Traumatic experiences adversely affect life cycle labor market outcomes of the next generation - Evidence from WWII Nazi raids. *

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Abstract

This paper examines the causal effect of a traumatic and stressful event experienced by pregnant women on life long labor market outcomes of their offspring. We exploit a unique natural experiment that involved randomly placed Nazi raids in municipalities in Italy during WWII. We link administrative data of male private sector workers to information about war casualties and Nazi raids. Our results suggest that prenatal exposure to violence affects offspring earnings along the whole working career and in retirement. The lower earnings are due to lower educational attainment, the type of jobs that they hold and interruptions in the working careers due to unemployment. We find that prenatal exposure exacerbates the negative effects of later life job loss on earnings, deepening the negative impact on earnings at later ages. We use a medical database on health expenditures to interpret the effect estimates. The prenatally exposed have higher medical care expenditures on diseases of the nervous system and mental disorders. This indicates that stress induced by the violent raids is likely to be an important factor driving our findings.

JEL Codes: J24, I15
Keywords: WWII; Violent raids; Prenatal exposure; Offspring; Life-cycle earnings; Mass layoff; Stress.

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1 Introduction

A mother's exposure to traumatic experiences during pregnancy may compromise the health and human capital of her children (Almond and Currie, 2012; Black et al., 2016; Kuzawa and Quinn, 2009; Persson and Rossin-Slater, 2018). Until now, not much is known about the effect of traumatic and stressful experiences on life cycle labor outcomes of the next generation. With this paper we aim to fill this gap. Specifically, this paper studies: *i)* how a traumatic event experienced by a pregnant mother shapes the evolution of the working career of her offspring from the start of the career to retirement; and *ii)* how the prenatally exposed respond to later later life job loss.

To this end we exploit Nazi raids against the Italian population perpetrated after the Italian "Armistice" in WWII on September 8^{th}, 1943. The raids were intentionally unpredictable and idiosyncratically distributed, with the intention to spread terror, disrupt social life and ultimately to discourage Italian resistance. Although there were relatively few Italian casualties and the raids only lasted for a few days, these raids were characterized by intense violence that may have caused great stress and trauma by those witnessing it.

This unique quasi-experimental setting allows us to analyze differences in outcomes for cohorts born in municipalities before and right after a Nazi raid, relative to the same cohorts which were never exposed. We exploit the unexpected outbreak of raids and the spatial (municipalities) and time (months) variation after controlling for potentially confounding factors (i.e. armed conflicts between German and Allied forces, local time invariant and time varying characteristics and the evolution of WWII at the local level). We thus identify the average causal effect of the prenatal exposure to a traumatic event, over and above other adversities related to war.

To identify life cycle labour market effects, we build a unique dataset combing three main data sources. The "Atlas of Nazi and Fascist massacres" lists all raids occurred in Italy after September 8, 1943. We link this dataset to an administrative individual employer-employee matched dataset from "Istituto Nazionale della Previdenza Sociale" (INPS) that covers the universe of Italian private sector workers (excluding agriculture) between 1974 and 2018. We focus on male cohorts born in 1943-44. The data contain detailed working histories and information on earnings, occupation, employment status, education, pensions, disability and unemployment benefits. The third source of information is

the archive of WWII conflicts in Italy collected by Statistics Italy (ISTAT, 1957) including granular information on the number of victims in WWII by province and month.

Results from a generalized Difference-in-Differences (DiD) model show that in utero exposure to the traumatic event reduces earnings with about 2% earnings at the start of the labor market career, relative to their non-exposed peers. The earnings penalty gradually increases along the entire career to about 6% when they retire. Moreover, we find that educational attainment is compromised for those who are exposed to a Nazi raid while in-utero (hereafter often referred to as the "exposed" or "prenatally exposed") and that the exposed are more likely to sort in lower skilled blue collar jobs at labor market entry and throughout the career. We also find that the exposed are more likely to experience unemployment spells. So, interruptions in the labor market career could have contributed to the large earnings penalty for older workers. It could also be the case that the exposed suffer more from job loss later in life. We therefore exploit information on firm level mass layoffs to examine whether later life job loss dis-proportionally affect the exposed.[1] We find that they do: job loss effects on earnings are between 31-34% for all workers and up to 47% for the exposed. Moreover, and in line with the existing literature (e.g. Sullivan and von Wachter, 2009), we find that mass layoff episodes are likely to increase mortality among workers, but this effect is independent from in utero exposure to a nazi raid.

These results are confirmed by a series of robustness checks. We use more flexible specifications, vary the definition of our outcome variables, use different analytic samples, use a different identification strategy and perform falsification tests. We additionally show that selective fertility, mobility and mortality do not bias our estimates.

As a further step in our analysis, we argue that stress is likely to be the most important mechanism driving our empirical findings. Several medical studies show that the mother's hormonal response to anxiety and stress increases Cortisol levels and that this may have a profound impact on the neurological development of the fetus. This can therefore lead to cognitive, emotional and mental problems as well as increased stress vulnerability of the offspring (see Boersma and Tamashiro, 2014; Cotter and Pariante, 2002; Marmot et al., 1991; Van den Bergh et al., 2005; Weinstock, 2005,

[1] In section 5.2 we show that the allocation of mass layoffs across the workers in our sample is independent from prenatal exposure to a Nazi raid.

and the literature reviewed in these papers). Also recent economic literature has given support to these claims (Black et al., 2016; Persson and Rossin-Slater, 2018; Quintana-Domeque and Rodenas-Serrano, 2017) by showing causal effects of maternal stress during pregnancy on birth outcomes, educational attainment and mental health of the offspring. Aizer et al. (2016) observe maternal Cortisol levels. They use a sibling fixed effects estimator and find that elevated Cortisol levels adversely affect cognition, education and behavioral and motor development in childhood.[2] In support of our interpretation and in line with the literature cited above we find a lower educational attainment for the exposed. Additionally, we use information from a medical database on health expenditures and find that prenatal exposure to a Nazi raid has significant and sizeable effects on health expenditures for diseases of the nervous system and mental disorders, conditions typically associated with prenatal stress exposure. We find no effect for other diseases types.[3]

We also show that we find no effect of exposure to a Nazi raid in the first and the second year of life. This is important as the raids may trigger other mechanisms, besides stress, that can harm life cycle labor market outcomes. The raids may have led to property destruction, loss of income, family ruptures and (in turn) may affect parental health (behaviors) and parenting skills. Should these alternative mechanisms be relevant, they would harm both the prenatally exposed and those exposed post-birth. Our finding thus suggests that our results are not driven by behavioral and/or income effects post-birth.

Several features distinguish our study from previous work. First, previous studies that looked at stressful events primarily looked at infant and childhood educational and health outcomes (e.g. Bundervoet and Fransen, 2018; Camacho, 2008; Quintana-Domeque and Rodenas-Serrano, 2017). To the best of our knowledge there are only two exceptions that also look at the causal effect of in-utero stress on adult outcomes (Black et al., 2016; Persson and Rossin-Slater, 2018). These studies examine the effect of a death of a relative of pregnant mothers in Sweden and Norway.

Persson and Rossin-Slater (2018) look at mothers who experience a family rupture while being

[2]Note that even with actual observation on stress hormone levels one can never rule out that other mechanisms besides stress play an important role.

[3]These include Cardiovascular, Diseases and Diabetes, diseases of the Respiratory system, Hormone system, Neoplasms, Skin and Musculo Skeletal diseases. The famine literature finds consistent effects of hunger on Cardiovascular Diseases and Diabetes (see Lumey et al. (2011) for a review). We find no effect of prenatal exposure on expenditures for these conditions. We turn to this in Section 7.

pregnant as opposed to mothers who undergo it shortly after giving birth. For infants they find that in-utero exposure to stress increases the likelihood of purchasing ADHD medications and the average daily dose of ADHD medications. For adults (aged 30) they find increases in the likelihood of consuming prescription drugs for anxiety and depression. The latter finding corroborates our findings based on the external medical database. Black et al. (2016) compare children born to the same mother, with the mother experiencing a parental death during one of the pregnancies to examine the effect of death induced stress on birth and labor market outcomes in adulthood. They find that parental death experienced in utero leads to small negative effects on birth outcomes. They find no effect on labor supply and earnings in 2010, when the individuals were between 25 and 43 years old.

Our analyses complement the findings of both studies. We show that the effects of prenatal exposure to a traumatic event experienced by the mother persist over the rest of the offspring's life. Our 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 wage earnings between ages 30 to 60 and income after retirement. We also study the effects of job loss due to a mass layoff at the firm. These analyses indicate that early life traumatic experiences may be an important mediator in explaining longer term negative effects on labor earnings and other social outcomes. In all fairness, our historical setting of raids against civilians in times of war may have less external validity for current developed countries than the studies by Black et al. (2016) and Persson and Rossin-Slater (2018). However, sadly, the shocks that we consider still relate to conflict areas around the world.

Second, our mass layoffs analyses supplement the existing economic literature that 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 Chan and Stevens, 2001; Ruhm, 1991, 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, suicide and traffic accidents and effects on alcohol-related diseases, and mental illness. Recently, Kaila et al. (2022) show that workers with less-well-off parents have lower earnings and higher unemployment

than similar workers with wealthier parents who were laid off. We add to this literature by examining the heterogeneous impact of job loss on mortality and earnings by prenatal exposure to a traumatic event. We show that the earnings penalty of job loss are up to 14 percentage points higher for those who are prenatally exposed to a traumatic event and that this effect increases with age.

Our job displacement analyses also 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 exposure to a traumatic event impairs the offspring's cognitive and/or stress management skills, leaving them more vulnerable to stressful and challenging events later in life.

Third, our setting features a unique natural experimental design with desirable properties supported by excellent administrative data. The raids we consider lasted mostly one day, 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 detailed administrative data allows us to continuously monitor individuals over their entire labor market career, while controlling for several confounding effects due to other war related events. Previous studies in this literature 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 births (Persson and Rossin-Slater, 2018). For the sake of comparison and to examine the robustness of our findings we also estimate models that only exploit randomness in the exact timing of the raid.[4] In all our models the idiosyncratic shocks are assigned at the aggregate level (municipality) and are beyond the individual's control, making them more likely to be exogenous from an individual point.

Section 2 describes specific aspects of WWII in Italy, relevant for the empirical design that we use in our study; Section 3 presents the data; Section 4 discusses the empirical strategy. Section 5 presents our main results. This section also discusses the interpretation of our findings presents

[4]See section 4 for a more detailed discussion.

additional robustness checks and addresses selection issues. Section 8 summarizes and concludes

2 Historical background

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 3rd 1943, but was made public on the radio on September 8th at 18:30 Italian time.[5]

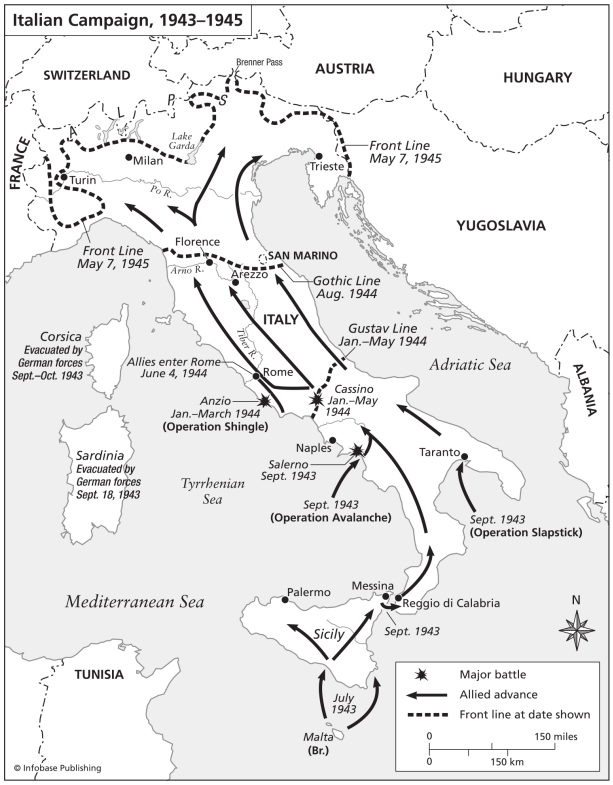
Only a few weeks before September 8th 1943, the Allies landed in Sicily (July 1943) to start their campaign against the German forces (see Figure 1). While the arrival of the Allied troops occurred before the 8th of September, the Italian campaign changed radically after the Armistice. In fact, by September 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 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.[6] The civilian population also had no information about the

[5]The Armistice was secretly signed in Santa Teresa Longarini district of Syracuse, 3 km from Cassibile. 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 9th.

[6]For example, on the 10th of September 1943, in Piombino, a small German flotilla tried to enter the harbour of

evolution of relations with Germany.

Figure 1: WWII fronts in Italy



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, there were no national evacuation plans (Baldoli et al., 2011). 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.

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.

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 and slowly moved North and then got stuck along the Winter line at Monte Cassino (just above Naples) in December 1943. The seven month stalemate that followed caused huge losses among civilians.[7]

The violent Nazi raids 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 generally aimed at the Italian population. German terror acts included exploiting women and children, confiscating economic resources and rounding up civilians and deporting them to labor camps. Although the raids involved relatively few Italian casualties and they lasted only for a few days, these massacres were characterized by intense violence witnessed by the civilian population.

3 Data

We construct an individual-level dataset as a combination of several administrative datasets. 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 data as "War data". Secondly, we use an

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

administrative employer-employee matched dataset on the universe of Italian private sector workers containing detailed working histories, information on earnings, occupation, employment status, education, pensions, disability, unemployment benefits and mortality. We refer to this data as "labor data".

3.1 War data

3.1.1 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 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.[8]

The database lists and analyses all raids occurred in Italy after September 8th, 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 25th 1945) in the regions of Piedmont, Lombardy and Trentino Alto Adige.

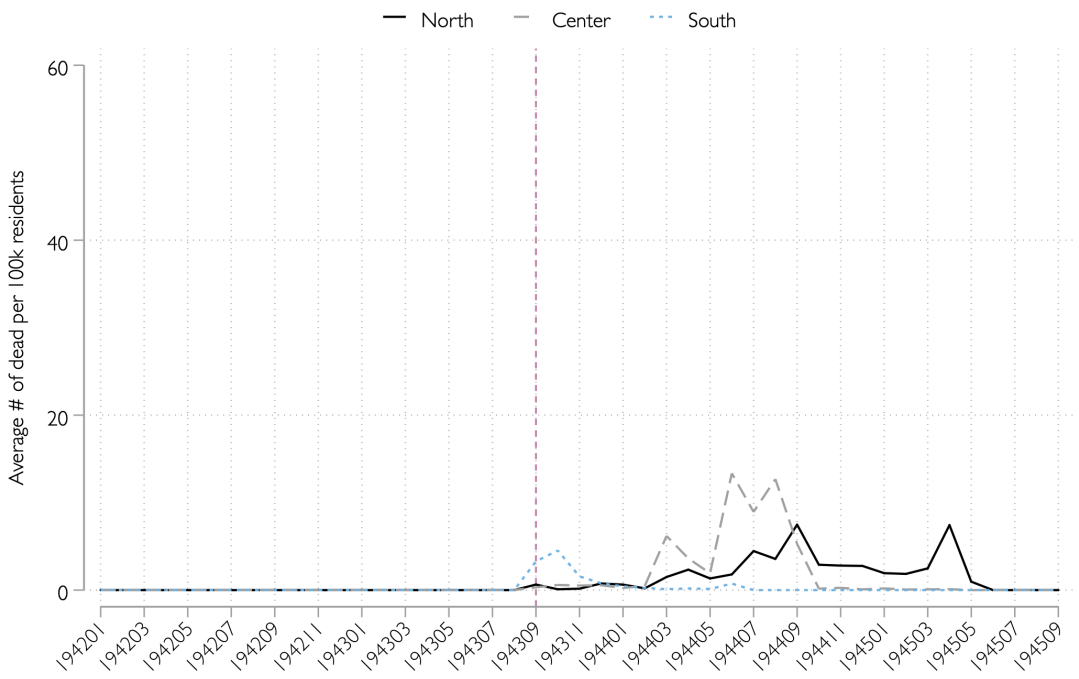
The database provides information on the number and type of victims by age and gender and the precise timing (day) and place (municipality).[9] The database counts more then 5,800 episodes producing 20,000 victims distributed across 2,200 municipalities. For reasons that will become clear in Sections 3.3 and 5, we focus on raids in smaller municipalities (less than 200,000 residents) and only use raids in the first 9 months following the armistice (September 1943–May 1944). This restricts the sample to 1,603 episodes, lasting on average 1.4 days with an average of 3.27 victims per episode (see Table 1).[10] Figure 2 depicts the evolution of casualties due to raids by region, over time. Additionally, Figure A1 of Appendix A shows that in the 9 months after the armistice the raids

[8]This database is also used by Gagliarducci et al. (2020). Information about the project, the database and access possibilities is available at `http://www.straginaziFasciste.it/?page_id=9&lang=en`.

[9]This is the smallest administrative unit.

[10]The summary statistics of the Nazi raids over the entire period are reported in Table B1 of Appendix B.

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



covered all Italian regions north of the battlefront. Table B2 in Appendix B.1 reports the number of raids by month for the first 9 months after the armistice.

HERE GOES TABLE 1

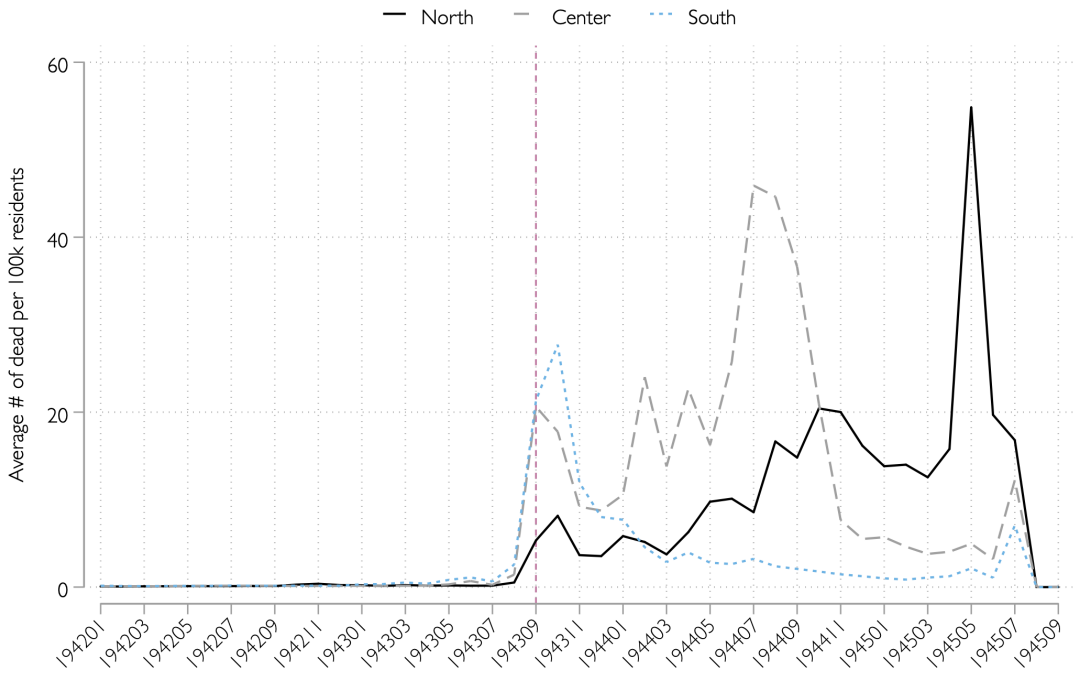
3.1.2 General data on deaths and missing persons during WWII in Italy

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), collected by the Italian National Institute of Statistics (ISTAT, 1957).[11] 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

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

the intensity of battles between the Allied Forces and the Germans.[12]

Figure 3: Average number of WWII victims per 100,000 residents by region



As described in the previous section, the Allied forces gradually progressed up from the South in September 1943 ultimately freeing the Northern territories of Italy in May 1945. Figure 3 shows how the peaks in casualties move over time, starting from the South, moving to the Center and finally to the North.

3.2 The labor data

The longitudinal employer-employee data are provided by the Istituto Nazionale della Previdenza Sociale (INPS), the national social security and welfare institute in Italy, one of the largest administrative bodies at the European level.[13] INPS oversees all private sector employees and employers operating in Italy during the years 1974–2018.

[12]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.

[13]More information about INPS can be obtained from https://www.inps.it/nuovoportaleinps/default.aspx?itemdir=47212

3.2.1 Matched employer-employee data

INPS provides longitudinal employment and earning histories including employee's demographics, occupation, contract type and earnings. The INPS data cover the universe of private sector work contracts (excluding agriculture) in Italy over the period 1974–2018. The dataset tracks labor market mobility of workers over their entire working careers. Being a matched employer-employee database, it also includes information on all private sector employers. Private sector jobs amount to more than 70% of all jobs in Italy. Zero earnings in the INPS data can arise due to due to moving out of the labor market or being employed in a sector different from the private one, this is a common feature of administrative employer-employee data.

Additionally, for all Italian residents INPS collects information on municipality and month of birth, labor market entry and death date. The data also includes detailed information on pensions and social security benefits claims for all workers in the private and public sector. This feature of the data allows us to address the external validity of our estimates based on private sector workers (see below and Section 6.6).

3.2.2 Pension and benefits data

The data on pensions are available from 1995 onward and include information on the age of onset of retirement benefits, the type of pension, the number of years of contributions as well as the amount of the benefit. Additionally, INPS collects social security premiums for the provision of Disability Insurance (DI) and Unemployment Insurance (UI), which allows us to examine interruptions during labor market careers.

The data cover all Italian beneficiaries independently of the sector which allows us to examine whether selection into the private sector is independent from in utero exposure to a Nazi raid. We find no evidence of a relationship between prenatal exposure to a Nazi raid and working in the private sector.[14]

[14] A regression of private sector worker status on in-utero exposure to a Nazi raid, controlling for municipality fixed effects, war casualties, year × month fixed effects and regional time trends results in a non-statistically significant coefficient estimate of -0.0015 (s.e. equals 0.0011). See also Section 6.6.

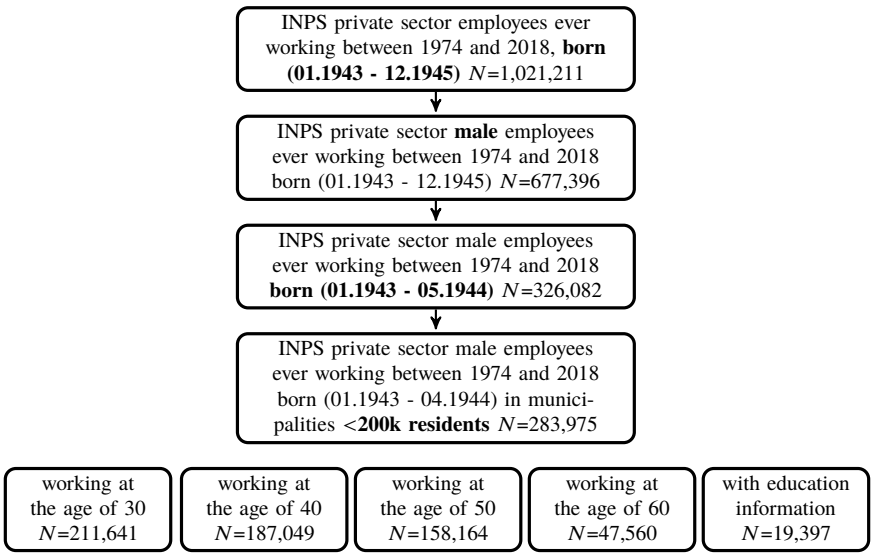
3.2.3 Education data

INPS collects data on educational attainment of private sector workers whose contracts underwent a change after 2010 (activation, renewal, termination, transformation). After 2010, employers were obliged to collect and disclose information on new contracts or a change in the old contracts to the Ministry of Labor and then to INPS. We observe educational attainment for 52% of the sample of workers in our sample. [15] We performed a regression of being present in the education sample on being prenatally exposed to a Nazi raid and find no evidence of a significant association (the coefficient of this regression equals -0.00004 (s.e. 0.0028)).[16]

3.3 Data selection

We link the Nazi raids data to the individual INPS data through the municipality, month and year of birth. For the general WWII data, we link the records at the province, month and year of birth level. We make a number of sample restrictions to obtain the dataset used in the analyses. Figure 4 offers a detailed description of our selection process.

Figure 4: INPS data selection



We first select all employees who worked at any point between 1974 and 2018 and were born

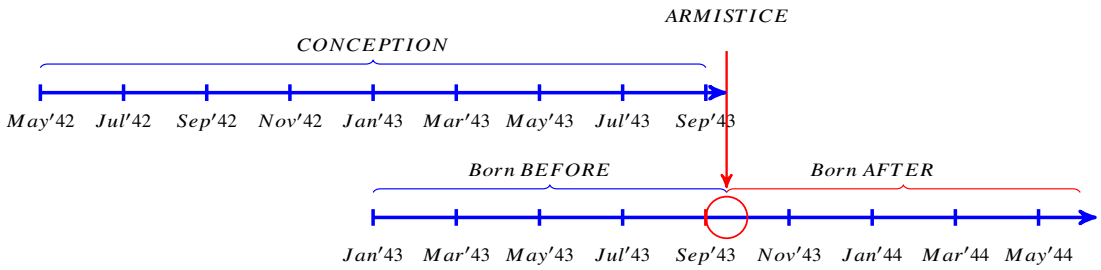
[15] Among the cohorts born around the Armistice, 37,471 individuals are still employed after 2010. We are able to determine the educational attainment for 19,397 individuals. See Section 3.3.

[16] See also Section 5.1.

in the WWII era, here defined as the period between January 1943 and May 1945 (this results in a total of 1,012,211 individuals). We then restrict our sample to males, since female labor force participation was very low (less than 30%) among the cohorts considered.[17] This results in 677,396 individuals. Given that the sudden onset of Nazi raids was triggered by the Armistice announced on September 8, 1943 (see Section 2), we restrict our sample to individuals born in a 9 month window around the date of the Armistice (i.e. born between January 1943 and May 1944). This results in 326,082 individuals.

Figure 5 describes the timeline of the events and the monthly cohort selection. Those born less than 9 months after September 8th were (potentially) exposed to in-utero stress, while others (born in the 9 months preceding September 8th) were not. This short time window rules out selective fertility and limits the potential impact of other behavioral responses that may occur after the Armistice as time proceeds. Yet, it does not rule out mortality selection in utero (still births) and between birth and 1974, the first year when INPS data on workers is collected (we return to these issues in Section 6.6).

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 mass destruction due to bombing and nutritional shortages. These other hardships may confound our estimates and, importantly, may also affect the interpretation of the channels through which the effects of the Nazi

[17]Exposure to stress early in life may also affect fertility behavior, which in itself may effect labor force participation decision of females and make the results difficult to interpret.

raids may have operated. We carefully address this issue in Section 7. We therefore further restrict our sample to individuals born in municipalities with less than 200 thousand residents.[18]

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 to 1,603 Nazi raids while in utero.

3.4 Exposure variables

For each individual in the sample we determine the intra-uterine period by counting 9 months backwards from the exact date of birth. In Section 6.4 we examine robustness of our results to alternative lengths of gestation.

Based on individual intra-uterine period and municipality of birth, we create an indicator for Nazi violence exposure. This indicator equals 1 if the municipality of birth had at least one Nazi violence episode in the 9 months preceding birth. Analogously we construct a variable describing exposure to general war intensity (see Section 3.1). This variable is defined as 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 ease of interpretation, we express this variable as a z-score with mean zero and standard deviation of one.

3.5 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 ages a (for $a =$ 30, 35, 40, 45, 50, 55 and 60). To minimize the variance in earnings, for each a we take the average of the earnings at $(a-1)$, a and $(a+1)$. We also construct indicator variables for less than secondary and less than tertiary education for the sub-sample of workers with information on educational attainment

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For each individual we construct binary indicators for mortality, unemployment benefit claims and disability benefit receipt. We also examine the effect of exposure to Nazi raids while in utero on

[18]We use current population counts, as complete municipality level census data are not available for the WWII period. As already mentioned before, we also use different cut-offs for the municipality size and discuss this in Section 7.1.

[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.

two retirement outcomes: the age at retirement and the pension benefit amount. The pension benefit amount is a function of work experience, age when the benefit is claimed, and average earnings over the last working years.[20]

our analyses also include the income and mortality effects of job loss at later ages. For these analyses we use the matched employer-employee information in the INPS data to identify episodes of job loss due to mass layoffs at the firm level.[21] In line with the literature (Sullivan and von Wachter, 2009), we use mass layoff episodes in firms with more than 25 workers who reduce their total work force by more than 30% in a given year.

3.6 First descriptive evidence based on the INPS data

Table 2 provides some relevant initial evidence. The table shows that earnings gradually increase with age, that about three quarters of the workers enter the labor market as a blue collar worker and that this share declines with age. Figure 6 and 7 plot shares of blue collar workers and average earnings across the working career by treatment status. Table 2 also shows that 22% of the workers are beneficiaries of a disability benefit at some point during their working career, that at the end of our observation period about 82% of them are retired, and that the average age at retirement is about 58 years old.

HERE GOES TABLE 2

Table 3 provides the first descriptive evidence of the prenatal exposure to a Nazi raid on selected outcomes. Columns 1 and 2 report the mean of the outcomes for individuals born in municipalities without any Nazi raid during our observation period, while columns 3 and 4 report the same information for individuals born in municipalities with a raid episode. The distinction between before

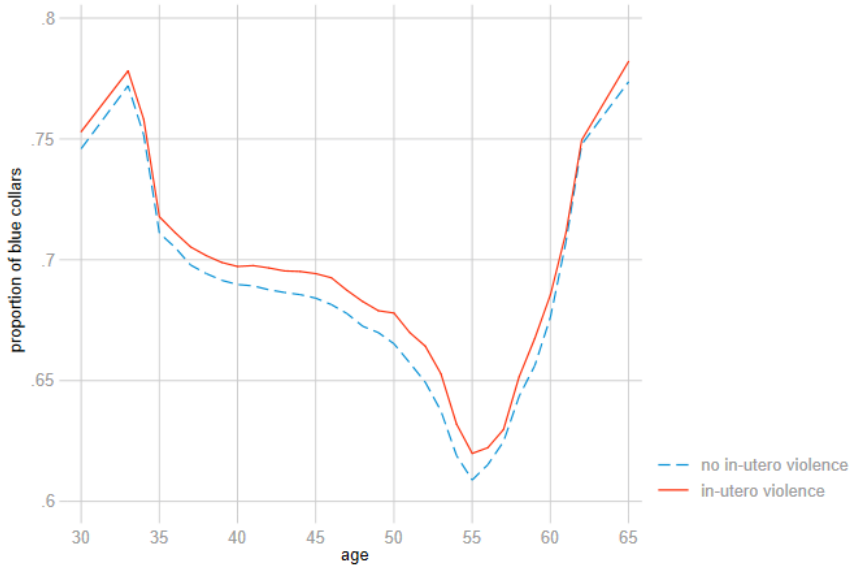
[20]The cohorts in our study have defined benefit (DB) pensions. Prior to 1996 the Italian system was financed by general taxation. In 1996, the Dini Reform transformed the scheme into a funded national defined contribution system (NDC). The new system was phased in through a long transitory process, 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 and therefore more than 99% of our sample is retired by 2011 and consequently falls under the DB scheme.

[21]Job loss due to a mass layoff is arguably more exogenous than an individual contract termination. See Sullivan and von Wachter (2009) for a discussion.

and after is based on the date of the Armistice (September 8, 1943). Finally, column 5 provides simple difference-in-differences calculations of the variable means. The results in Column 5 confirm 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 are also found to retire a bit later.

HERE GOES TABLE 3

Figure 6: Proportion of blue collar workers by treatment status

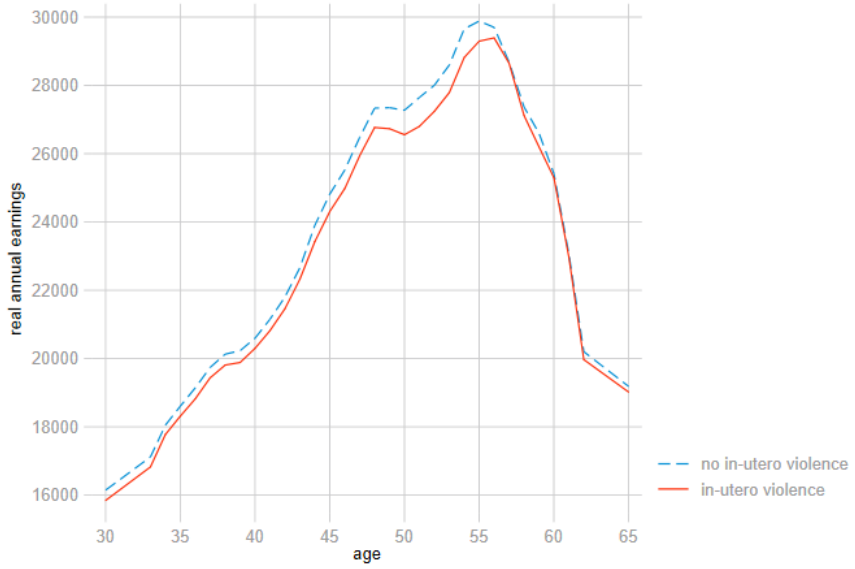


Notes: The numbers refer to 283,975 men working in the private sector between 1974 and 2017, born between January 1943 and May 1944 in municipalities with fewer than 200,000 residents. The figure plots differences between individuals exposed in-utero to a Nazi raid and those not exposed, controlling for municipality fixed effects.

4 Empirical model and identification strategy

The descriptive analyses shown above provide preliminary and tentative evidence of the effect of in-utero exposure to a Nazi raid on a range of outcomes. However, they do not exploit the exact timing of the raids, nor do they control for other circumstances, municipality characteristics and time fixed

Figure 7: Wage earnings 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. The figure plots differences between individuals exposed in-utero to a Nazi raid and those not exposed, controlling for municipality fixed effects. Annual earnings are expressed in 2005 euros.

effects, which might confound our preliminary reading of the evidence. In this section, we present our empirical model and identification strategy, which are designed to overcome these concerns.

As discussed in Section 2, the Armistice was unexpected and gave rise to a series of episodes of violent Nazi raids. These raids were intentionally unpredictable and idiosyncratically placed across time and space. Our empirical analyses exploits the random variation in the violent raids to assess the causal effects on a range of 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, measured 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. To control for general war effects we include War_{pt}, a z-score of the number of war related deaths in the province of birth p in the 9 months prior to the month of birth.[22] We also include municipality fixed effects (α_m), $year \times month$ 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.[23]

The coefficient β_1 is identified by comparing individuals exposed in utero to a Nazi raid to those exposed in the first year of life and those born in municipalities not exposed to a Nazi raid. As such, β_1 measures the effect of a Nazi raid over and above other general war effects (controlled for by War_{pt}). This margin is similar to Camacho (2008) and Quintana-Domeque and Rodenas-Serrano (2017) who study the effects of randomly placed terrorist attacks on birth outcomes in Columbia and Spain, respectively.[24] While it is plausible that the timing of the raids is unpredictable, one could argue that even after conditioning on municipality fixed effects the targeting of treated municipalities was not random. In section 6.2 we address this assumption of conditional random assignment with additional tests. In addition we also use an alternative identification strategy that only relies on the weaker assumption of random variation in the exact timing of the raids. Effectively we then compare the offspring of mothers who are exposed to a raid during pregnancy, as opposed to mothers exposed after giving birth. So, within this subsample of treated municipalities, all children face the same postnatal consequences of a raid, but only some of them are also exposed in utero. In this case, the effective margin that we use to identify β_1 is similar to that of Persson and Rossin-Slater (2018) who also exploit the timing of bereavement of a close relative of the mother to identify the causal effect on adolescent mental health of their offspring.

Some considerations are in order before we present our estimation results. It could be argued that the effects of a raid are not limited to the affected municipality, but also extend to neighboring control municipalities. If true, the control municipalities would be indirectly affected by the raid, which

[22]During a war the provision of food may be hampered, the stress levels of citizens may be higher, infectious diseases may become more prevalent and health care systems function sub-optimal.

[23]Spatial correlation may be relevant. One way of taking this into account is to cluster the standard errors at the province level. This hardly affects the standard errors.

[24](Camacho, 2008) uses administrative hospital data over the period 1998-2003. (Quintana-Domeque and Rodenas-Serrano, 2017) use administrative data on live births over the period 1980–2003. A potential drawback of these studies is that longer periods of potential exposure may lead to behavioral (fertility and mobility) responses.

may lead to an underestimate of the true effect of the raid. In section 6 we also provide the results of additional analyses where we exclude the neighboring control municipalities. Further, note that spillover effects are not relevant when we estimate our models on the sample of treated municipalities only. Both analyses show that spillovers are not important (see subsection 6.2)).

Further, Nazi raids may give rise to elevated cortisol levels and/or trauma for the parent(s) that persist for a longer period (Post Traumatic Stress Disorder, PTSD). PTSD may influence parental (health) behaviors and parenting skills when the child grows up (Akresh et al., 2012a; Christie et al., 2019). The systematic review of Christie et al. (2019) finds that PTSD is associated with impaired functioning across a number of parenting domains such as parenting stress, less optimal parent-child relationship and negative parenting practices. Further, the violent raids, led in some cases to property destruction and confiscation of economic resources, which in turn may have led to structural income losses. These lasting effects of a Nazi raid can have important implications for the interpretation of the parameter estimates of Equation 1. The estimate of in-utero exposure in Equation 1 will in this case include both a biological and a behavioral (PTSD)/income effect that may extend to later years. Additionally, in the baseline specification (equation 1) part of the reference group, namely those exposed to a Nazi raid post-birth, may also be affected by the PTSD/income effect, leading to a downward bias of the treatment effect. To examine the relevance of such effects we also estimate models that in addition to the in utero effect allow for the effect of a Nazi raid in the child's first and second year of life. Absence of statistically significant effects for the first and the second year of life suggests that in-utero exposure to the raids is primarily driven by the biological effect, rather then via income and/or altered parental behavior. Note that the test for the first and the second year effects is equivalent to a test of the common trends assumption. The results of this model, presented in the robustness section 6, show that only $prenatal$ exposure to the Nazi raids has lasting negative effect on labor market outcomes.

Besides the alternative models, section 6 also includes a range of additional robustness checks. These include, models where we replace WWII casualties (War_{pt}) and regional trends with flexible $province \times year \times month$ fixed effects. We also examine the sensitivity of our finding with respect to changes in the sample and the definition of the treatment variable. We further verify our identification

assumptions and the statistical power by a falsification test where we randomly assign the Nazi raid to each individual in the sample. In this section we also examine whether selection into the private sector, selective mobility and selective fertility and mortality may confound our findings. Lastly, in section 7 we address the interpretation of our findings. We argue that prenatal stress is likely to be the most important factor explaining our findings.

5 Results

In this section we present the main set of results based on Equation 1. The outcomes are defined at age 30/31 (when we observe individuals for the first time in 1974) and at later ages (until 2018). In Section 5.1 we examine the effects on earnings (in logarithms), blue collar status, disability and unemployment benefit receipt, retirement age, pension benefits (in logarithms) and mortality. In section 5.2 we present the results of job loss due to a mass layoff on subsequent labor earnings and mortality.

5.1 Labor market outcomes across the life cycle

We conduct our baseline analysis on the sample of 283,975 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). 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 prenatally exposed to a Nazi raid ('the exposed') and this effect does not dissipate over time. The penalty is slightly smaller at ages 40 and 45, but it widens at ages 50 and older. At age 60 the earnings penalty increases to about 5.5%. We also find significant negative general war effects (WWII casualties) on earnings.

HERE GOES TABLE 4

The persistent differences in earnings for the exposed could be due to differences in the type of jobs that people hold. However, sorting into lower skill jobs cannot alone explain the stark increase in the earnings penalty later in the working career. A possible reason for this pattern might be related

to interrupted working careers, for instance, due to unemployment or disability. An alternative explanation is retirement timing. The male cohorts considered started retiring as early as age 55 and by age 60 only 22% of these cohorts were still at work. This retirement effect may be selective with respect to earnings and the type of job.[25]

Table 5 shows the results for the type of job. The prenatally exposed 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).

HERE GOES TABLE 5

To understand to what extent sorting into low skilled blue collar jobs is mediated by the accumulation of human capital before labor market entry, we use information about educational attainment. We have information on educational attainment of individuals whose employment contract changed after 2010. This concerns 52% the sample of workers who are still in work after 2010 (19,379 out of 37,471 workers, see Section 3.2.3). A change in the labor contract might potentially be related to prenatal exposure to a Nazi raid. We therefore ran a regression that relates the presence in the education sample to in-utero exposure to a Nazi raid. Column 1 of Table 6 shows that there is no significant association between the two variables (t-statistic of 0.014, p-value exceeds 0.99). In column 2 and 3 we show the results for the effect of prenatal exposure to a Nazi raid on educational attainment: the exposed have lower levels of educational attainment. This suggest that the traumatic experience of a Nazi raid affects cognition and education, which in turn may affect job skill levels and earnings. Previous work by Aizer et al. (2016) has found effects of elevated Cortisol levels of the mother on offspring cognition and educational attainment.[26]

HERE GOES TABLE 6

[25]Italian pension benefits that apply to these cohorts are 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, in additional analyses we find that the earnings penalty at the last measured earnings before retirement is smaller than the penalty at age 60 (-0.028, s.e.= 0.009 instead of -0.0551, s.e. = 0.0194).

[26]This supports our interpretation of stress driving our findings (see Section 7 for more analyses to support this interpretation).

In Table 7 we present the results for disability, the number of unemployment benefit claims before age 60, the age at retirement, pension benefits and mortality. 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 at retirement.

HERE GOES TABLE 7

The results in Table 7 show a small and insignificant effect for disability (insurance receipt), but a sizeable and marginally significant effect for the number of unemployment claims prior to age 60. This last result suggests that at least part of the earnings penalty at age 60 for our treated population may be due to interruptions in their labor market careers. The role of later retirement for lower earnings (blue collar) workers in determining a pronounced earnings penalty at age 60 (see Table 5) is supported by the results in Table 7, although the coefficient on retirement age is not statistically significant. Pension benefits are related to average earnings ad therefore the exposed have lower pension benefits upon retirement. Finally, Table 7 shows that there is no differential mortality by treatment status.

5.2 What if lightening strikes twice?[27]

The evidence presented above showed that the earnings gap increases with age and suggests that job interruptions due to unemployment may also play a role. What happens if the prenatally exposed are exposed to a second shock (job loss) later in life? Or, what if "lightening strikes twice"? Work by Cunha and Heckman (2007) and Almond et al. (2018) argues that dynamic complementarities in skills may be important. Dynamic complementarities in skills, refer to the idea that investments made in later periods are more productive when the baseline stock of skills is higher. Conversely, a negative shock early in life may amplify the effects of a negative shock later in life. In our context this may imply that the consequences of job loss later in life may be stronger for those who have been exposed prenatally to a Nazi raid. Medical studies also point in that direction: in-utero exposure to

[27]Sub-section title adapted from Almond et al. (2018), Section 5.

stress affects the cognition and stress vulnerability of the offspring (see e.g. Boersma and Tamashiro (2014)).

To address the sensitivity of later life shocks, following a shock early in life, we use information on mass layoffs available in the linked worker firm data from INPS. Contract terminations resulting from mass layoffs are particularly suitable in this setting, as this type of job separations are more likely to be exogenous from an individual point of view. Specifically, we examine whether the effect of job loss due to a mass layoff on subsequent earnings and mortality is different for those prenatally exposed to a Nazi raids.

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).

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 for the period 1983 (aged 40) to 2004 (aged 61).[28]

For the causal interpretation of our estimates we first have to rule out potential endogenous allocation of mass layoffs to prenatally exposed workers. Table B3 of Appendix B.2 shows that there are no effects of prenatal exposure (or general WWII effects) on the probability of working in a firm that experiences a mass layoff.[29]

Subsequently, we specify a more general model to assess the causal effect of job loss due to mass layoff (LO) for prenatally exposed workers:

$$y^a_{imt} = \beta^a_0 + \beta^a_1 Nazi_{mt} + \beta^a_2 LO^a_{it} + \beta^a_3 LO^a_{it} * Nazi_{mt} + \beta^a_4 * War_{pt} + \alpha^a_m + \gamma^a_t + \delta^a_{tr} + \epsilon^a_{imt}, \quad (2)$$

[28]Participation rates for older workers decline rapidly after age 60. At age 60 only about 22% of male workers are still at work.

[29]Also, neither blue collar, or white collar workers are over represented in mass layoffs (results available upon request).

where LO_{it}^{a} is an indicator that equals 1 if an individual i, born at time t loses his job due to a mass layoff at age $a-1$. As in equation 1, β_1^a measures the average effect of in-utero exposure to a Nazi raid of those who are born in a village that was subject to a Nazi raid. The parameter β_2^a is the effect of a layoff, the effect usually reported in the existing literature 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_3^a measures the *additional* effect of a job loss due to a mass layoff for the prenatally exposed. So the sum β_1^a and β_3^a is the $total$ effect of a layoff for those who are prenatally exposed. When our outcome is earnings, we measure the effect on next year's earnings. For mortality, we look at the effect on the probability of dying within the next 10 years.

Table 8 presents the estimates of Equation 2 on next year earnings from age 45 to 60. The effect of prenatal exposure to Nazi raid (β_1^a, in the first row) is remarkably similar to the general treatment effect presented in Table 4, hinting that the assignment of the mass layoff is independent from prenatal exposure to a Nazi raid. The estimates of β_2^a indicate that a layoff 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 for the US earnings penalties of about 32–33%. Jacobson et al. (1993) find immediate losses of more than 40%. Importantly, the estimates of β_3^a show that the prenatally exposed, suffer an additional earnings penalty at ages 45, 50 and 55 of about 8%, 10% and 15%, respectively.[30] So, the results in Table 8 show that an average worker faces an immediate earnings loss aftyer job loss (due to a mass layoff) of about 31–33%, which can increase to more than 47% ($\beta_1^a + \beta_3^a$) for the prenatally exposed to a Nazi raid.

HERE GOES TABLE 8

Finally, we also examine whether the layoff leads to increased mortality rates within 10 years following the event. The results of this analysis (Table 9) show, in line with the literature (Sullivan and von Wachter, 2009), that job loss leads to increased 10-year mortality rates. However, we do not find evidence of additional effects for those prenatally exposed to a Nazi raid.

[30]Note that the effect at age 55 rather than at age 60 is most relevant for the large earnings penalty at age 60 (cf Table 4).

HERE GOES TABLE 9

Our finding above on the interaction between prenatal exposure to a Nazi raid and another exogenous labor market shock later in life provides support for the idea of dynamic complementarities in skills (Almond et al., 2018; Cunha and Heckman, 2007). It is difficult to test empirically for the relevance of dynamic complementarities in skills. 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 in the second shock later in life. Such settings are rare in practice, but our empirical design combined with an extremely long follow up period allows us to address this issue.[31]

Taken together, the results in this section indicate that a traumatic experience of the mother leads to lower earnings of her offspring. This effect increases with age and ultimately leads to lower pensions in retirement. These negative outcomes are due worse educational outcomes, lower skilled jobs and to interruptions in the working career. Our results for the effects of a layoff later in life indicate that those exposed prenatally to a negative shock face higher earnings penalties at later ages after job loss. This suggests that dynamic complementarities may be important.

6 Robustness checks, falsification tests and selection effects

In this section we start with estimates from an alternative model that tests for the relevance of trauma related changes in the behavior and parenting skills of the parents (Section 6.1). This alternative model implicitly also tests for the common trend assumption that is crucial for the causal interpretation of our estimates of β_1 in equation (1) (for earnings and blue collar status) and β_3 in equation (2) (mass layoff effects). In subsection 6.2 we examine the assumption of conditional random assignment and test for possible spillover effects. We next perform several robustness checks (6.4). The results of these analyses are summarized in Panels B-G of Tables 10 (earnings), 11 (blue collar status) and

[31] Still, as pointed out by Malamud et al. (2016), exogeneity of the two shocks is a necessary, but not sufficient condition. Parents may respond with investments in their children to counter the negative effects of a bad start. This may confound a test for dynamic complementarities in skills. For instance, the effect of the second shock will be biased towards zero if parents make investments to compensate for adversities due to the prenatal shock. On the other hand, our estimates will overstate the effects if parents make reinforcing investments, e.g. they invest more in high endowed children (those who are not exposed to the prenatal shock).

12 (mass layoffs). The baseline results are repeated in Panel A. The tables only report the effect estimates of β_1 and β_3. The full tables can be found in the online Appendix D. In Section B.4) we report the results of a falsification tests. Section 6.6 addresses issues of selective fertility, mortality and mobility.

6.1 Post Traumatic Stress Disorder (PTSD) and the Common trend assumption

As discussed in Section 4, witnessing a violence may lead to a lasting trauma for the parent(s) which in turn may influence parental (health) behavior and parenting skills when the child grows up (Akresh et al., 2012a,b). Furthermore, in case of persistent income loss resulting from a Nazi raid, victims might suffer from longer-term consequences. In the presence of such effects the estimate of in-utero exposure in Equation 1 will include a biological and a behavioral (PTSD) and/or income effect. We examine this by augmenting the specification in Equation (1) with separate effects for exposure in the first and the second year of life. For this purpose we extend the period of the analysis to the left, i.e. the time window is [-24,9] around September 8^{th}, 1943. This alternative specification resembles an event study approach and implicitly allows for a test on the common trend assumption.

The results of these analyses are reported in Panel B of Tables 10, 11 and 12. We see in the top row of Panel B 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 two following rows for the first and the second year effects show small and insignificantly coefficients. This indicates that the common trends assumption is satisfied, that it is primarily prenatal exposure that drives our findings and that the effect estimates reflect primarily a biological effect, rather than effects of reduced income and/or altered parental and parenting behavior post-birth.

6.2 Conditional random assignment assumption / a different identification strategy

As argued in Section 2, Nazi raids were idiosyncratically distributed across time and space. For this reason, in Equation 1 the control group includes those who are born in municipalities that were not subjected to a Nazi raid and those born in treated municipalities post birth. While the exact timing of the raids was hard to predict, one could argue that the assumption of conditional random assignment across municipalities may not hold. In Appendix A we provide evidence that the assumption of

conditional random assignment can not be rejected. Additionally, we estimate Equation (1) for the sub-sample of treated municipalities only. In this case, identification relies solely on the weaker assumption of randomness in the exact timing of the treatment. This is the same strategy that Persson and Rossin-Slater (2018) use to examine the effect of in-utero exposure of the child to stress due to the loss of a parent or close relative of the pregnant mother. The results of this exercise for earnings, blue collar status and mass layoffs are reported in Panel C of Tables 10, 11 and 12. The results are very similar for blue collar status and mass layoffs, but become smaller and less precise for earnings at age 55 and 60.

6.3 Testing for spillover effects

The information of the occurrence of a Nazi raid may also transmit to neighboring villages/municipalities and fear for a Nazi raid may also impact their residents. If this is the case, estimates of β_1 (Equation 1) and β_3 (Equation 2) estimated on the full sample of treated and control municipalities are likely to be downward biased. To examine the relevance of such effects we excluded the municipalities that underwent a raid. Next, in the remaining sample (of the controls) we defined the municipalities in the vicinity (within a distance of 5km) of a treated municipality as treated. Significant estimates of β_1 (earnings and blue collar status) and β_3 (layoffs) indicate relevance of spillover effects. Panel D Tables 10, 11 and 12 present the results. The estimates are small and insignificant, indicating that spillover effects are not important.[32].[33]

6.4 Flexible province trends and different samples

Flexible province trends

Equation 1 may not be sufficiently flexible to capture general war effects at the province level. We therefore also estimated more flexible models where we replace War_{pt} and the regional trends by

[32]We additionally performed analyses where we keep the treated municipalities in the sample, but exclude nearby (5km radius) municipalities. These regressions gave results that where virtually identical to the baseline results (Panel A). Results available upon request

[33]Further, note that spillover effects do not play a role when we use the alternative identification strategy that only uses the sample of treated municipalities (i.e. that exploits randomness in the exact timing of the raid). The results of these analyses (see above, Panel C) are very similar, also suggesting that spillover effects to neighboring municipalities are likely to be of limited importance.

$province \times year \times month$ fixed effects. The results displayed in Panel E of Tables 10, 11 and 12 show that this does not affect the effects of Nazi raids on earnings, blue collar status and mass layoffs.

Changes in the length of gestation

As a further sensitivity test, we explore the effect of changes in the length of gestation. As we will argue later in Section 7, stress is likely to be an important factor explaining our findings. Maternal stress increases the likelihood of a preterm birth (Lilliecreutz et al., 2016), in the medical literature defined as a gestational period of less than 37 weeks. We derived the exposure variable $Nazi_{mt}$ assuming a 9 month gestation period (see Section 3.4). With shorter gestation some of those exposed may in fact have been conceived after September 8, 1943, or may have been exposed post birth. This measurement error may bias the parameter estimates (downward). Unfortunately, we only observe births and lack information on gestational length or preterm status. We therefore proceed differently and examine the sensitivity of our results by estimating Equation 1 and 2 assuming both an eight month and a seven month gestation period.[34] We report the results of this exercise in Panel F of Tables 10, 11 and 12. Reducing the gestation period, increases the magnitude and significance of the estimates.

Using only information from the first 6 months

The Armistice announced on September 8, 1943 was unexpected and therefore it can be argued that the population had not anticipated the series of events that followed. This limits the role of fertility responses (see also the Section 6.6). However, as the war went on, more information about the raids and the WWII in general may have disseminated across Italy. This may imply that with time, stress may have gained in importance in control municipalities, leading to a downward bias in our effect estimates. Further, Table B2 in Appendix B.1 shows that in the first six months after September 8, the raids involved primarily civilian victims. For these reasons we also estimated our main model using the data up to 6 months after September 8, 1943. Note that by using this restricted time window we also reduce measurement error in the exposure variable ($Nazi_{mt}$) (see above). Results using only information of those born in the interval [-9,6] around the Armistice are reported in Panel G of Tables

[34]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.

10, 11 and 12 present the results. D10 of Appendix D4. The tables shows that the effects are very similar and quantitatively even gain in importance.

Changing the municipality size cut-off.

The nature of the war was different in large cities. Also, raids in large cities generally were more brutal and involved more victims and therefore nearby areas were more likely to hear about these raids. We therefore restricted our sample to municipalities with less than 200,000 inhabitants. To examine the sensitivity of our findings to this cut-off we also perform additional analyses where we vary the municipality size. The results are in Panel H and I of Tables 10, 11 and 12. Panel H shows the results if we use a cap of 500,000 inhabitants, Panel I is we also include the big cities (i.e. no cap on the size of the municipality size. As expected (see the discussion in Section 3.3), relaxing the cap leads to smaller and less precise parameter estimates.

HERE GOES TABLESS 10, 11 and 12

6.5 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. More precisely, we randomly assign the Nazi raids to each individual in our sample. If our identification strategy is valid, we would expect estimates using those pseudo-samples to be centered around zero. We can then confront our baseline estimates with the results from the pseudo-sample. We 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 in Figures B3 and B4 of Appendix B.4 (results for other ages 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. Specifically, the share of t-values that (in absolute terms) exceeds the t-statistics from our main model is less than 1%. These p-values give confidence in our design and statistical power of our exercises.

6.6 Selection effects

In this Section we address several problems related to sample selection, selective fertility, mobility and mortality.

Private sector worker selection.

Our analyses are based on a sample of private sector workers and this selection on sector of work may be related to the treatment variable. This selection may affect labor market outcomes and may also question the external validity of our findings. As the INPS data include pension information of all workers, both working in the private and public sector, we can used the INPS data to examine whether those prenatally exposed to a Nazi raid were more or less likely to sort into private sector jobs. We find that they did not. More specifically, we estimated a model where we regressed an indicator for working in the private sector on in utero exposure to a Nazi raid, municipality fixed effects, the number war casualties, year × month fixed effects and regional trends. The coefficient of the Nazi raid is small (-0.0015) and insignificantly different from zero (s.e. 0.0011, p-value of 0,17).

Selection issues at conception, in utero and in later life

Confounding due to selection can take place at several stages in the life cycle: *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 conceived prior to the date of the unexpected proclamation of the Armistice. Thus fertility decisions in treated and control municipalities were made with similar priors. Still one might argue that conception rates vary with socio-economic status and that there are structural differences between municipalities. Note, however, that in all our regressions we control for municipality specific fixed effects.

Further, selective mortality due to reasons *ii* - *iv* is likely to lead to survival of the stronger individuals. In this case our estimates are likely to be a lower bound of the true effects. It would still be informative to test for the relevance of such selection effects. To deal with in-utero selection, we ideally would like to 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 gave 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 WWII casualties (War_{pt}), Nazi raids and year × months effects. Table B4 of Appendix B.5 shows that the Nazi raids did not increase mortality due to pregnancy complications.

Mortality selection after conception can take place in utero (miscarriages), between birth and the first time that we observe the individuals at age 30 and at later ages. The results in column 2 of Table 7 showed that there is no mortality selection between age 30 and 60 (reason *iv*). In order to test for selective mortality due to miscarriages and between birth and age 30 (reasons *ii* and *iii*), we examine standardized cohort sizes at the municipality level. Negative values are associated with lower than average cohort sizes and vice versa. Mortality selection for reasons *ii* and *iii* is relevant if the variation in cohort sized is related to our treatment variable. To test for endogenous selection effects we estimated the following regression on the sample of treated and control municipalities:

$$CS^a_{mt} = \beta^a_0 + \beta^a_1 Nazi_{mt} + \beta^a_2 War_{pt} + \alpha^a_m + \gamma^a_t + \delta^a_{tr} + \epsilon^a_t, \tag{3}$$

where CS^a_{mt} is the standardized cohort size at age a (a = 30) in municipality m at time t (for t = Jan 1943, . . . , May 1944), $Nazi_{mt}$ is an indicator that equals one if municipality m at time t was exposed to a Nazi raid, War_{pt} the number of casualties, α^a_m are municipality fixed effects, γ^a_t are year × month fixed effects, δ^a_{tr} regional trends and an idiosyncratic error term ϵ^a_{mt}.

Our main interest is in the coefficient β^a_1. A significant negative effect indicates a smaller cohort size in the months that a municipality was subject to a Nazi raid (and vice versa). [35] We report this coefficient in Table B5 of Appendix B.5. The results show that the cohort size of a municipality is not significantly different in the months when a Nazi raid took place. This suggests that selection effects for reasons *ii* and *iii* are not likely to bias our effect estimates.

Selective mobility

[35] Note that β^a_1 may also pick up fertility effects.

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 was very difficult due to the destruction of railroads and transport networks (Baldoli and Knapp, 2012). To empirically test for selective mobility responses we use the sample of treated and control municipalities and estimate an event history model that relates the standardized municipality cohort sizes to leads and lags of Nazi raid exposure, province level general war effects War_{pt}, municipality fixed effects and year $\times$ month effects. For this we need a normalization. We take the normalization at the month of a Nazi raid. A positive coefficient indicates that the municipality cohort size in a specific month is larger than the cohort size in the month prior to the Nazi raid (and vice versa). Residential mobility may affect our effect estimates if prior to the month of the raid people moved out of the municipality. In that case we should find significantly lower cohort sizes prior to the month of the Nazi raid. Note that this would also be indicative of non-random allocation of the Nazi raids. Figure B5 in Appendix B.5 reports the estimated leads and lag coefficients (relative to the month prior to the Nazi raid). The figure shows that the lead and lag effects are very small (less than 0.05 of a standard deviation) and insignificantly different from zero. Hence, cohort sizes are relatively constant and endogenous residential mobility is not likely to have played an important role in the 9 month window surrounding that date of the Armistice.

7 Interpreting our results: Is it stress?

During a war the provision of food may be hampered, the stress levels of citizens may be higher, infectious diseases may become more prevalent and health care systems function sub-optimal. In our regressions we control for such effects with the variable (War_{pt}).[36] Therefore, $Nazi_{mt}$ measures the *additional* effect of a Nazi raid, over and above general war effects. Below we argue that maternal stress is likely to be the main driver of this *additional* effect of a Nazi raid. However, it should be noted that one can never rule out that other mechanisms also play a role, even with actual observations on stress hormone (Cortisol) levels.

There is an extensive body of medical research documenting that parental stress may transfer to

[36]We also replaced War_{pt} by flexible province $\times$ year $\times$ month fixed effects.

child cognition and mental well-being and that this can start in the earliest phases of life. Specifically, medical 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 (see Boersma and Tamashiro, 2014; Cotter and Pariante, 2002; Marmot et al., 1991; Van den Bergh et al., 2005; Weinstock, 2005, and the literature reviewed in these papers).

Recent economic literature has given support to these claims by identifying 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). Our findings for educational attainment (Section 5.1) are in line with the findings of these studies. Below we examine whether, even after controlling for general war effects, the variable $Nazi_{mt}$, may pick up other possible effects.

7.1 Supporting evidence in favor of a stress interpretation

Is it prenatal? In Section 4 we referred to studies showing that the effects of a psychological trauma may persist over a longer time period and affect parental health (behaviors) and parenting skills. Moreover, the Nazi raids may also have affected the longer term income of the families involved. This all implies that the effects of the raids may not be limited to the prenatal period, but also extend to later childhood ages. In subsection 6.1 we reported the results for models that include possible effects of exposure in the first and second year of life and concluded that there is no evidence for lasting (behavioral) effects. This means that the effects are primarily driven by prenatal conditions and that behavioral effects, such as parental health (behaviors) and parenting skills, and family income, are not driving the effect estimates of the Nazi raid.

Is it stress or are there other war related conditions such as malnutrition that drive our results? We would like to stress once more that the effect estimate β_1 in equations 1 and 2 reflect the $additional$ effect of a Nazi raid over and above the general war effects (controlled for by War_{pt}).

Still, it could be the case that the variable War_{pt} may be a poor proxy of the general war effects and may therefore also affect the estimate of the effect of a Nazi raid as well as our interpretation of

this effect. We also estimated more flexible models where we replaced War_{pt} and the regional time trends by flexible province × year × month fixed effects. These models provided virtually identical parameter estimates for all our outcome variables (see above in subsection 6.4.)

We additionally use information from a medical database to examine more directly whether the *additional* effect of the Nazi raid primarily reflects stress. This database, the Health Search/CSD Patient Database, is a nation-wide representative sample of Italian adult patients, containing patient electronic clinical records (ECRs) from General Practitioners (GP). The records include ATC (Anatomic Therapeutic Chemical) drug classification codes. With this information we can compute drug expenditures for specific disease types at the patient level. More information about this database and the Italian health care system can be found in Appendix C. We regressed health expenditure distinguished by ATC codes on our treatment variable, controlling for age, WWII casualties at the province level, GP fixed effects, municipality fixed effects, time fixed effects and regional trends. Tables C1, C2 and C3 in Appendix C show that in-utero exposure to a Nazi raid only has a significant and sizable effect on health expenditure for diseases of the nervous system and mental disorders. 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, 1996; Heffelfinger and Newcomer, 2001; Selten et al., 1999). This finding also aligns with the results of Persson and Rossin-Slater (2018) who find effects of adolescent mental health (care).

Still, Italy was at war in the period that we consider and disruptions due to the war, notably food shortages, may also drive our effect estimate of the Nazi raid if this resulted in food shortages at the municipality level. Indeed, during and just after the war years some countries (Greece (1941-1942), Russia (Leningrad, 1942), The Netherlands (1944-1945), Germany (1945-1946)) experienced drastic famines, with daily food intake dropping to 500-1,000 calories per day (Van den Berg and Lindeboom, 2018). However, the medical and epidemiological literature generally agrees that famine exposure has the strongest association with weight, diabetes and hypertension (Van den Berg and Lindeboom, 2018). Lumey et al. (2011) review observational studies based on the Dutch, Chinese, and Leningrad famines and show a persistent association across the three famines between famine exposure early in

life and weight, diabetes and schizophrenia.

While Italy's food supply was not as limited as in these countries, still in some areas food availability was not sufficient during WWII. Up to 1942 food production in Italy was at about the pre-war levels (2600 calories per day), but food shortages became more apparent in 1942 and the supply dropped to about 2000 calories per day to remain at these levels until the end of the war (Daniele and Ghezzi, 2017; Ó Gráda, 2019; Vecchi, 2017). This is substantially higher than the calorie intake of people during the famines of the countries mentioned above. Indeed, the regression results in Tables C1, C2 and C3 of Appendix C show that there is no association between prenatal exposure to a Nazi raid and cardiovascular conditions and diabetes (see the second column of the Tables), conditions generally found in famine studies.

Further, in support of this finding, there was a stark rural - urban distinction: nutrition of rural households, was quantitatively not significantly dissimilar to that of the pre-war years, while a substantial part of non-rural households suffered from starvation (Daniele and Ghezzi, 2017). Indeed, larger cities generally, due to political and strategic reasons, suffered more from bombings, mass destruction and nutritional shortages. For this reason we base our analyses on municipalities with less than 200,000 inhabitants. These municipalities mostly belong to rural areas.

8 Conclusions and Discussion

This paper complements the previous contributions in the literature by examining the causal effect of a traumatic and stressful event experienced by pregnant women on life long labor market outcomes of their child. We exploit a unique natural experiment that involved short lived, randomly placed violent Nazi raids across municipalities after the Armistice of 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 and find that the prenatally exposed to a Nazi raid have an earnings penalty of about 2% at the start of their working career. This effect

increases to about 5.5% at age 60. These lower earnings translate into lower pensions. The lower earnings are due to lower educational attainment, the type of jobs that they hold and interruptions in their working careers due to unemployment. We find that a bad start (i.e. prenatal exposure) exacerbates the negative effects of later life job loss on earnings, deepening the negative impact on earnings at later ages. These job loss effects on earnings are substantial: between 31–34% for all workers and up to 47% for workers exposed prenatally to a traumatic and stressful event. Our findings are robust against a number of specification and falsification tests.

The results for job loss due to a mass layoff also suggest that dynamic complementarities in skills (Cunha and Heckman, 2007) may be 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.

We use a medical database on health expenditures to interpret the effect estimates. Previous work in medical sciences, epidemiology and economics has documented negative effects of prenatal stress exposure on various psycho-pathologies later in life such as decreased learning function, cognition, education and mental health. Indeed, we find a lower educational attainment for those who are prenatally exposed to a Nazi raid and higher medical care expenditures on diseases of the nervous system and mental disorders. We also show that the effects of a Nazi raid do not include effects of hunger, changes in maternal health, maternal health behaviors and reduced family income.

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).[37]

In addition to a different stressor, the outcomes in our study are also different from the outcomes

[37]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 previous work, and the subjects involved are much older. For our set of adult males we find that those aged 60–65, who were exposed in utero to stress, are about 3.6 percentage points more likely (about 13% increase) to purchase drugs for treating diseases of the nervous system and mental disorders, and they have 17% 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. 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 0.32.[38]

The total costs associated with prenatal stress are of course substantially higher since these costs also include other medical costs, increased social security spending, reduced productivity and life time income. The per-person present discounted value (PDV) of income losses (in 2005 euros) due to additional prenatal stress caused by the Nazi raids is estimated at 14,219 euros. This is equal to about one year of earnings at age 30 (see Table 2).[39]

What about external validity? Our study concerns a historical context that could be qualified as extreme and rare. We feel, however, that the context in this paper extends to settings still observed today. Unfortunately, war related conflicts that involve aggression and violence against civilians remain relevant, especially in developing countries. Besides, stressful conditions are not limited to war like situations, but also hold for deprived neighborhoods, where crime, unemployment and poverty rates are high. Further, the current COVID-19 pandemic is associated with fear and stress for health, economic security and well-being of individuals and their families. Such adversities affect

[38]The reduction in log earnings at age 60 equals 5.5%, while the relative change in expenditures equals 17%.

[39]The per-person PDV of life-long income loss is derived using:

$$PDV^{loss} = \sum_{a=30}^{100} \frac{\beta_1^a \times \overline{income_a}}{(1+i)^{a-30}} \times P(a+1|a) \tag{4}$$

where $\beta_1^a \times \overline{income_a}$ is the yearly income loss due to prenatal exposure at age a, using Table 4, i is the discount factor, which we set at 2%, and $P(a+1|a)$ as the probability of survival to age $a+1$, given survival up to age a.

families with poor qualifications and limited resources most. Therefore, stressful conditions may play an important role in the persistence of low socio-economic status across generations (see also Aizer et al., 2016). Our findings suggests that public 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.

Tables

Table 1: 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: Nazi raids between Sept 1943 – May 1944 occurring in Italian municipalities with resident population under 200,000.

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. Earnings 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 2018 born in January 1943–May 1944 in municipalities with fewer than 200,000 residents. The sample size varies according to outcome variable and to when it was measured. Wages and pensions are expressed in 2005 euros. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Table 4: The effect of in-utero exposure to a Nazi raid: Age specific (log) 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 year × month fixed effects, municipality fixed effects, and 20 region specific time trends. Robust standard errors (in parentheses) are clustered at the municipality level.

Table 5: The effect of in-utero exposure to a Nazi raid: 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 year × month fixed effects, municipality fixed effects, and region specific time trends. Robust standard errors (in parentheses) are clustered at the municipality level.

Table 6: The effect of in-utero exposure to a Nazi raid: Education attainment

	Presence in education sample	Less than secondary education	Tertiary education
Nazi raid	-.00004	0.0450**	-0.0357***
in utero	(0.0028)	(0.0188)	(0.0121)
WWII casualties (SD)	0.0005	0.0104	-0.0035
in utero	(0.0013)	(0.0076)	(0.0053)
R^2	0.0384	0.2359	0.2122
N	283,741	19,397	19,397
Time FEs	YES	YES	YES
Municipality FEs	YES	YES	YES
Reg trends	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. All regressions include year × month fixed effects, municipality fixed effects, and region specific time trends. Robust standard errors (in parentheses) are clustered at the municipality level.

Table 7: The effect of in-utero exposure to a Nazi raid: Other outcomes before age 60

	Disability before 60	Dead before 60	Age at retirement	First pension (log)	# Unemployment claims 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	283,975	283,975	227,987	227,987	283,975
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 283,975 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 claims. Age at retirement and first pension are the first outcome occurring between 1974 and 2017. All regressions include year × month fixed effects, municipality fixed effects, and region specific time trends. Standard errors (in parentheses) are clustered at the municipality level.

Table 8: The effect of in-utero exposure to a Nazi raid: Effects of a 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. All regressions include year × month and municipality fixed effects as well as region specific time trends. Standard errors (in parentheses) are clustered at the municipality level.

Table 9: The effect of in-utero exposure to a Nazi raid: Effect of mass layoffs on probability of death within 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. All regressions include year × month and municipality fixed effects as well as region specific time trends. Standard errors (in parentheses) are clustered at the municipality level.

Table 10: Robustness checks: Age specific (log) earnings

	age 30	age 35	age 40	age 45	age 50	age 55	age 60
(*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)
N	211,641	207,420	187,049	170,775	158,164	101,081	47,560
(*B*) *Post Traumatic Stress Disorder (PTSD) and the Common trend assumption [-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)
N	376,895	386,610	354,811	319,900	299,326	191,942	89,267
(*C*) *Conditional random assignment assumption/a different identification strategy [-9,9] window*							
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)
N	93,778	91,319	82,300	75,373	70,457	43,375	18,945
(*D*) *Baseline model - spatial spillovers - 5km radius [-9,9] window*							
Nazi raid	0.0031	0.0001	-0.0045	-0.0009	0.0052	-0.0161	0.0081
in utero	(0.0179)	(0.0169)	(0.0152)	(0.0166)	(0.0169)	(0.0263)	(0.0409)
N	195,406	191,349	172,793	157,893	146,512	92,692	43,255
(*E*) *Flexible province trends [-9,9] window*							
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)
N	93,778	91,319	82,300	75,373	70,457	43,375	18,945
(*F*) *Changes in the length of gestation [-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)
N	201,078	197,052	177,859	162,270	150,403	95,783	45,091
(*G*) *Changes in the length of gestation [-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)
N	189,423	185,605	167,716	152,960	141,929	89,947	42,377
(*H*) *Using only information from the first 6 months [-9,6] window*							
Nazi raid	-0.0228**	-0.0264***	-0.0211**	-0.0221**	-0.0276**	-0.0391***	-0.0692***
in utero	(0.0102)	(0.0096)	(0.0097)	(0.0095)	(0.0107)	(0.0140)	(0.0234)
N	175,883	172,306	155,884	141,936	131,898	83,162	39,307
(*I*) *Changing the municipality size cut-off - municipalities under 500k residents [-9,9] window*							
Nazi raid	-0.0244***	-0.0268***	-0.0140**	-0.0191**	-0.0294***	-0.0310***	-0.0526***
in utero	(0.0076)	(0.0070)	(0.0071)	(0.0075)	(0.0081)	(0.0116)	(0.0184)
N	222,610	218,207	196,613	179,350	166,095	106,767	50,244
(*J*) *Changing the municipality size cut-off - municipalities no cap on size [-9,9] window*							
Nazi raid	-0.0158**	-0.0159**	-0.0077	-0.0116	-0.0203**	-0.0252**	-0.0463***
in utero	(0.0076)	(0.0068)	(0.0068)	(0.0079)	(0.0080)	(0.0119)	(0.0156)
N	242,079	237,520	213,823	194,641	179,792	118,123	56,271

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

Notes: Unless otherwise specified, all estimates are based on municipalities with fewer then 200,000 residents, are obtained for age specific subsamples (30 to 60), include year × month fixed effects, municipality fixed effects as well as region specific time trends. Robust standard errors (in parentheses) are clustered at the municipality level.

Table 11: Robustness checks: Age specific blue collar status

	age 30	age 35	age 40	age 45	age 50	age 55	age 60
(*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)
N	211,641	207,420	187,049	170,775	158,164	101,081	47,560
(*B*) *Post Traumatic Stress Disorder (PTSD) and the Common trend assumption [-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)
N	376,895	386,610	354,811	319,900	299,326	191,942	89,267
(*C*) *Conditional random assignment assumption/a different identification strategy [-9,9] window*							
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)
N	93,778	91,319	82,300	75,373	70,457	43,375	18,945
(*D*) *Baseline model - spatial spillovers - 5km radius [-9,9] window*							
Nazi raid	-0.0027	-0.0023	-0.0110	-0.0090	0.0001	-0.0173	-0.0337
in utero	(0.0095)	(0.0097)	(0.0110)	(0.0111)	(0.0123)	(0.0162)	(0.0249)
N	195,406	191,349	172,793	157,893	146,512	92,692	43,255
(*E*) *Flexible province trends [-9,9] window*							
Nazi raid	0.0202***	0.0004	0.0135**	0.0132*	0.0117*	0.0195**	0.0399***
in utero	(0.0059)	(0.0070)	(0.0066)	(0.0068)	(0.0070)	(0.0079)	(0.0119)
N	211,641	207,420	187,049	170,775	158,164	101,081	47,560
(*F*) *Changes in the length of gestation [-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)
N	201,078	197,052	177,859	162,270	150,403	95,783	45,091
(*G*) *Changes in the length of gestation [-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)
N	189,423	185,605	167,716	152,960	141,929	89,947	42,377
(*H*) *Using only information from the first 6 months [-9,6] window*							
Nazi raid	0.0214***	0.0053	0.0189**	0.0141*	0.0155**	0.0229***	0.0308**
in utero	(0.0065)	(0.0075)	(0.0074)	(0.0073)	(0.0077)	(0.0087)	(0.0129)
N	175,883	172,306	155,884	141,936	131,898	83,162	39,307
(*I*) *Changing the municipality size cut-off - municipalities under 500k residents [-9,9] window*							
Nazi raid	0.0284***	0.0042	0.0220***	0.0212***	0.0223***	0.0308***	0.0419***
in utero	(0.0055)	(0.0063)	(0.0060)	(0.0058)	(0.0060)	(0.0069)	(0.0103)
N	222,610	218,207	196,613	179,350	166,095	106,767	50,244
(*J*) *Changing the municipality size cut-off - municipalities no cap on size [-9,9] window*							
Nazi raid	0.0218***	-0.0060	0.0155***	0.0168***	0.0168***	0.0234***	0.0258***
in utero	(0.0056)	(0.0063)	(0.0059)	(0.0063)	(0.0061)	(0.0070)	(0.0097)
N	242,079	237,520	213,823	194,641	179,792	118,123	56,271

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

Notes: Unless otherwise specified, all estimates are based on municipalities with fewer then 200,000 residents, are obtained for age specific subsamples (30 to 60), include year × month fixed effects, municipality fixed effects as well as region specific time trends. Robust standard errors (in parentheses) are clustered at the municipality level.

Table 12: Robustness checks: Effect of a mass layoff on (log) earnings in the following year

	age 45	age 50	age 55	age 60
(*A*) *Baseline model [-9,9] window*				
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)
N	155,587	145,885	85,302	39,325
(*B*) *Post Traumatic Stress Disorder (PTSD) and the Common trend assumption [-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)
N	292,821	275,090	165,434	73,987
(*C*) *Conditional random assignment assumption/a different identification strategy [-9,9] window*				
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)
N	69,346	65,211	36,922	15,932
(*D*) *Baseline model - spatial spillovers - 5km radius [-9,9] window*				
Nazi raid				
in utero (β_1^a)				
Layoff (β_2^a)				
Layoff × Nazi raid				
in utero (β_3^a)				
N				
(*E*) *Flexible province trends [-9,9] window*				
Nazi raid	-0.0240***	-0.0241***	-0.0131	-0.0566***
in utero (β_1^a)	(0.0085)	(0.0087)	(0.0133)	(0.0215)
Layoff (β_2^a)	-0.3352***	-0.3226***	-0.3319***	-0.3173***
	(0.0140)	(0.0120)	(0.0165)	(0.0236)
Layoff × Nazi raid	-0.0794*	-0.0993**	-0.1469**	-0.0503
in utero (β_3^a)	(0.0474)	(0.0492)	(0.0623)	(0.0721)
N	155,587	145,885	85,302	39,325
(*F*) *Changes in the length of gestation [-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)
N	147,806	138,618	80,851	37,243
(*G*) *Changes in the length of gestation [-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)
N	139,311	130,812	76,002	34,939
(*H*) *Using only information from the first 6 months [-9,6] window*				
Nazi raid	-0.0221***	-0.0240***	-0.0293*	-0.0522***
in utero	(0.0088)	(0.0094)	(0.0195)	(0.0201)
Layoff	-0.3371***	-0.3121***	-0.3387***	-0.2928***
	(0.0131)	(0.0119)	(0.0130)	(0.0236)
Layoff × Nazi raid	-0.0976*	-0.1002*	-0.1301*	-0.0493
in utero	(0.0589)	(0.0626)	(0.0765)	(0.0813)
N	130,720	122,832	73,204	31,386
(*I*) *Changing the municipality size cut-off - municipalities under 500k residents [-9,9] window*				
Nazi raid	-0.0244***	-0.0295***	-0.0199	-0.0688***
in utero	(0.0085)	(0.0088)	(0.0130)	(0.0206)
Layoff	-0.3351***	-0.3251***	-0.3333***	-0.3269***
	(0.0138)	(0.0117)	(0.0160)	(0.0240)
Layoff × Nazi raid	-0.1041**	-0.1195**	-0.1829***	-0.0780
in utero	(0.0496)	(0.0509)	(0.0605)	(0.0709)
N	163,426	153,060	89,791	41,543
(*J*) *Changing the municipality size cut-off - municipalities no cap on size [-9,9] window*				
Nazi raid	-0.0152*	-0.0236***	-0.0115	-0.0589***
in utero	(0.0082)	(0.0081)	(0.0121)	(0.0191)
Layoff	-0.3369***	-0.3220***	-0.3389***	-0.3149***
	(0.0134)	(0.0115)	(0.0160)	(0.0253)
Layoff × Nazi raid	-0.0369	-0.0925**	-0.1538**	-0.0663
in utero	(0.0595)	(0.0418)	(0.0654)	(0.0553)
N	177,292	165,623	98,673	46,637

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

Notes: Unless otherwise specified, all estimates are based on municipalities with fewer then 200,000 residents, are obtained for age specific subsamples (45 to 60), include year × month fixed effects, municipality fixed effects as well as region specific time trends. Robust standard errors (in parentheses) are clustered at the municipality level.

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A Appendix: Examining the random assignment of Nazi raids

In the main model the parameter of interest (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. This requires an assumption of (conditional on municipality fixed effects) random assignment of the Nazi raids across municipalities. This appendix aims to support evidence in favor of this assumption. Before we proceed it is good to note that we also estimated models that are subject to the weaker assumption that the exact timing of the Nazi raid is not predictable, i.e. analyses based omn treated municipalities only (see section 4 for a discussion about this).

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 Caserta (October 1943) and Monte Cassino (December 1943) (see section 2, 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 A1 that depicts the monthly distribution of the Nazi raids over the country for the period September - May 1944. Figure A2 aggregates these raids over all months. Figure A1 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.

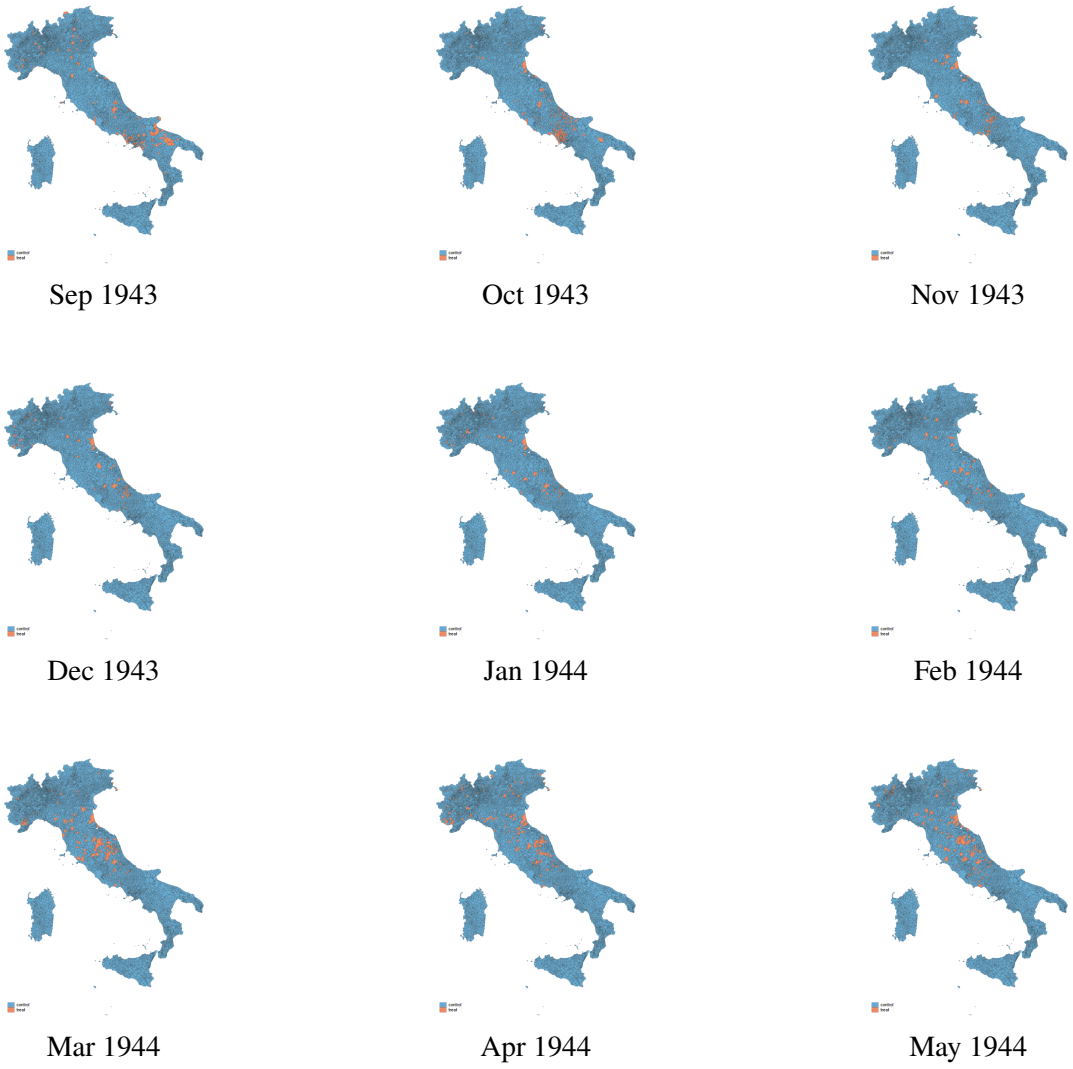
In 1 of section 4 we control for time invariant differences between treated and control municipalities. This, however, still leaves some room for structural differences between municipalities that vary over time. In order to address this we estimated a model that relates whether a municipality experienced a Nazi raid in the period September 1943– May 1944 to a range of municipality characteristics obtained from the 2011 census[40] and province $\times$ year $\times$ month fixed effects. The 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 A1. The table shows that apart from population size there are no associations with other characteristics. We next examine whether the impact of the regressors change over time. To this end we also estimated the model for

[40]There is no information for the WWII years

each month over the period September 1943 – May 1944 and tested for time invariant effects of the regressors using an F-test. [41]. For each covariate we report the p-values of the F-test in the last column of Table A1. The p-values of the F-test indicate that the null of time invariant structural differences between treated and control municipalities can not be rejected for any of the explanatory variables.

Figure A1: Evolution of Nazi raids (Sept 1943 - May 1944)



[41]More specifically, we do a full interaction of all the characteristics with month dummies

Figure A2: Placement of Nazi raids



Notes: The dark spots on the map indicate municipalities with a registered Nazi raid in the period September 1943 and May 1944.

Table A1: 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 obtained from the 2011 census. Nazi event is an indicator dummy for a municipality that had 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 × month fixed effects. All regressions control for province fixed effects.

B Appendix: Additional evidence

B.1 Additional descriptive statistics

Table B1: 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 resistance fighters (%)	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 resistance fighters (%)	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 resistance fighters (%)	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 resistance fighters (%)	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 resistance fighters (%)	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 Nazi raids between Sept 1943–May 1945.

Table B2: Descriptive statistics: Nazi raids between Sept 1943 - May 1944 in municipalities with fewer than 200,000 residents

Date	N. of raids	% of resistance victims	% of male victims
Sept 1943	246	1.9	86.7
Oct 1943	358	1.3	88.0
Nov 1943	132	5.9	84.1
Dec 1943	131	12.7	83.3
Jan 1944	97	15.5	89.7
Feb 1944	78	14.5	88.6
Mar 1944	170	35.4	94.1
Apr 1944	201	44.6	90.7
May 1944	190	36.1	91.1

Notes: The numbers refer to a sample of 1,603 Nazi raids between Sept 1943 – May 1944 in Italian municipalities with less than 200,000 residents.

B.2 Layoffs assignment

Table B3: Layoff event random assignment check.

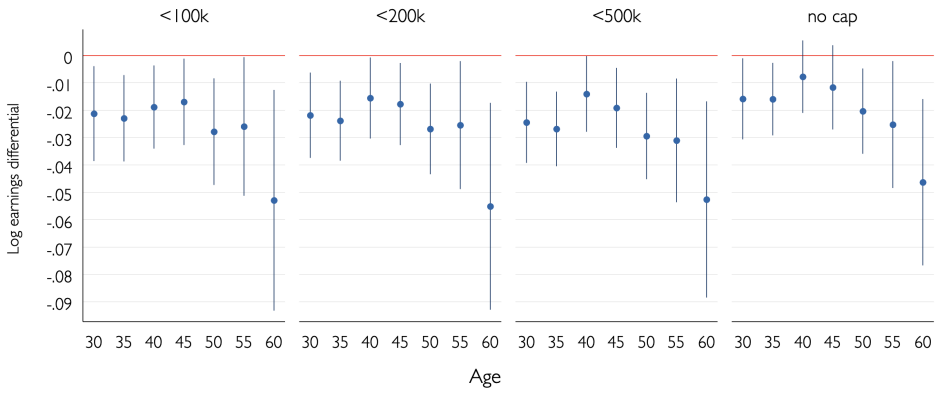
	layoff at anytime	layoff at 45	layoff at 50	layoff at 55	layoff at 60
Nazi raid	-0.0013	0.0001	-0.0018	-0.0006	0.0017
in utero	(0.0027)	(0.0018)	(0.0024)	(0.0023)	(0.0026)
WWII casualties (SD)	0.0000	-0.0004	-0.0009	0.0001	0.0001
in utero	(0.0013)	(0.0009)	(0.0011)	(0.0009)	(0.0011)
R^2	0.0426	0.0454	0.0513	0.0528	0.0666
N	283,975	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 (Jan 1943–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. All regressions include year × month and municipality fixed effects as well as region specific time trends. Robust standard errors (in parentheses) are clustered at the municipality level.

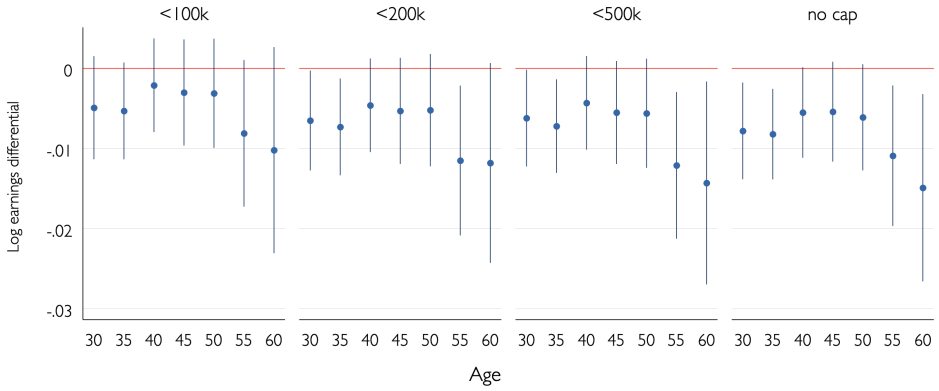
B.3 Sensitivity to municipality size

Figure B1: Sensitivity to municipality size - the effect of a Nazi raid on age specific log earnings



Notes: Effects on earnings of Nazi exposure in utero among individuals born in the [-9,9] month window around the Armistice (coefficients and 95% confidence intervals). Subsamples include municipalities with progressively increasing resident population size (under 100,000, under 200,000, under 500,000, no cap).

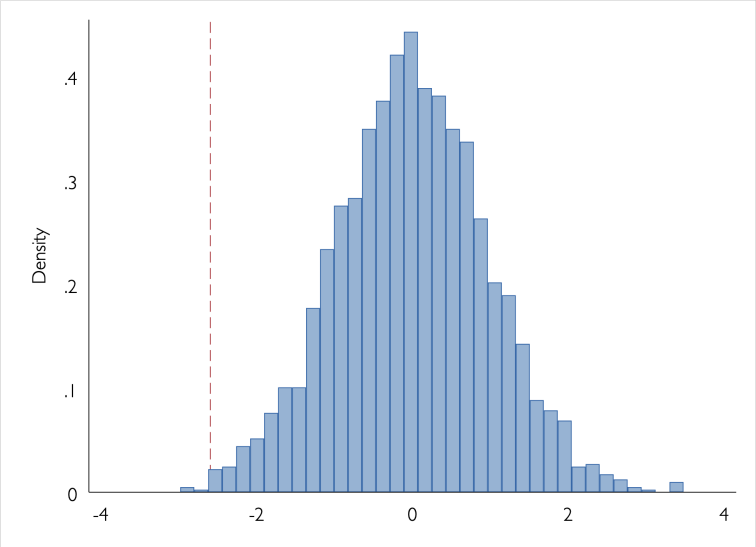
Figure B2: Sensitivity to municipality size - the general WWII effects (WWII casualties) on age specific log earnings



Notes: Effects on earnings of WWII exposure in utero among individuals born in the [-9,9] month window around the Armistice (coefficients and 95% confidence intervals). Subsamples include municipalities with progressively increasing resident population size (under 100,000, under 200,000, under 500,000, no cap).

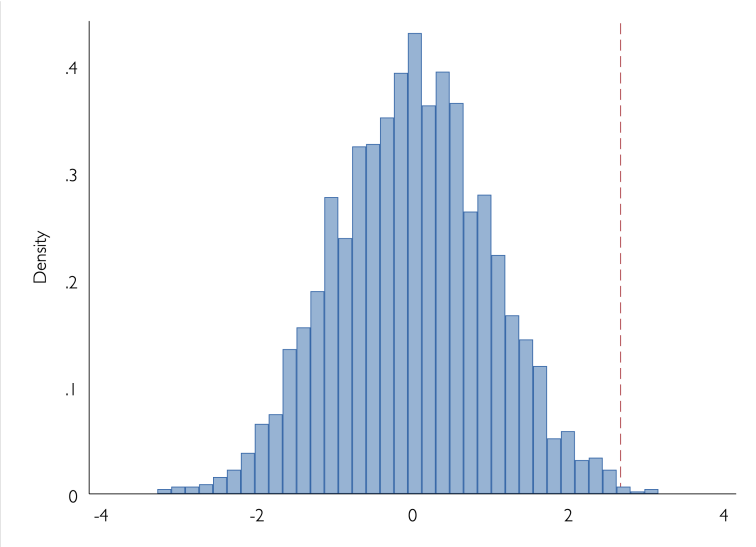
B.4 Falsification tests

Figure B3: 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 B4: 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.5 Selectivity checks

Table B4: 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 observations 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).

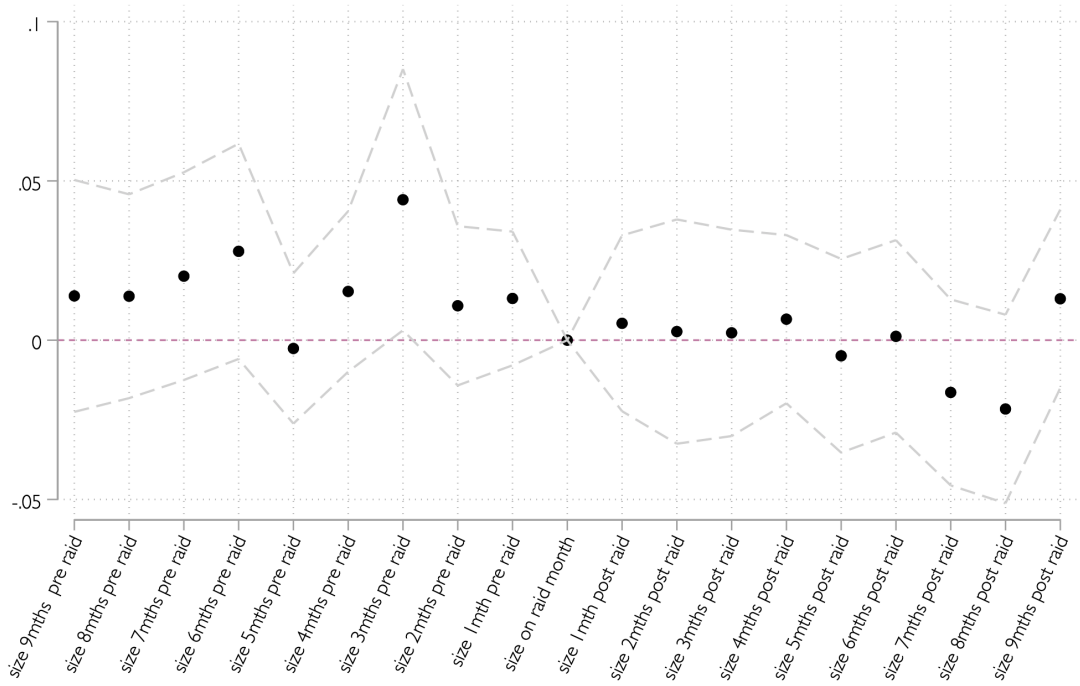
Table B5: Effect of Nazi raids on municipality level cohort size

	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: Results from a regression of municipality monthly cohort sizes (as share in total municipality population), derived from the INPS data, concerning individuals born between Jan 1943 and May 1944. All regressions include the number of war casualties, year × month and municipality fixed effects as well as region specific time trends. Robust standard errors are clustered at the municipality level.

Figure B5: Monthly cohort sizes observed at the age of 30



Notes: The figure shows an event study coefficient estimates from a regression of municipality monthly cohort sizes (z-scores), derived from the INPS data, concerning individuals born between Jan 1943 and May 1944. All regressions include year × month and municipality fixed effects. The confidence intervals correspond to a threshold of 95%.

C 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).[42] 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 3) 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 C1 (expenditures) and Table C2 (logarithms of expenditures). The tables 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 coefficients imply a 17 percentage change in expenditures. Table C3 presents the results for the extensive margin (whether or not an individual had a positive expenditure). The table shows that those who were exposed to prenatal stress are about 3.6 percentage points more likely to acquire any drug for treating diseases of the nervous system and mental disorders. These findings are 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

[42]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.

and dementia (Checkley, 1996; Heffelfinger and Newcomer, 2001; Selten et al., 1999). There are no effects on expenditure related to cardiovascular diseases and diabetes (column 2), diseases that typically show up in famine studies (Van den Berg and Lindeboom, 2018). Important for our study, these findings give credit to our interpretation of the effect of the Nazi raids as being primarily a stress effect.

Table C1: 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 refers to individuals born in the 9 month window around the Armistice (Jan 1943–May 1944) and corresponds to patients managed by 468 GPs between 2004 and 2010. The expenditure refers to annual outpatient drug expenditures expressed in euros. All regressions control for WWII intensity and include year × month, municipality, GP, age and wave fixed effects as well as region specific time trends. Robust standard errors are clustered at the municipality level.

Table C2: Effect of prenatal exposure to Nazi raids on health expenditure types (in logs)

	(1) Neuro/ Mental.	(2) Cardio/ Diabetic	(3) Respir. syst.	(4) Hormone syst.	(5) Neoplasms	(6) Skin	(7) Musculo/ skeletal
Nazi violence	.1737803**	.0049712	.0075452	-.0068495	-.0140483	-.0265167	-.0982221
	.0812961	.0772294	.019095	.0325375	.0247252	.0286837	.068499
WWII casualties (SD)	-.0311454	-.0156929	.0004989	-.0160935	-.0195766*	-.0100156	-.0063346
in utero	.0232688	.0257558	.0049053	.0108522	.0112903	.0092541	.0266152
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 refers to individuals born in the 9 month window around the Armistice (Jan 1943–May 1944) and corresponds to patients managed by 468 GPs between 2004 and 2010. The expenditure refers to annual outpatient (log) drug expenditures expressed in euros. All regressions control for WWII intensity and include year × month, municipality, GP, age and wave fixed effects as well as region specific time trends. Robust standard errors are clustered at the municipality level.

Table C3: Effect of prenatal exposure to Nazi raids on probability of consuming various drug 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 refers to individuals born in the 9 month window around the Armistice (Jan 1943–May 1944) and corresponds to patients managed by 468 GPs between 2004 and 2010. The dependent variable is an indicator for positive annual outpatient drug expenditures. All regressions control for WWII intensity and include year × month, municipality, GP, age and wave fixed effects as well as region specific time trends. Robust standard errors are clustered at the municipality level.

D ONLINE Appendix: Sensitivity checks: Full tables

D1 Exploiting time variation (using only treated municipalities)

Table D1: Age specific earnings, treated in utero versus treated after birth (only treated 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) 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 year × month fixed effects, municipality fixed effects as well as region specific time trends. Robust standard errors (in parentheses) are clustered at the municipality level.

Table D2: Age specific blue collar status, treated in utero versus treated after birth (only treated 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) 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 year × month fixed effects, municipality fixed effects as well as region specific time trends. Robust standard errors (in parentheses) are clustered at the municipality level.

Table D3: Effect of layoff on wages, treated in utero versus treated after birth (only treated 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) 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 year × month fixed effects, municipality fixed effects as well as region specific time trends. Robust standard errors (in parentheses) are clustered at the municipality level.

D2 Differential effects for exposure in utero, in the first year of life and in the second year of life

Table D4: Differential effects of exposure in utero, in the first year of life, and in the second year of life: (log) 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), while the bottom row of Panel A and Panel B refer to individuals born in the window [-24,9] month around the Armistice (Sept 1941 – May 1944), the columns refer to separate outcomes. All regressions include year × month and municipality fixed effects as well as region specific time trends. Standard errors (in parentheses) are clustered at the municipality level.

Table D5: Differential effects of exposure in utero, in the first year of life, and in the second year of life: 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 (Jan 1943–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 separate outcomes. All regressions include year × month and municipality fixed effects as well as region specific time trends. Robust standard errors (in parentheses) are clustered at the municipality level.

Table D6: Differential effects of exposure in utero, in the first year of life, and in the second year of life: Mass layoff effects on (log) earnings

	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 (Jan 1943–May 1944), while the bottom row of Panel A and Panel B refer to individuals born in the window [-24,9] month around the Armistice (Sept 1941 – May 1944), the columns refer to separate outcomes. All regressions include year × month and municipality fixed effects as well as region specific time trends. Robust standard errors (in parentheses) are clustered at the municipality level.

D3 Using flexible province × year × month fixed effects

Table D7: The effect of in-utero exposure to a Nazi raid: Age specific earnings - province × year × month 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 in utero	-0.0263***	-0.0224***	-0.0150*	-0.0187**	-0.0250***	-0.0199	-0.0672***
	(0.0085)	(0.0078)	(0.0082)	(0.0084)	(0.0088)	(0.0130)	(0.0203)
R^2	0.1597	0.1513	0.1480	0.1528	0.1451	0.1580	0.2075
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
Prov 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. All regressions include year × month fixed effects, municipality fixed effects as well as province × year × month fixed effects. Robust standard errors (in parentheses) are clustered at the municipality level.

Table D8: The effect of in-utero exposure to a Nazi raid: Age specific blue collar status - province × year × month 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 in utero	0.0202***	0.0004	0.0135**	0.0132*	0.0117*	0.0195**	0.0399***
	(0.0059)	(0.0070)	(0.0066)	(0.0068)	(0.0070)	(0.0079)	(0.0119)
R^2	0.0973	0.1051	0.1127	0.1203	0.1264	0.1817	0.2317
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
Prov 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. All regressions include year × month fixed effects, municipality fixed effects as well as province × year × month fixed effects. Robust standard errors (in parentheses) are clustered at the municipality level.

Table D9: The effect of in utero exposure to a Nazi raid: Effect of mass layoff on earnings - province × year × month fixed effects

	age 45	age 50	age 55	age 60
Nazi raid	-0.0240***	-0.0241***	-0.0131	-0.0566***
in utero (β_1^a)	(0.0085)	(0.0087)	(0.0133)	(0.0215)
Layoff (β_2^a)	-0.3352***	-0.3226***	-0.3319***	-0.3173***
	(0.0140)	(0.0120)	(0.0165)	(0.0236)
Layoff × Nazi raid	-0.0794*	-0.0993**	-0.1469**	-0.0503
in utero (β_3^a)	(0.0474)	(0.0492)	(0.0623)	(0.0721)
R^2	0.1581	0.1533	0.1857	0.2393
N	155,587	145,885	85,302	39,325
Time FEs	YES	YES	YES	YES
Municipality FEs	YES	YES	YES	YES
Prov 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. All regressions include year × month fixed effects, municipality fixed effects as well as province × year × month fixed effects. Robust standard errors (in parentheses) are clustered at the municipality level.

D4 Using 6-month exposure window

Table D10: Age specific earnings - exposure limited to 6 months after Sept 1943

	Earnings at 30	Earnings at 35	Earnings at 40	Earnings at 45	Earnings at 50	Earnings at 55	Earnings at 60
Nazi raid	-0.0228**	-0.0264***	-0.0211**	-0.0221**	-0.0276**	-0.0391***	-0.0692***
in utero	(0.0102)	(0.0096)	(0.0097)	(0.0095)	(0.0107)	(0.0140)	(0.0234)
WWII casualties (SD)	-0.0091**	-0.0079*	-0.0055	-0.0061	-0.0103**	-0.0164**	-0.0061
in utero	(0.0042)	(0.0041)	(0.0041)	(0.0045)	(0.0047)	(0.0064)	(0.0085)
R^2	0.1558	0.1487	0.1453	0.1501	0.1417	0.1529	0.1845
N	175,883	172,306	155,884	141,936	131,898	83,162	39,307
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 to 6 months post Armistice (Jan 1943 – Feb 1944) in municipalities with fewer then 200,000 residents. All regressions include year × month fixed effects, municipality fixed effects as well as region specific time trends. Robust standard errors (in parentheses) are clustered at the municipality level.

Table D11: Age specific blue collar status - exposure limited to 6 months after Sept 1943

	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	0.0214***	0.0053	0.0189**	0.0141*	0.0155**	0.0229***	0.0308**
in utero	(0.0065)	(0.0075)	(0.0074)	(0.0073)	(0.0077)	(0.0087)	(0.0129)
WWII casualties (SD)	0.0055**	0.0058**	0.0088***	0.0069**	0.0072**	0.0102**	-0.0014
in utero	(0.0027)	(0.0026)	(0.0030)	(0.0032)	(0.0034)	(0.0044)	(0.0052)
R^2	0.0959	0.0986	0.1094	0.1168	0.1226	0.1746	0.2097
N	175,883	172,306	155,884	141,936	131,898	83,162	39,307
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 to 6 months post Armistice (Jan 1943 – Feb 1944) in municipalities with fewer then 200,000 residents. All regressions include year × month fixed effects, municipality fixed effects as well as region specific time trends. Robust standard errors (in parentheses) are clustered at the municipality level.

Table D12: The effect of in utero exposure to a Nazi raid: Effect of mass layoff on earnings - exposure limited to 6 months after Sept 1943

	age 45	age 50	age 55	age 60
Nazi raid	-0.0221***	-0.0240***	-0.0293*	-0.0522***
in utero (β_1^a)	(0.0088)	(0.0094)	(0.0195)	(0.0201)
Layoff (β_2^a)	-0.3371***	-0.3121***	-0.3387***	-0.2928***
	(0.0131)	(0.0119)	(0.0130)	(0.0236)
Layoff × Nazi raid	-0.0976*	-0.1002*	-0.1301*	-0.0493
in utero (β_3^a)	(0.0589)	(0.0626)	(0.0765)	(0.0813)
R^2	0.0431	0.0881	0.0935	0.1125
N	130,720	122,832	73,204	31,386
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. All regressions include year × month fixed effects, municipality fixed effects as well as region specific time trends. Robust standard errors (in parentheses) are clustered at the municipality level.

D5 Shorter gestation

Table D13: 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/April/March 1944) in municipalities with fewer then 200,000 residents. All regressions include year × month fixed effects, municipality fixed effects as well as region specific time trends. Robust standard errors (in parentheses) are clustered at the municipality level.

Table D14: 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/April/March 1944) in municipalities with fewer then 200,000 residents. All regressions include year × month fixed effects, municipality fixed effects as well as region specific time trends. Robust standard errors (in parentheses) are clustered at the municipality level.

Table D15: 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/April/March 1944) in municipalities with fewer then 200,000 residents. All regressions include year × month fixed effects, municipality fixed effects as well as region specific time trends. Robust standard errors (in parentheses) are clustered at the municipality level.