0033	0.0167**	0.0148**	0.0172**	0.0272***	0.0342***
in utero	(0.0056)	(0.0067)	(0.0065)	(0.0063)	(0.0068)	(0.0076)	(0.0111)
WWII casualties (SD)	0.0035	0.0044**	0.0065***	0.0051*	0.0054**	0.0070**	0.0013
in utero	(0.0022)	(0.0022)	(0.0025)	(0.0026)	(0.0028)	(0.0034)	(0.0044)
R^2	0.0906	0.0972	0.1058	0.1128	0.1179	0.1690	0.2016
N	201,078	197,052	177,859	162,270	150,403	95,783	45,091
7 month gestation period [-9,7] window							
Nazi raid	0.0241***	0.0028	0.0174**	0.0146**	0.0166**	0.0290***	0.0382***
in utero	(0.0061)	(0.0073)	(0.0070)	(0.0068)	(0.0072)	(0.0080)	(0.0115)
WWII casualties (SD)	0.0037	0.0046**	0.0076***	0.0062**	0.0059**	0.0074**	-0.0007
in utero	(0.0023)	(0.0023)	(0.0026)	(0.0027)	(0.0030)	(0.0037)	(0.0046)
R^2	0.0931	0.0981	0.1076	0.1148	0.1205	0.1718	0.2059
N	189,423	185,605	167,716	152,960	141,929	89,947	42,377
Time FEs	YES	YES	YES	YES	YES	YES	YES
Municipality FEs	YES	YES	YES	YES	YES	YES	YES
Reg trends	YES	YES	YES	YES	YES	YES	YES

$^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

Notes: The samples refers to individuals born 9 months prior and 9, 8 or 7 months post Armistice (Jan 1943 – May 1944) in municipalities with fewer then 200,000 residents and refer to age specific outcomes between the ages of 30 and 60. All regressions include month × year fixed effects, municipality fixed effects as well as region specific time trends. Robust standard errors (in parentheses) are clustered at the municipality level.

Table B14: Effect of layoff on wages and shorter gestation period.

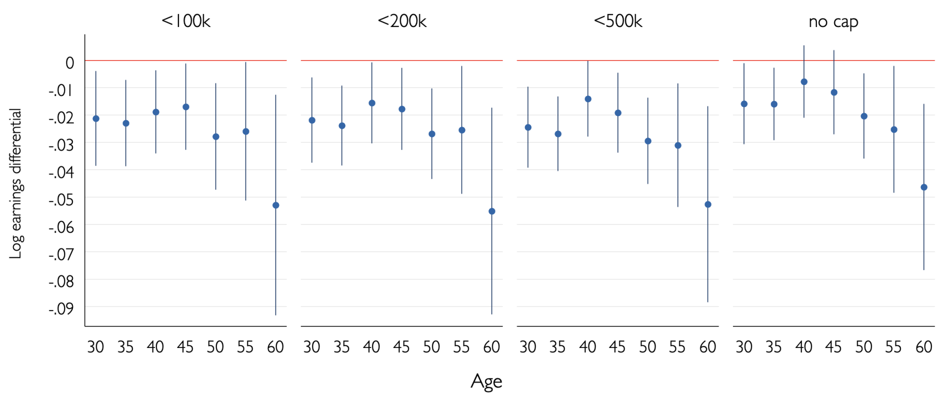
	age 45	age 50	age 55	age 60
9 month gestation period [-9,9] window				
Nazi raid	-0.0244***	-0.0247***	-0.0233*	-0.0519***
in utero	(0.0079)	(0.0083)	(0.0123)	(0.0197)
Layoff	-0.3359***	-0.3234***	-0.3350***	-0.3157***
	(0.0139)	(0.0120)	(0.0163)	(0.0234)
Layoff × Nazi raid	-0.0781*	-0.0996**	-0.1436**	-0.0445
in utero	(0.0474)	(0.0490)	(0.0625)	(0.0709)
WWII casualties (SD)	-0.0063*	-0.0063*	-0.0097*	-0.0058
in utero	(0.0034)	(0.0033)	(0.0050)	(0.0070)
R^2	0.1500	0.1427	0.1682	0.2006
N	155,587	145,885	85,302	39,325
8 month gestation period [-9,8] window				
Nazi raid	-0.0261***	-0.0271***	-0.0264**	-0.0503**
in utero	(0.0084)	(0.0087)	(0.0126)	(0.0204)
Layoff	-0.3359***	-0.3238***	-0.3385***	-0.2980***
	(0.0141)	(0.0122)	(0.0167)	(0.0238)
Layoff × Nazi raid	-0.1011*	-0.0971*	-0.1490**	-0.0547
in utero	(0.0529)	(0.0531)	(0.0679)	(0.0841)
WWII casualties (SD)	-0.0036	-0.0046	-0.0084*	-0.0012
in utero	(0.0030)	(0.0030)	(0.0044)	(0.0064)
R^2	0.1504	0.1445	0.1708	0.2005
N	147,806	138,618	80,851	37,243
7 month gestation period [-9,7] window				
Nazi raid	-0.0234***	-0.0241**	-0.0320**	-0.0653***
in utero	(0.0091)	(0.0095)	(0.0140)	(0.0220)
Layoff	-0.3398***	-0.3196***	-0.3399***	-0.2978***
	(0.0144)	(0.0124)	(0.0171)	(0.0244)
Layoff × Nazi raid	-0.0995*	-0.1109*	-0.1286*	-0.0551
in utero	(0.0581)	(0.0577)	(0.0731)	(0.0874)
WWII casualties (SD)	-0.0049	-0.0069**	-0.0099**	-0.0006
in utero	(0.0031)	(0.0033)	(0.0048)	(0.0074)
R^2	0.1524	0.1465	0.1739	0.2041
N	139,311	130,812	76,002	34,939
Time FEs	YES	YES	YES	YES
Municipality FEs	YES	YES	YES	YES
Reg trends	YES	YES	YES	YES

$^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

Notes: The samples refers to individuals born 9 months prior and 9, 8 or 7 months post Armistice (Jan 1943 – May 1944) in municipalities with fewer then 200,000 residents and refer to age specific outcomes between the ages of 45 and 60. All regressions include month × year fixed effects, municipality fixed effects as well as region specific time trends. Robust standard errors (in parentheses) are clustered at the municipality level.

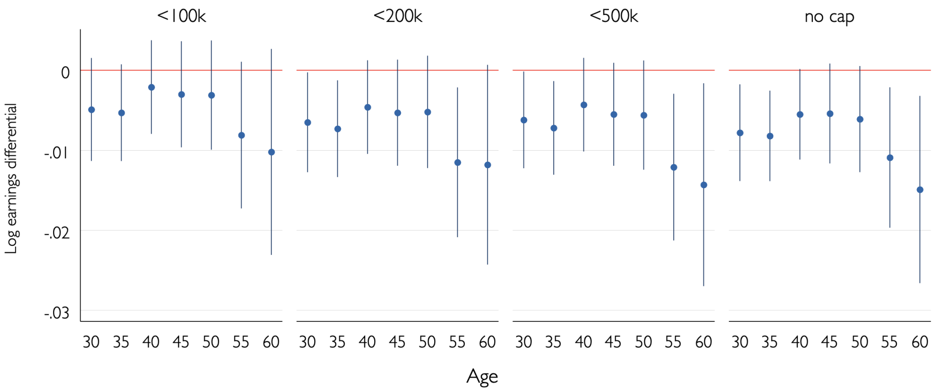
B.5 Sensitivity to municipality size

Figure B7: Sensitivity to municipality size - the effect of Nazi raids on age specific log earnings



Notes: Nazi exposure in utero among individuals born in the [-9,9] month window around the Armistice in subsamples which include municipalities with progressively increasing resident population size (under 100,000, under 200,000, under 500,000, no cap).

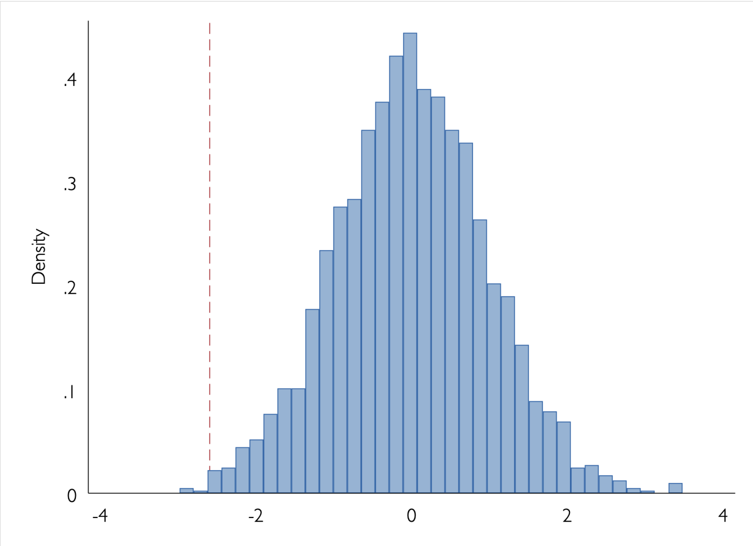
Figure B8: Sensitivity to municipality size - the general WWII effects (WWII casualties) on age specific log earnings



Notes: WWII exposure in utero among individuals born in the [-9,9] month window around the Armistice in subsamples which include municipalities with progressively increasing resident population size (under 100,000, under 200,000, under 500,000, no cap).

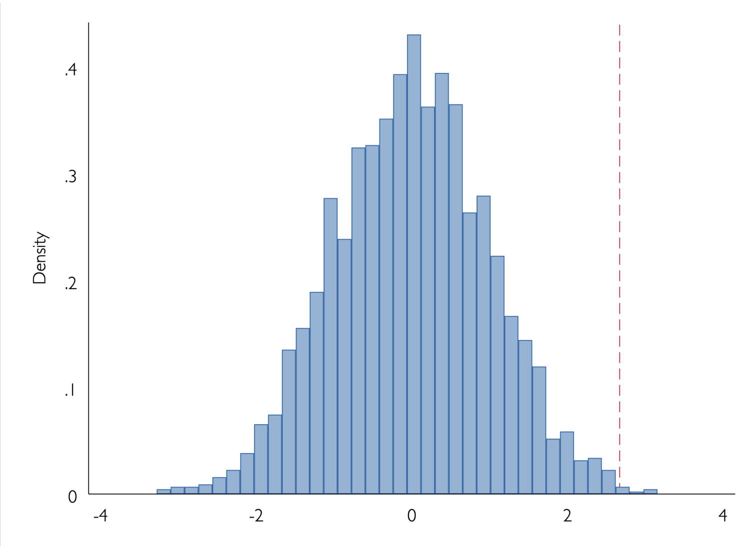
B.6 Falsification tests

Figure B9: Placebo assignment of Nazi raids - earnings at age 30



Notes: Pseudo-treatment vs. actual Nazi raids: the distribution of t-statistics resulting from 5,000 random assignments of treatment to individuals, as well as the t-statistics from the actual treatment (red dotted line).

Figure B10: Placebo assignment of Nazi raids - blue collar status at age 30



Notes: Pseudo-treatment vs. actual Nazi raids: the distribution of t-statistics resulting from 5,000 random assignments of treatment to individuals, as well as the t-statistics from the actual treatment (red dotted line).

B.7 Selectivity robustness checks

Table B15: Effect of WWII intensity and Nazi raids on mortality rate from pregnancy complications.

	Mortality rate from pregnancy complications
No. Nazi massacres	-.00092
in utero	(-0.21)
WWII casualties (SD)	.0247***
in utero	(2.74)
R^2	0.45
N	132

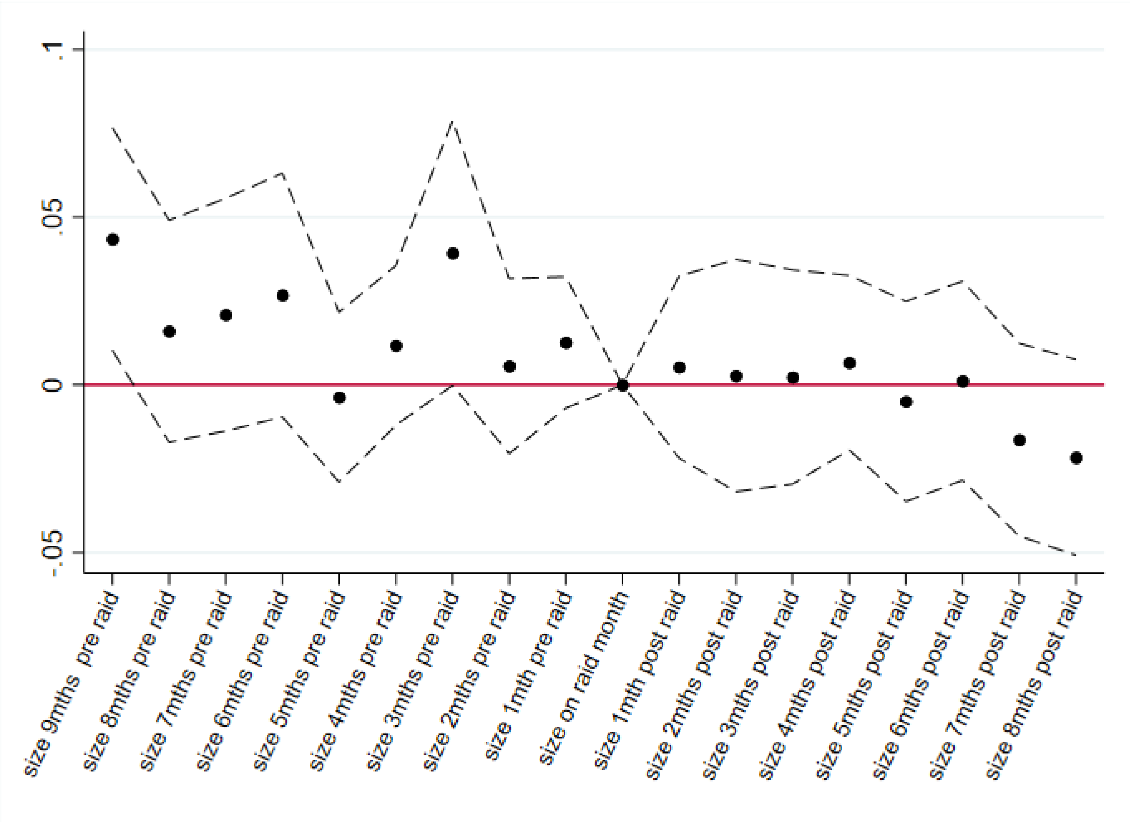
$^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$
Notes: The sample of 132 refers to 21 regions in 6 years (1941-1946). All regressions include year and region fixed effects as well as robust standard errors (t statistics in parentheses).

Table B16: Effect of Nazi raids on municipality level cohort size.

	cohort size 30-year-olds	cohort size 40-year-olds	cohort size 60-year-olds	cohort size 70-year-olds
Nazi Raid	0.0053	0.0055	0.0027	-0.0041
	(0.0138)	(0.0138)	(0.0130)	(0.0119)
WWII casualties	0.0011**	0.0011	0.009*	0.0007
(SD)	(0.0005)	(0.0008)	(0.0006)	(0.0005)
R^2	0.2979	0.2980	0.2880	0.2707
N	135,150	135,150	135,150	135,150
Time FEs	YES	YES	YES	YES
Municipality FEs	YES	YES	YES	YES
Reg trends	YES	YES	YES	YES

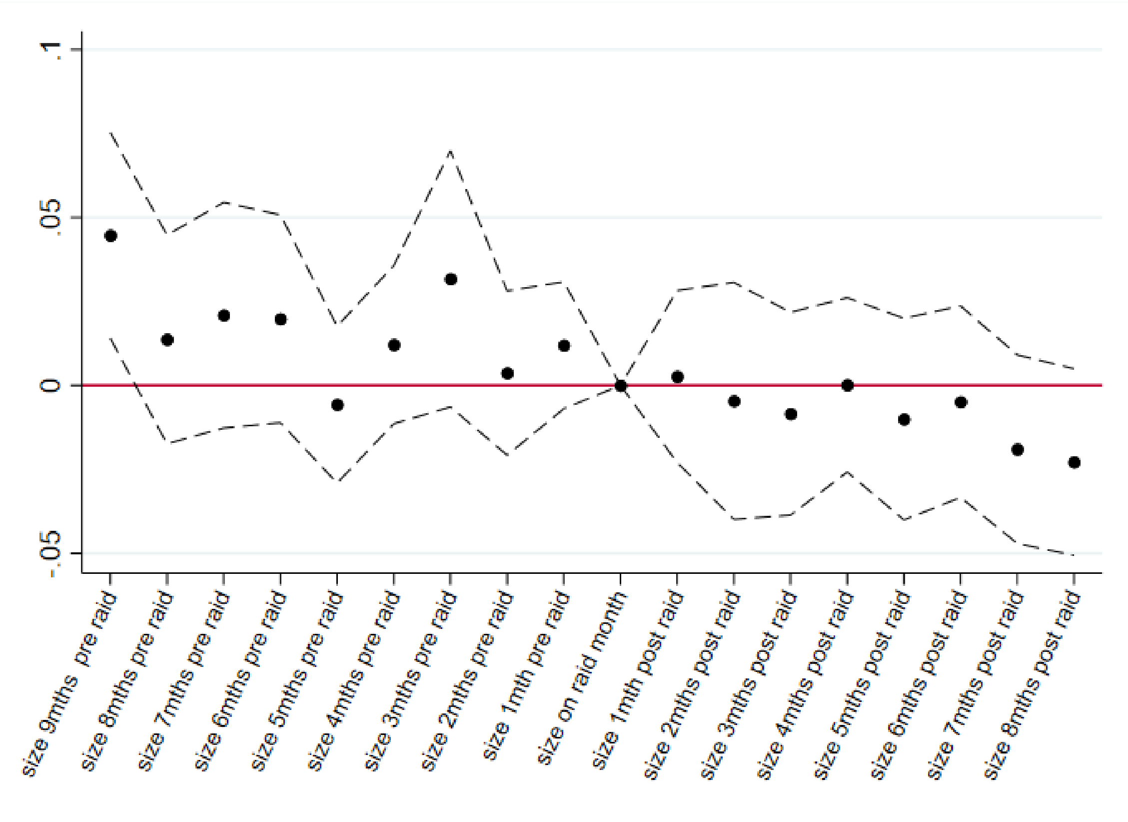
$^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$
Notes: The sample of 135,150 observations refers to a balanced panel of monthly percentage changes in municipality cohort sizes of individuals born between Jan 1943 and May 1944 with respect to current population size. Monthly level cohort size is calculated based on INPS beneficiaries as observed at the age of 30, 40, 60 and 70. All regressions include month × year and municipality fixed effects as well as region specific time trends.

Figure B11: Monthly cohort sizes observed at the age of 30 in treated municipalities.



Notes: The figure shows coefficient estimates on monthly percentage changes in municipality cohort sizes of individuals born between Jan 1943 and May 1944 with respect to current population in treated municipalities only. Monthly level cohort size is calculated based on INPS beneficiaries as observed at the age of 30. We use lags and leads of exposure for each cohort/municipality of birth pair. As standard in the baseline specification, the model includes time fixed effects (year/month), municipality fixed effects and regional trends. The confidence interval bans correspond to 95% threshold.

Figure B12: Monthly cohort sizes observed at the age of 60 in treated municipalities.



Notes: The figure shows coefficient estimates on monthly percentage changes in municipality cohort sizes of individuals born between Jan 1943 and May 1944 with respect to current population in treated municipalities only. Monthly level cohort size is calculated based on INPS beneficiaries as observed at the age of 60. We use lags and leads of exposure for each cohort/municipality of birth pair. As standard in the baseline specification, the model includes time fixed effects (year/month), municipality fixed effects and regional trends. The confidence interval bans correspond to 95% threshold.

C Appendix: Examining the random assignment of the Nazi raids

The assumption of random assignment of the Nazi raids across municipalities is crucial for the identification of causal effects. This appendix aims to support evidence in favor of this assumption. We start with graphs of the (evolution of) the Nazi raids in the period September 1943 – May 1944.

In the spring of 1943 the battlefront was in the north Africa and progressed in the first month following September 8th, 1943 via Sicily to Napels in October 1943 (see Figure 3). From thereon the Allied forces made little progress up to June 1943 when they broke through the defense lines of the German forces near Rome. This is also reflected in Figure C1 that depicts the monthly distribution of the Nazi raids over the country for the period September - May 1944. Figure C2 aggregates these raids over all months. Figure C1 shows that the raids were not isolated to areas in the vicinity of the battlefront, but rather covered all of Italy, north of the battlefront.

The empirical model in section 5 includes municipality fixed effects. In this way we control for time invariant differences between treated and control municipalities. This, however, still leaves some room for structural differences between municipalities that vary over time. In order to address this we estimated a model that relates whether a municipality experienced a Nazi raid in the period September 1943– May 1944 to a range of municipality characteristics obtained form the 2011 census [40] and province and time fixed effects. These characteristics include the (logarithm of) population size, population density, socio-demographic characteristics and geographical information.

The results of this regression are reported in the first column of Table C1. The table shows that apart from population size there are no associations with other characteristics. Our regressions include municipality fixed effects and therefore control for time invariant effects from the population size. We therefore examine whether the impact of the regressors change over time. To this end we also estimated the model for each month over the period September 1943 – May 1944 and tested for time invariant effects of the regressors using an F-test. [41]. The p-values of this F-test are reported in the last column of Table C1 and indicate that the null of invariant structural differences between treated and control municipalities is not rejected.

[40]There is no information for the WWII years

[41]This boils down to using the longitudinal (monthly) information and doing a full interaction of all the characteristics with month dummies

Figure C1: Evolution of Nazi raids (Sept 1943 - May 1944)

Sep 1943

Oct 1943

Nov 1943

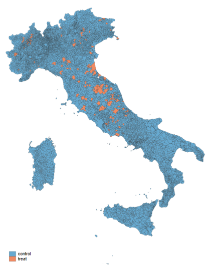
Dec 1943

Jan 1944

Feb 1944

Mar 1944

Apr 1944



May 1944

Figure C2: Placement of Nazi raids

Notes: The map shows all Italian municipalities, evidencing in dark the treated municipalities, municipalities which registered an occurrence of a Nazi raid in the period between Sept 1943 and May 1944.

Table C1: The assignment of Nazi raids to municipalities.

	Nazi raid event (1943.09-1944.05)	
	Coefficient	Joint sign. (p-val)
Log population size	0.026***	0.1434
	(0.004)	
Population density	0.006	0.2930
	(0.008)	
Mortality 65+	0.001	0.2394
	(0.001)	
Low education	-0.006	0.2801
	(0.003)	
Log per-capita income	-0.006	0.3928
	(0.015)	
Sea access municipality	-0.010	0.6498
	(0.008)	
Snowfall	-0.000	0.2823
	(0.000)	
Rainfall	-0.000	0.1558
	(0.003)	
Maximum temperature	0.001	0.4966
	(0.001)	
Minimum temperature	-0.005***	0.6234
	(0.002)	
N	8,091	
R^2	0.11	

$^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

Notes: $N = 8091$ refers to all Italian municipalities. The characteristics are relative to year 2011, corresponding to the census year which collected also municipality level education attainment. Nazi event is an indicator dummy for municipality with a Nazi raid in the period September 1943 – May 1944. The second column reports p-values of joint significance tests of the interaction terms of the municipality level characteristics and year $\times$ month fixed effects. All regressions control for province fixed effects.