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 event experienced by pregnant women on the 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 on male private sector workers to information about Nazi raids and war casualties. Our results suggest that prenatal exposure to traumatic events affects offspring earnings throughout the working career and in retirement. The lower earnings are due to lower educational attainment, the type of jobs held, and interruptions in working careers due to unemployment. We further find that prenatal exposure exacerbates the adverse effects of later-life job loss on earnings. We use a medical database on health expenditures to interpret the effect estimates. The prenatally exposed have higher medical expenditures on diseases of the nervous system and mental disorders, indicating that stress is likely to be an important factor driving our findings. (JEL: J24, I15)

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Teaching Slides
A set of Teaching Slides to accompany this article is available online as Supplementary Data.

1. Introduction

Recent estimates suggest that, on average, between 50% and 85% of the developed country population experience a potentially traumatic life event (PTE) during their lifetime, such as being involved in a severe accident, losing a partner, being a victim of a crime or witnessing acts of extreme violence or terror (Kessler and Wang 2008; de Vries and Olff 2009; Benjet et al. 2016). PTEs pose a significant threat to a person's physical and/or psychological well-being, but adverse effects may also extend to the next generation. Previous work (Kuzawa and Quinn 2009; Almond and Currie 2012; Black, Devereux, and Salvanes 2016; Persson and Rossin-Slater 2018) has demonstrated that a mother's exposure to traumatic experiences during pregnancy may compromise the health and human capital of her children. Until now, we had minimal knowledge about the effect of traumatic and stressful experiences on the 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 job loss later in life.

To this end, we exploit plausibly exogenous events in Italy that followed after the "Armistice" in WWII on September 8, 1943, when Italy ceased hostilities against the allied forces. In response to this act, the Germans occupied the country and performed violent raids to spread fear and terror. The raids were intentionally unpredictable and idiosyncratically distributed in time and space. Although there were relatively few casualties and the raids lasted only a few days, they were characterized by intense violence that caused great stress and trauma to those who witnessed it.

This unique quasi-experimental setting allows us to analyze differences in outcomes for prenatally exposed cohorts to otherwise similar cohorts, which were never exposed or exposed after birth. We exploit the unexpected outbreak of the raids and the spatial (municipalities) and temporal (months) variation after controlling for the intensity of the war and local time-invariant and time-varying characteristics. We

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thus identify the average causal effect of prenatal exposure to a PTE over and above other adversities related to war.

We build a unique dataset combining three primary data sources. The "Atlas of Nazi and Fascist massacres" lists all raids that 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 between 1974 and 2018. We focus on male cohorts born around September 8, 1943. The data contain detailed working histories and information on wage earnings, occupation, employment status, educational attainment, pensions, and disability, and unemployment benefits. The third source of information is the archive of WWII conflicts in Italy from Statistics Italy (ISTAT 1957). This dataset includes granular information on the number of victims of armed conflicts between Allied Forces and Germans by province and month.

Results of generalized Difference-in-Differences (DiD) models show that prenatal exposure to a PTE reduces earnings by about 2% at the start of the labor market career. The earnings penalty gradually increases along the career to about 6% at retirement. Moreover, we find that the lower earnings can be attributed to lower educational attainment and occupation sorting. Those prenatally exposed to a Nazi raid (hereafter often referred to as the "exposed" or "prenatally exposed") are more likely to sort into low-skilled blue-collar jobs. We also find that the exposed are more likely to experience unemployment in their career, causing the earnings penalty to increase as people age. It could also be that the earnings penalty increases because the exposed suffer more from job loss later in life. To shed light on this hypothesis, we exploit the matched worker-firm information in the INPS data to examine the effect of job loss due to a mass layoff at the firm.[1] Estimates of a triple DiD model show that later-life job loss disproportionally affects the exposed: job loss impacts next year's earnings by 31%–34% for all workers and up to 47% for the exposed. Moreover, and in line with Sullivan and von Wachter (2009), we find that mass layoff episodes are likely to increase mortality, but this effect is independent of in-utero exposure to a Nazi raid.

While the effects of prenatal exposure to a Nazi raid are substantial, no effects are found for exposure in the first and second years of life. This is important as the raids may have caused lasting psychological trauma Post-Traumatic Stress Disorder (PTSD), affecting parental health (behaviors) and parenting skills. Also, the raids may have led to property destruction and persistent income losses. Should these alternative mechanisms be relevant, they would harm both the prenatally exposed and those exposed shortly after birth. Thus, our results are driven by in-utero exposure to a raid and not by maternal health, maternal health behaviors and/or reduced family income.

A series of robustness checks confirm our results. We use more flexible specifications, vary the definition of our outcome variables, use different samples, use a

1. Mass layoffs are plausibly more exogenous than regular job separations. In Section 5.2 we show that the allocation of mass layoffs across the workers in our sample is independent of prenatal exposure to a Nazi raid.

different identification strategy, and perform falsification tests. Additionally, we show that selective fertility, mobility, and mortality do not bias our estimates.

As a further step in our analysis, we argue that maternal stress triggered by the traumatic event 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 maternal Glucocorticoids (such as Cortisol) and that this may have a profound impact on the neurological development of the fetus, leading to cognitive, emotional, and mental problems as well as increased stress vulnerability of the offspring (see Marmot et al. 1991; Cotter and Pariante 2002; Van den Bergh et al. 2005; Weinstock 2005; Boersma and Tamashiro 2014, and the literature reviewed in these papers). Also, recent economic literature has supported these claims (Black, Devereux, and Salvanes 2016; Quintana-Domeque and Rodenas-Serrano 2017; Persson and Rossin-Slater 2018), showing causal effects of maternal stress during pregnancy on the birth outcomes, educational attainment, and mental health of offspring. Aizer, Stroud, and Buka (2016) measure actual 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.

In support of our interpretation and in line with the literature cited above, we find lower educational attainment among the exposed. A mediation analysis shows that a large share (42%) of the total treatment effect on wage earnings can be explained by the impact of the treatment on educational attainment. Additionally, we use a medical database on health expenditures and find that prenatal exposure to a Nazi raid has significant and sizeable effects on health expenditures only for diseases of the nervous system and mental disorders, conditions typically associated with prenatal stress exposure. We find no effect on cardiovascular conditions and diabetes, conditions typically associated with prenatal exposure to hunger. However, it must be noted that other mechanisms can never be ruled out, even if one has information on stress hormone (cortisol) levels. We turn to the interpretation of our findings in Section 7.

Our study concerns a historical event that could be described as extreme and rare. Unfortunately, extreme violence against civilians in wars and civil conflicts are still very relevant today. According to UNHCR, by the end of 2021, propelled by new waves of conflict and violence the number of displaced people worldwide rose to 90 million. In 2022, the war in Ukraine displaced 8 million within the country and forced around 6 million to leave the nation. Our study shows that such traumatic experiences of pregnant women scar the next generation's educational and labor outcomes. Besides, as Akbulut-Yuksel, Tekil, and Turan (2022) show, the trauma, fear, and anguish children are experiencing are likely to place an enormous strain on their mental health throughout their life. Traumatic experiences are not only limited to war situations but also hold for deprived neighborhoods, where crime, unemployment, and poverty rates are high. Such adversities affect families with poor qualifications and limited resources the most. Therefore, traumatic and stressful events may play an important role in the persistence of low socio-economic status across generations (Aizer, Stroud, and Buka 2016).

Our paper relates to earlier work, but there are some crucial differences. First, quite a few studies examine the long-term effects of WWII and associated disruptions on later life outcomes. Akbulut-Yuksel (2014) exploits region by cohort variation in the intensity of WWII destruction and finds that exposure to destruction has long-lasting detrimental effects on human capital formation, health, and labor outcomes of Germans who were at school age during WWII. Likewise Akbulut-Yuksel (2017), Akbulut-Yuksel, Tekil, and Turan (2022) find that those exposed to WWII destruction during the prenatal and early postnatal periods have higher BMIs, are more likely to be obese as adults, and have worse mental health. Kesternich et al. (2014) use recall information from the Survey of Health and Retirement in Europe (SHARE) and find that WWII experiences of dispossession, persecution, combat in local areas, and hunger significantly affect economic and health outcomes at older ages.

In our study, we examine the effect of a very local and precisely measured intervention (i.e. the raid) while at the same time carefully controlling for region-by-time variation in the intensity of the war. The raids mainly lasted one day, were unexpected, and varied widely across municipalities and time. As we focus on smaller municipalities, no one could escape witnessing the horrors of executions, physical abuse against harmless civilians, and destruction. In this way, we more precisely measure the causal effect of the event rather than an intention-to-treat effect, which most other war studies do. Moreover, the interpretation of effect estimates is facilitated as we measure the effect of a PTE over and above the general war effects. We also use a medical database on disease expenditures and alternative specifications to rule out some other potential explanations.

Second, most previous studies that looked at PTEs primarily looked at infant and childhood education and health outcomes (e.g. Camacho 2008; Quintana-Domeque and Rodenas-Serrano 2017; Bundervoet and Fransen 2018). To the best of our knowledge, only two other studies looked at the causal effect of in-utero exposure to a PTE on adult outcomes (Black, Devereux, and Salvanes 2016; Persson and Rossin-Slater 2018). These studies examine the effect of the death of a relative of pregnant mothers.

Persson and Rossin-Slater (2018) find that the offspring of mothers who experience a family rupture while pregnant, as opposed to mothers who undergo it shortly after giving birth, use more ADHD medications. For adults (aged 30), they find increases in the likelihood of consuming prescription drugs for anxiety and depression, evidence that corroborates our findings based on the medical database. Black, Devereux, and Salvanes (2016) compare children born to the same mother who experienced a parental death during one of the pregnancies. They found that parental death experienced in utero leads to negative effects on birth outcomes, but not 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 offspring's working career. Our outcomes include wage earnings from age 30 to age 60 and income after retirement. We examine the effect of exposure on educational attainment and selection into the type of job as possible mediators for the earnings penalty. We also study the heterogeneous impact on wage earnings of a further shock later in life. Jointly our findings indicate that early life traumatic experiences set in motion a chain of education and labor outcomes across the life cycle that ultimately lead to increased earnings penalties with age and lower pension benefits in retirement.

Third, our mass layoff analyses supplement the existing economic literature showing that workers displaced in a mass layoff experience significant long-term earnings losses, job instability, lower employment rates, and earlier retirement (see Ruhm 1991; Chan and Stevens 2001, and the literature cited in these papers). On the health side, Sullivan and von Wachter (2009) find substantial increases in mortality rates for male workers that persist up to 20 years after job displacement. Browning and Heinesen (2012) find for males 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, Nix, and Riukula (2022) show that workers born to less-well-off parents have lower earnings and higher unemployment than similar workers born to wealthier parents. We add to this literature by showing that the earnings penalty of job loss depends on prenatal conditions.

Our job displacement analyses also speak to a small but growing literature that empirically addresses the issue of dynamic complementarities (see, e.g. Almond and Mazumder 2013; Malamud, Pop-Eleches, and Urquiola 2016). Dynamic complementarities, 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. Indeed, this is what we find. 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.

2. Historical Background

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

Despite the start of WWII in September 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 its troops mostly outside the national territory, which was only modestly affected by war events. This period was marked by relatively few casualties, concentrated around strategic bombing targets, such as harbors, industrial sites, and key railways (Baldoli, Knapp, and Overy 2011; Baldoli and Knapp 2012).

The Armistice ceased hostilities between the Kingdom of Italy and the Allies and marked the beginning of German occupation and Italian resistance against fascism. The act was secretly signed on September 3, 1943 but was made public on the radio on September 8 at 18:30 Italian time.[2] The Italian campaign changed radically after the

2. 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 hours on Radio Algiers with a declaration from General

Armistice. In the days following the Armistice Italian servicemen were left without orders from their commands due to the German "Wehrmacht" disrupting Italian radio communication. The dismantling of the Italian military was immediately evident, as was the absence of a clear military strategy. There is abundant historical evidence of contradictory orders coming from higher-ranking officers.[3] The civilian population also had no information about the evolution of relations with Germany.

Given the unforeseen circumstances in the first few days of September 1943, the events that followed were very difficult to predict, ruling out strategic migration responses by civilians. According to Strazza (2010), information about the arrival of military troops frequently did not spread across neighboring villages. Moreover, there was no national evacuation plan (Baldoli, Knapp, and Overy 2011). Moving across provinces was extremely difficult since railroads and main transportation networks had been destroyed by tactical bombings by the Allied forces (Baldoli and Knapp 2012).

After 8 September, 1943, Italy was exposed to two major types of adversities that affected civilian life: (i) general armed conflicts between the Allied and German forces, and (ii) violent Nazi (and fascist) raids in towns and villages. Regarding the general armed conflicts, the post-Armistice period was characterized by military battles, ranging from quick victories and front-line movements entailing relatively few casualties to long stalemates associated with a sizeable number of fatalities. The underground resistance was not coordinated at the national level. Especially immediately after the Armistice, the nascent movement was formed of independent operating groups led by previously outlawed political parties or by former officers of the Royal Italian army. The first major act of resistance against the German occupation was in the city of Naples, liberated by a chaotic popular rebellion on September 28–30, 1943. Figure 1 provides a detailed map of the WWII events. The allied forces entered Italy in the South, 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.[4]

In the first months following the Armistice, the Raids were intentionally unpredictable and largely performed as retaliation for the surrender of the Italian population to the allied forces. The raids were part of a deliberate strategy to spread fear and terror and suppress any resistance and were often carried out in villages and towns without any apparent provocation or reason for suspicion (Holland 2008,

Dwight Eisenhower. Just one hour later, at 19:42, it was confirmed by a proclamation from Marshal Pietro Badoglio via the Italian public broadcasting network EIAR, (Zangrandi 1974). After the signing of the act, the Royal family and the prime minister fled from Rome on the morning of September 9.

3. For example, on September 10, 1943, in Piombino, a small German flotilla tried to enter the harbor of Piombino but was denied access by port authorities. Servicemen received two contrasting orders, one from the Italian coastal forces commanded by a former fascist "Gerarca" granting access, while the naval commander denied access to the port.

4. 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.

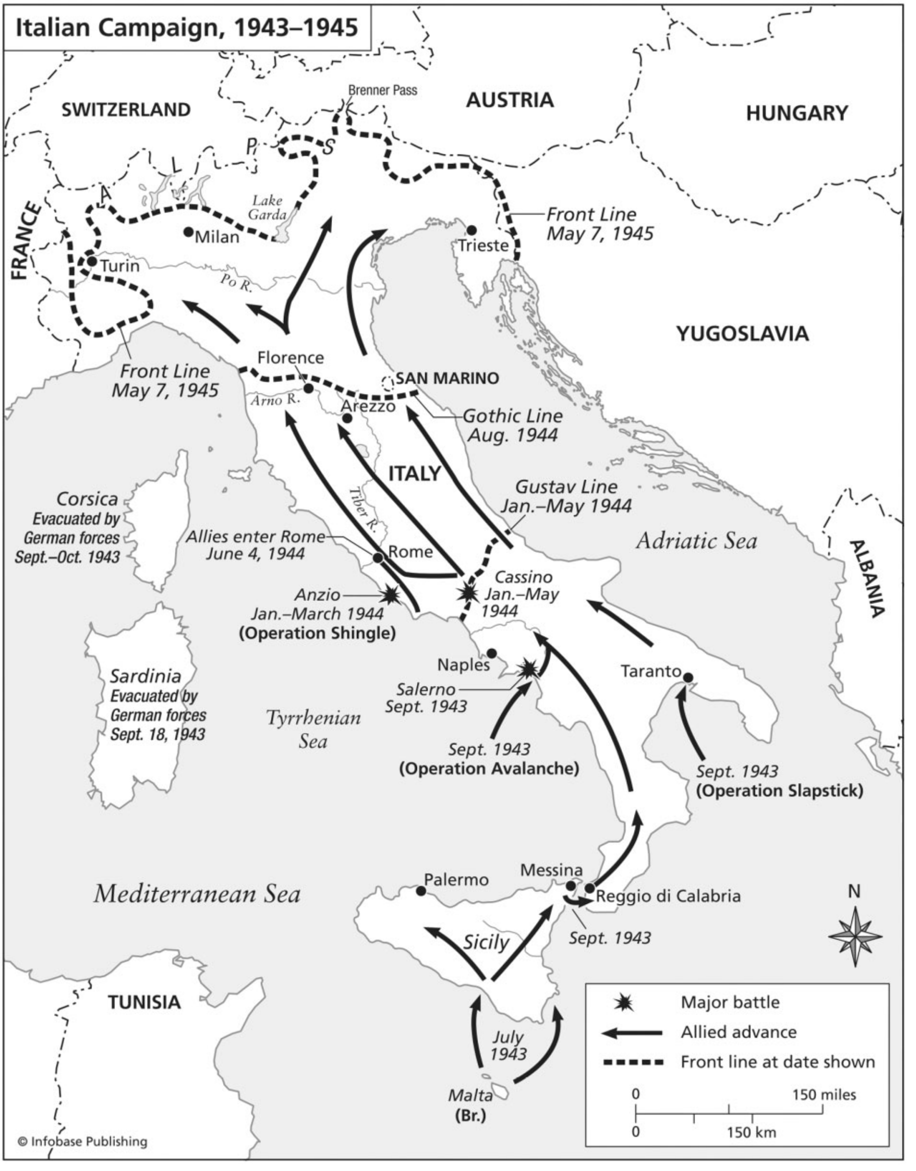

FIGURE 1. WWII fronts in Italy. Source: https://www.ww2history.org/war-in-europe/wwii-italy-and-the-story-of-1938-winner-of-the-tour-de-france-gino-bartali/.

Gentile 2020). While for some Nazi raids, there may have been specific targets, such as partisan/resistance fighters, most raids appear to have been carried out with little or no intelligence on the individuals being targeted (Pardini 1994).[5] The Nazi terror

5. It should be noted that only later, the resistance movement became more organized, and the raids involved more casualties of resistance fighters. See also Table B.2 of Online Appendix B. For this reason,

included physical violence, confiscating economic resources, rounding up civilians, and deporting them to labor camps. Although most raids involved relatively few casualties and lasted only a few days, the extreme acts and threats of violence and terror witnessed by the civilian population caused great fear and stress.

3. Data

We construct an individual-level dataset combining several administrative sources. Firstly, we collect historical information on (i) Nazi violence episodes and (ii) the number of deaths and missing persons in Italy during WWII. We refer to this data as "War data". Secondly, we use an administrative employer–employee matched dataset on the universe of Italian private sector workers. We refer to this data as "labor data".

3.1. War Data

3.1.1. Nazi Violence Data. The INSMLI (National Institute for the History of the Italian Liberation Movement) and the ANPI (National Association of Italian Partisans) created the "Atlas of Nazi and Fascist massacres", created a database from historical sources (documents, pictures, and videos) on all violent episodes perpetrated against civilians by the German army and its fascist allies in Italy between 1943 and 1945.[6] The episodes include information on the first murders in the South to withdrawal massacres committed in the North, just after the Liberation (April 25, 1945).

The database provides information on the number and type of victims by age and gender and the precise timing (day) and place (municipality).[7] The database counts more than 5,800 episodes including 20,000 victims across 2,200 municipalities. Figure 2 depicts the evolution of casualties due to raids by region over time. For reasons that will become clear in Section 3.3, we focus on raids in the first nine months following the Armistice (September 1943–May 1944) performed in smaller municipalities (less than 200,000 residents). This restricts the sample to 1,601 episodes, lasting on average 1.4 days with an average of 3.27 victims per episode (see Table 1).[8] Figure A.1 of Online Appendix A shows that in the nine months after the Armistice, the raids covered all Italian regions north of the battlefront. Table B.2 in Online Appendix B.1 reports the number of raids by month for the first nine months after the Armistice.

we only use Nazi raids taking place in the period of September 1943 to May 1944 (see Section 3.3). Of course, the randomness of the raids needs to be verified more formally. We turn to this in Section 6.2 and Online Appendix A.

6. 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.

7. This is the smallest administrative unit.

8. The summary statistics of the Nazi raids over the entire period are reported in Table B.1 of Online Appendix B.

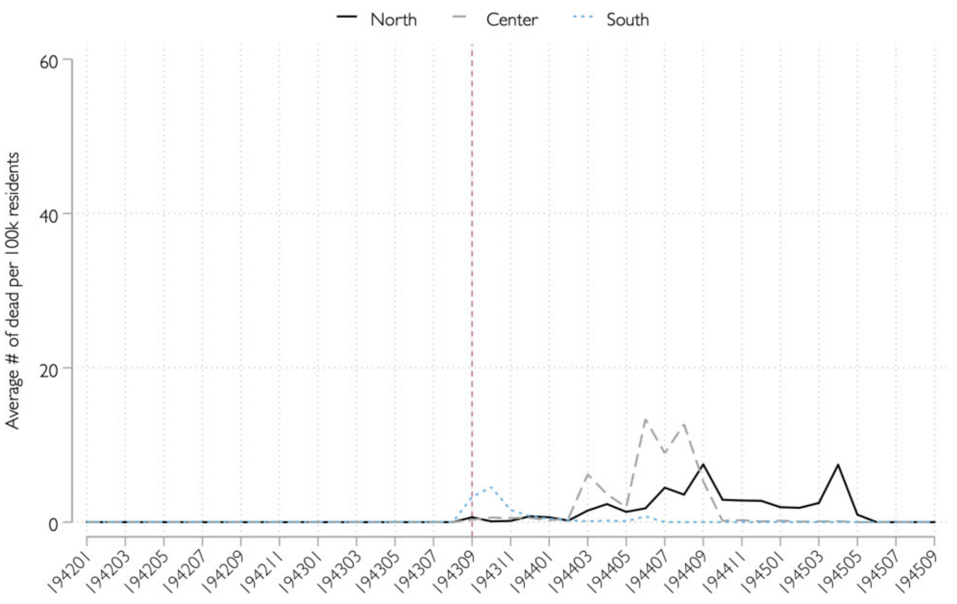

FIGURE 2. Average number of Nazi raid victims per 100,000 residents by region.

3.1.2. General Data on Deaths and Missing Persons during WWII in Italy. We constructed from the archive publication "Morti e dispersi per cause belliche negli anni 1940–1945" (The dead and the missing due to war causes between 1940 and 1945), of the Italian National Institute of Statistics (ISTAT 1957) a dataset including the number of casualties of armed conflicts by province and month.[9] Figure 3 shows how casualties by region evolved 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 INPS, the national social security and welfare institute in Italy, one of the largest administrative bodies at the European level.[10]

3.2.1. Matched Employer–Employee Data. INPS provides individual employment histories of the universe of private sector workers (excluding agriculture) in Italy from 1974 to 2018. The data include employee demographics, occupation, contract type, and

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

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

TABLE 1. Descriptive statistics on Nazi raids episodes between September 1943 and May 1944 in municipalities with fewer than 200,000 residents.

	Number of observations	Mean	Standard deviation	Min	Max	p1	p50	p99
Italy								
Length in days	1,601	1.40	2.67	1	54	1	1	14
Number of victims	1,601	3.28	7.36	0	130	1	1	29
Proportion of women (%)	1,601	9.42	25.89	0	100	0	0	100
Proportion of children (%)	1,601	1.91	11.53	0	100	0	0	80
Proportion of men (%)	1,601	87.73	29.71	0	100	0	100	100
Proportion of partisans (%)	1,601	17.22	36.47	0	100	0	0	100
Proportion of searches (%)	1,601	31.11	46.31	0	100	0	0	100
Proportion of retaliations (%)	1,601	10.74	30.98	0	100	0	0	100
Northwest								
Length in days	251	1.76	3.42	1	36	1	1	21
Number of victims	251	4.89	9.21	1	97	1	2	51
Proportion of women (%)	251	5.67	19.41	0	100	0	0	100
Proportion of children (%)	251	0.40	6.31	0	100	0	0	0
Proportion of men (%)	251	92.73	22.72	0	100	0	100	100
Proportion of partisans (%)	251	42.30	46.66	0	100	0	0	100
Proportion of searches (%)	251	51.39	50.08	0	100	0	100	100
Proportion of retaliations (%)	251	15.94	36.67	0	100	0	0	100
Northeast								
Length in days	183	1.66	4.62	1	54	1	1	29
Number of victims	183	3.39	10.36	1	130	1	1	32
Proportion of women (%)	183	7.37	25.08	0	100	0	0	100
Proportion of children (%)	183	0.68	7.53	0	100	0	0	19
Proportion of men (%)	183	90.86	27.89	0	100	0	100	100
Proportion of partisans (%)	183	34.63	45.87	0	100	0	0	100
Proportion of searches (%)	183	31.69	46.66	0	100	0	0	100
Proportion of retaliations (%)	183	17.49	38.09	0	100	0	0	100
Center								
Length in days	399	1.35	2.19	1	27	1	1	11
Number of victims	399	3.27	5.12	1	46	1	1	29
Proportion of women (%)	399	6.32	20.55	0	100	0	0	100
Proportion of children (%)	399	1.66	10.61	0	100	0	0	67
Proportion of men (%)	399	90.77	25.62	0	100	0	100	100
Proportion of partisans (%)	399	22.61	40.46	0	100	0	0	100
Proportion of searches (%)	399	45.86	49.89	0	100	0	0	100
Proportion of retaliations (%)	399	8.77	28.32	0	100	0	0	100
South								
Length in days	768	1.25	1.86	1	28	1	1	8
Number of victims	768	2.74	6.71	0	125	1	1	25

TABLE 1. Continued

	Number of observations	Mean	Standard deviation	Min	Max	p1	p50	p99
Proportion of women (%)	768	12.75	29.75	0	100	0	0	100
Proportion of children (%)	768	2.84	13.80	0	100	0	0	100
Proportion of men (%)	768	83.78	33.43	0	100	0	100	100
Proportion of partisans (%)	768	2.07	13.88	0	100	0	0	100
Proportion of searches (%)	768	16.67	37.29	0	100	0	0	100
Proportion of retaliations (%)	768	8.46	27.85	0	100	0	0	100

Notes: Nazi raids between September 1943 and May 1944 occurring in Italian municipalities with resident population under 200,000.

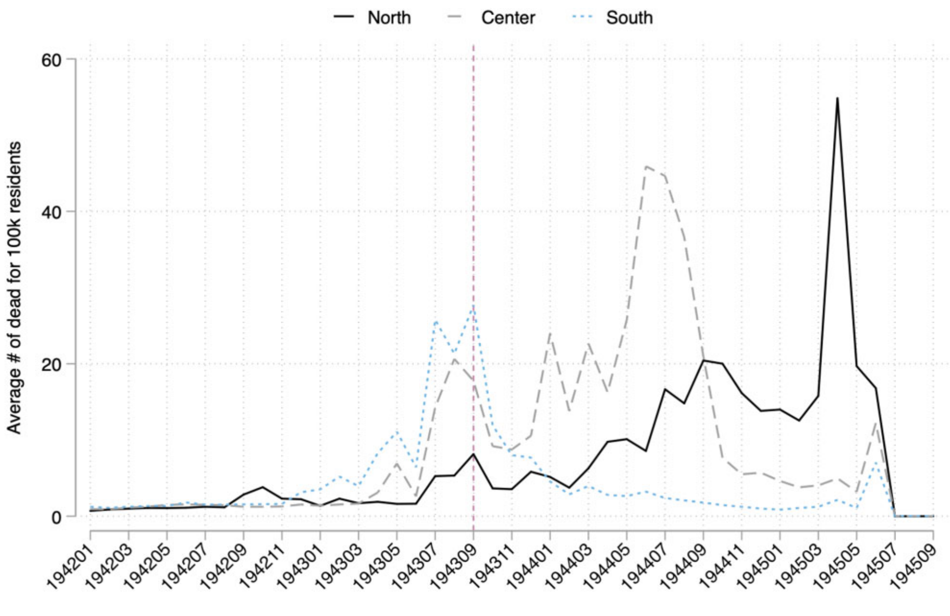

FIGURE 3. Average number of WWII victims per 100,000 residents by region.

earnings. 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 be due to employment in a non-private sector or exit from the labor market. This is a common feature of administrative employer–employee data.

Additionally, for all Italian residents, INPS collects information on the municipality and date of birth, date of death, and date of labor market entry. The data also include detailed information on pensions, Unemployment Insurance (UI), and Disability Insurance (DI) premiums and claims.

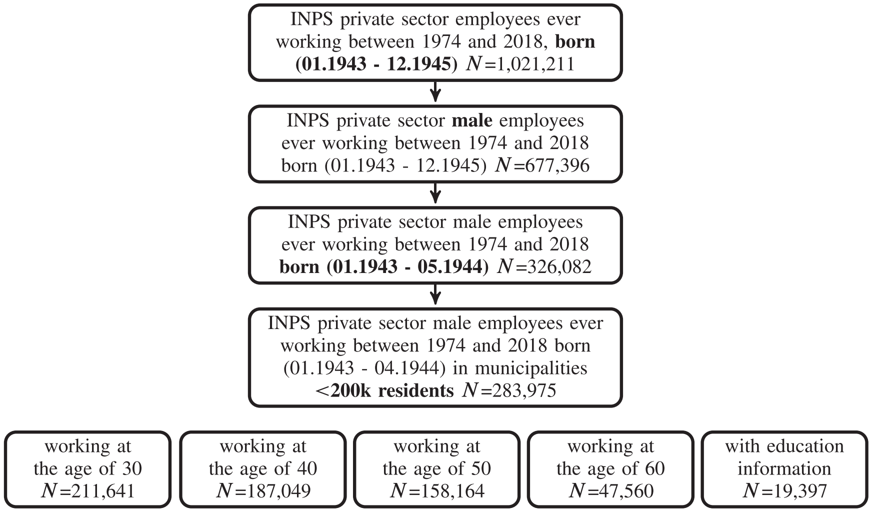

FIGURE 4. INPS data selection.

3.2.2. Pension and Benefits Data. The data on pensions are available from 1995 onward and include information on the age at which retirement benefits are collected, the type of pension, the number of years of contributions as well as the benefit amount. The UI and DI premiums and benefits information are available for all Italian workers, independent of the sector of work, and thus give information about interruptions in the labor market career. This information also allows us to address the external validity of our estimates based on private-sector workers. Specifically, we examine whether prenatal exposure to a raid is related to the sector of work. We find it is not. We refer to Section 6.6 for more detail.

3.2.3. Education Data. After 2010, employers were obliged to disclose to INPS information about educational attainment for all contracts that opened, changed, or closed. We observe educational attainment for 52% of the sample of workers who were still employed in 2010 (19,397 out of 37,471 workers; see below in Section 3.3). This subsample is representative of the full sample.[11]

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 graphically describes this selection process.

11. We perform a regression analysis of being present in the education sample on being prenatally exposed to a Nazi raid and find no evidence of a significant association. See Section 5.1 for details.

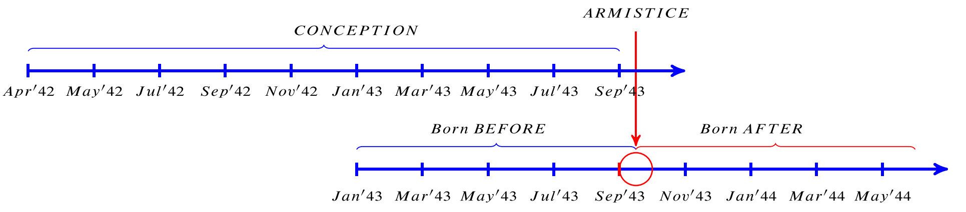

FIGURE 5. Cohort selection and timing of events.

We first select all employees who worked at any point between 1974 and 2018 and were born between January 1943 and May 1945 (in total 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 (677,396 individuals).[12] We next restrict our sample to individuals born in a nine-month window surrounding the date of the Armistice (i.e. born between January 1943 and May 1944) (326,082 individuals). Assuming a nine-month gestation period, this implies cohorts conceived between April 1942 and September 1943. Those born after September 8 were (potentially) prenatally exposed to a Nazi raid, while others (born in the nine months preceding September 8) were not. See Figure 5 for a schematic representation.

The short-time window rules out selective fertility and limits the potential impact of other behavioral responses that may occur after the Armistice as time progresses (see also Section 6.4). Yet, it does not rule out mortality selection in utero (stillbirths) and between birth and 1974, when we observe our outcomes for the first time. We return to selection issues in Section 6.6). Another potentially confounding factor is that the nature of the war was different in large cities. Raids in large cities generally involved more victims and nearby areas were more likely to know about this, blurring the distinction between the treated and controls. Besides, some large cities experienced other adverse WWII conditions such as bombings and nutritional shortages.[13] These other hardships may confound our estimates and their interpretation. We therefore further restrict our sample to individuals born in municipalities with less than two hundred thousand residents.[14]

This final selection reduces our sample to 283,975 males born in 7,507 municipalities. In this sample, we have 1,601 Nazi raids and 22,194 (7.8%) individuals prenatally exposed to a Nazi raid.

12. Exposure to stress early in life may also affect fertility behavior, which in itself may affect the labor force participation decisions of females and make the results difficult to interpret.

13. In particular, Rome faced severe bombings, deportations of Jews for extermination at Auschwitz, and mass roundups of non-Jewish males for forced labor. On March 24, 1944, the Nazis performed the Fosse Ardeatine massacre in Rome, where 335 civilians were killed. (Portelli 2003) provides a detailed account from witnesses.

14. We examine the sensitivity of our estimates to different cut-offs for the municipality size in Section 6.4.

3.4. Exposure Variables

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

We create an indicator for Nazi violence exposure, which equals 1 if the municipality of birth had at least one Nazi violence episode in the nine months preceding birth. Analogously we construct a measure for general war intensity, defined as the number of war victims in the birth province due to armed fights between Allied forces and the German troops in the nine months before birth. For ease of interpretation, we standardize this variable (mean zero and a standard deviation of one).

3.5. Outcome Variables

We create two annual labor outcomes: log wage earnings and an indicator for manual unskilled labor (blue-collar jobs).[15] We define our outcomes at different ages a ($a =$ 30, 35, 40, 45, 50, 55, and 60). To minimize the variance in earnings, we take for each a the average of the earnings at $(a - 1)$, a, and $(a + 1)$. For the sub-sample of workers with information on educational attainment, we also construct indicator variables for less than secondary and less than tertiary education.

For each individual, we construct binary indicators for mortality, unemployment, and disability benefit receipt and variables for the age at retirement and pension benefit.[16]

We also examine income and mortality effects of job loss at later ages (Section 5.2). For these analyses, we use the matched employer–employee data to identify episodes of job loss due to mass layoffs at the firm.[17] In line with the literature (Sullivan and von Wachter 2009), we define mass layoff episodes in firms with more than 25 workers who reduce their total workforce 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 blue-collar workers, and that this share declines with age. Figures 6 and 7 depict this graphically. Interestingly, the fall in earnings at around age 55 coincides with an increase in the share of lower-skilled blue-collar workers. At age 55, retirement

15. The blue-collar indicator is derived from a hierarchical categorical variable that has five values: (1) Manual; (2) Skilled non-manual; (3) Professional; (4) Managerial; and (5) Apprentice. Earnings are expressed in 2005 euros, adjusted for inflation using the CPI index.

16. The cohorts in our study have defined benefit pensions. The pension benefit amount is a function of work experience, age when the benefit is claimed, and average earnings over the last working years.

17. 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.

TABLE 2. Descriptive statistics for INPS men born between January 1943 and May 1944.

	Mean	Standard deviation	Number of observations
Nazi violence in utero	0.08	(0.27)	283,975
War victims in utero (# for 100 k province population)	57.08	(69.38)	283,975
First year of earnings	1,977	(6.61)	283,975
Last year of earnings	1,994	(9.77)	283,975
Number of years with positive earnings	16	(9.89)	283,975
Earnings at 30	15,014	(8123)	211,641
Earnings at 40	19,999	(10240)	187,049
Earnings at 50	26,582	(18626)	58,164
Earnings at 60	24,712	(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	1,173	(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.

becomes more prominent. This suggests that higher-earning and more skilled workers retire earlier. 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.

Table 3 provides descriptive evidence of prenatal exposure to a Nazi raid on selected outcomes. Columns (1) and (2) report the mean 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 and after is based on the date of the Armistice (September 8, 1943). Finally, column (5) provides simple DiD calculations of the variable means. The results in column (5) confirm our priors (see Figures 6 and 7): exposure leads to lower earnings[18] and a higher likelihood of being in a blue-collar job. Column (5) also shows that exposed individuals are more likely to experience

18. This is about 2% at age 30 and 6% at age 60.

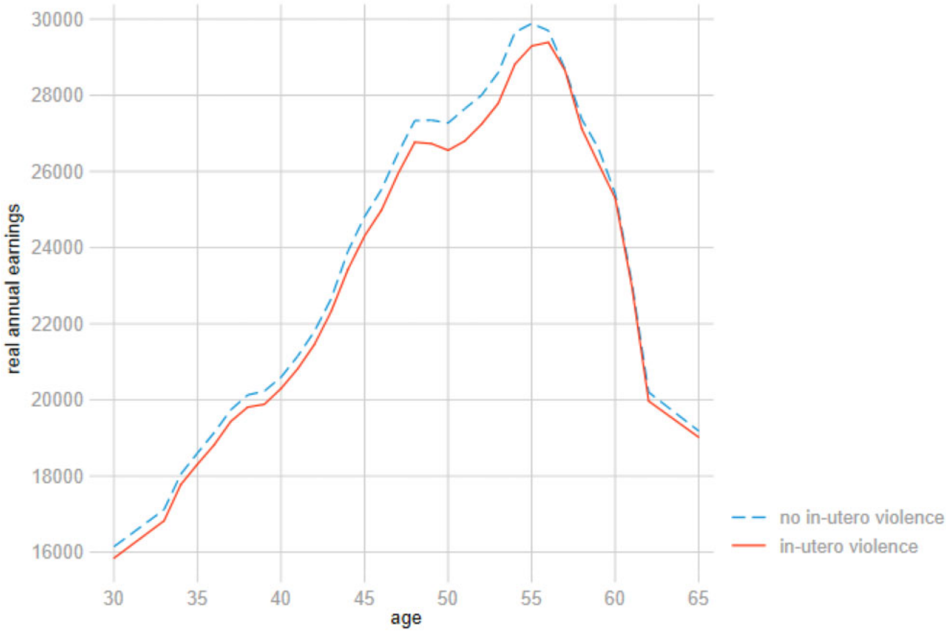

FIGURE 6. Wage earnings by treatment status. The numbers refer to 283,975 men working in the private sector between 1974 and 2017, born between January 1943 and May 1944 in municipalities with fewer than 200,000 residents. The figure plots differences between individuals exposed in utero to a Nazi raid and those not exposed, controlling for municipality fixed effects. Annual earnings are expressed in 2005 euros.

a disability and an unemployment spell during their working career. Interestingly, exposed individuals are also found to retire later.

4. Empirical Model and Identification

4.1. Relationships between Variables of Interest

Before introducing the empirical model, we present in Figure 8 a directed acyclic graph (DAG) to structure thoughts and highlight where we differ from previous studies. The nodes in the figure represent the variables, while the solid arrows represent causal relations between the variables. Nodes written in *italics* represent factors that we do not observe. The dashed arrows represent relations that could potentially confound our causal estimates. The DAG runs forward in time; it starts with in-utero exposure to a Nazi raid (N) that may affect Educational outcomes (EDU) and Labor Outcomes (LO) at later ages (LO age 30, LO age 35,..., LO age 60) and beyond (retirement). There are direct and indirect effects that run between variables.

First, we assume that there is a direct path from the Nazi raid exposure to Educational attainment and to labor market outcomes (occupation and wage earnings)

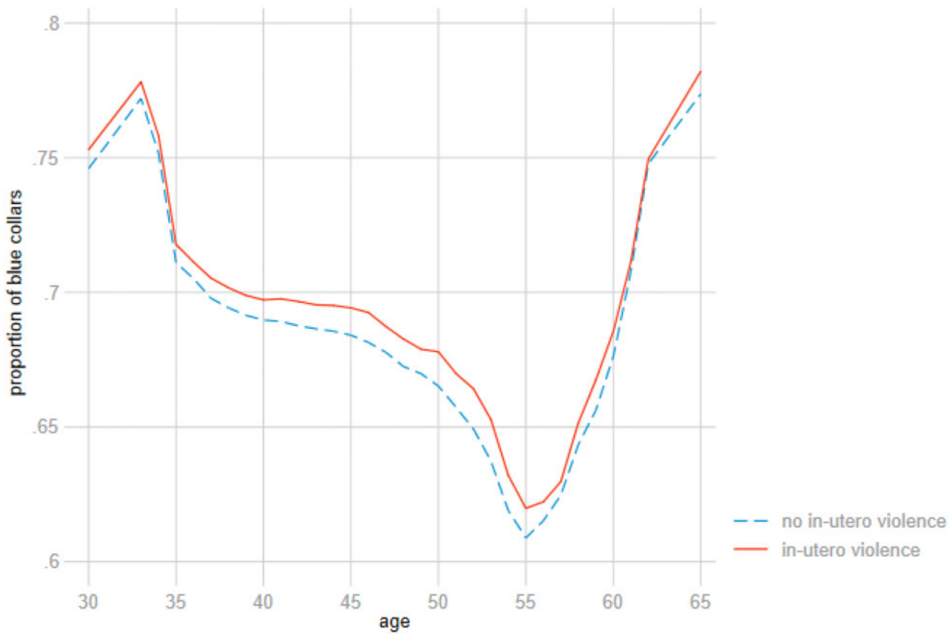

FIGURE 7. Proportion of blue-collar workers by treatment status. The numbers refer to 283,975 men working in the private sector between 1974 and 2017, born between January 1943 and May 1944 in municipalities with fewer than 200,000 residents. The figure plots differences between individuals exposed in utero to a Nazi raid and those not exposed, controlling for municipality fixed effects.

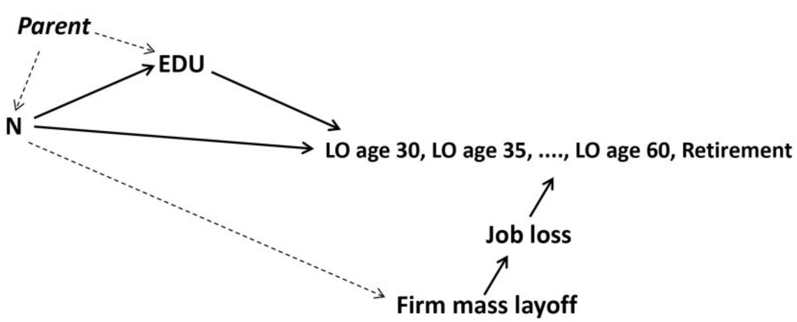

FIGURE 8. Research design and causal effects identification: a directed acyclic graph.

at age 30 and beyond. These are the baseline effect we want to identify. Second, it is also likely that a portion of the baseline effect on labor market outcomes is mediated through Educational Attainment. We assess the relative importance of Educational attainment for wage earnings with a mediation analysis.

Essential for the causal interpretation of N on EDU and N on LO age 30, 35, etcetera is the assumption of random assignment of N. As discussed in Section 2, the raids were intentionally unpredictable and idiosyncratically placed across time and space. In the presentation of our empirical model (see below), we will discuss

TABLE 3. Descriptive DiD 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	14,579	15,212	15,183	15,531	−285**
Earnings at 40	19,683	19,917	20,768	20,622	−381**
Earnings at 50	26,049	26,206	28,721	27,764	−1113***
Earnings at 60	23,865	24,180	27,689	26,387	−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 the outcome variable and when it was measured. Wages and pensions are expressed in 2005 euros. *, **, and ***indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

the plausibility of this random assignment assumption. Still, parents may selectively migrate (referred to by the dotted arrow from *Parent* to N). We address selection issues in Section 6.6. Unobserved parental characteristics (such as parenting skills and/or parental investment behavior) may also affect EDU (and therefore later life labor outcomes). This is referred to by the dotted arrow from *Parent* to EDU. Therefore, the effect of N on EDU is the net effect of a biological effect and parental investment behavior (see also Section 5.2).

Black, Devereux, and Salvanes (2016) study the effect of family ruptures on birth outcomes and labor supply and earnings at the time of the survey (2010). Likewise, Persson and Rossin-Slater (2018) examine the effect of family ruptures on birth outcomes, ADHD medication during childhood, and mental health at age 34–36. In our figure, these effects correspond to the direct effects of N on EDU and of N on LO age 30. We complement these findings by examining the evolution of labor outcomes from age 30 up to age 60 and retirement. Additionally, we assess the portion of the impact of N on LO, mediated through EDU.

As suggested in clinical studies, prenatal exposure to trauma may affect how individuals cope with further shocks later in life.[19] To examine this, we look at the effect of job separations during a working career on wage earnings (the arrow from job loss to LO at later ages). We use individual contract terminations due to mass layoffs, an event more likely to be exogenous from an individual point of view. We formally test whether prenatal exposure to a traumatic event is unrelated to the probability of working in a firm that experiences a mass layoff.

4.2. *Specification of the Empirical Model*

Our baseline specification is the following generalized DiD model:

$$y_{imt}^{a} = \beta_0^a + \beta_1^a Nazi_{mt} + \beta_2^a War_{pt} + \alpha_m^a + \gamma_t^a + \delta_{tr}^a + \varepsilon_{imt}^a, \quad (1)$$

where y_{imt}^{a} represents the labor outcome for individual i, born in municipality m at time (month) t, measured at age a (for a=30, 35, 40, 45, 50, 55, and 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 of whether an individual's municipality of birth had at least one episode of Nazi violence in the nine months before birth. This variable picks up the direct effects of a raid. To control for general war effects we include War_{pt}, a standardized z-score of the number of war-related deaths in the province of birth p in the nine months prior to the month of birth. General war effects aim to control for the adversities of war, including fear and stress resulting from the potential threat of a raid or bombings; nutritional shortages; disease spread; and sub-optimal functioning of health care systems. We also include municipality fixed effects (α_m), *year* × *month* fixed effects (γ_t), and region specific time trends (δ_{tr}). ε_{imt}^a is an idiosyncratic error term.[20] In all our analyses, we cluster standard errors at the municipality level.[21]

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 while controlling for general war effects (War_{pt}). As such, β_1 measures the direct effect of a Nazi raid *over and above* general war effects. 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, we test the conditional (on municipality fixed effects)

19. Medical studies show that the pregnant mother's hormonal response to anxiety and stress affects the stress coping phenotype of the offspring Boersma and Tamashiro (2014).

20. In this DiD setting, the first difference is a comparison of cohorts who were born before and after the armistice, the second difference compares those who are prenatally exposed and those who are not. Our Nazi raid indicator ($Nazi_{mt}$) only turns on when an individual is born after the armistice and prenatally exposed.

21. 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.

random assignment of the raids in more detail and conclude that we cannot reject it.[22]

To address the remaining concerns, we also use an alternative identification strategy that only relies on the weaker assumption of random variation in the exact timing of the raids (results reported in Section 6.2). Effectively, we then compare the offspring of mothers exposed to a raid during pregnancy, as opposed to mothers exposed after giving birth. This identification strategy is the same as in Persson and Rossin-Slater (2018) who examine the effects of grief during pregnancy on offspring's mental health.

Some other considerations are in order before we present our results. It could be argued that the effects of a raid are not only limited to the affected municipalities but also extend to neighboring control municipalities. If true, the control municipalities would be indirectly affected by the raid, which may lead to an underestimate of the effect of a raid. In Section 6, we provide the results of additional analyses where we examine the effect of a raid on neighboring control municipalities. Further, note that spillover effects are not relevant when we estimate our models on the sample of treated municipalities only. Both approaches suggest that spillovers are not important (see Section 6.2).

Further, a potentially traumatic event may give rise to psychological trauma for the parent(s) that persist for a longer period PTSD.[23] PTSD may influence parental (health) behaviors and parenting skills when the child grows up (Akresh et al. 2012b; 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 less optimal parent–child relationships 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.

The lasting effects of a Nazi raid can have important implications for the interpretation of the estimates β_1 in equation (1). The estimate of β_1 will in this case include both a biological and a behavioral (PTSD)/income effect that may also affect the child after birth. 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. We therefore also estimate models that, in addition to the in-utero effect, allow for effects of a Nazi raid in the child's first and second year of life. The absence of statistically significant effects for the first and the second year of life suggests that the estimate of β_1 should be interpreted primarily as driven by a biological effect, rather than via income and/or altered parental behavior. Note that the test for the first and second-year

22. The Nazis may have targeted municipalities that colluded with the partisans. Historical studies (O'Reilly 2001) document the engagement of the partisans with the occupants, but this primarily occurred in big cities or around strategic points, and importantly, it started later in the war when the partisan movement became better organized. We nevertheless estimated the baseline model, excluding raids involving resistance fighters. We also formally test for conditional random assignment and conclude that this assumption cannot be rejected (see Online Appendix A for more details).

23. Note, that only about 2% of individuals exposed to a potentially traumatic life event are diagnosed with PTSD (Benjet et al. 2016).

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	−0.0065**	−0.0073**	−0.0046	−0.0053	−0.0052	−0.0115**	−0.0118*
(z-score) 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

Notes: The samples refer to individuals born in the nine-month window around the Armistice (January 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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

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 a Nazi raid has lasting negative effects on offspring labor market outcomes.

Section 6 also includes a range of additional robustness checks. Failure to adequately control for the general war effect may impact our treatment effect and its interpretation. We therefore also estimate more flexible models where we replace the control for general war effects (War_{pt}) and regional trends with flexible *province* × *year* × *month* fixed effects. We also examine the sensitivity of our findings 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. In Section 6.6, 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.

5. Results

Section 5.1 presents estimates of the baseline model equation (1) on earnings (in logarithms), blue-collar status, disability and unemployment benefit receipt, retirement age, pension benefits (in logarithms), and mortality over the period 1974 (aged 30–31) to 2018 (aged 74–75). In Section 5.2, we present the results of job loss at later ages on subsequent labor earnings and mortality.

5.1. Labor Market Outcomes across the Life Cycle

The results for wage earnings are presented in Table 4. At age 30, the prenatally exposed show an earnings penalty of about 2% and this effect is slightly smaller at

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 in utero	0.0222***	0.0028	0.0176***	0.0172***	0.0184***	0.0277***	0.0370***
	(0.0055)	(0.0067)	(0.0063)	(0.0062)	(0.0065)	(0.0072)	(0.0109)
WWII casualties (z-score) in utero	0.0030	0.0039**	0.0062***	0.0048**	0.0050**	0.0064**	0.0027
	(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

Notes: The samples refer to individuals born in the nine-month window around the Armistice (January 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. $^{***} p < 0.01$, $^{**} p < 0.05$, $^{*} p < 0.1$.

ages 40 and 45, but it widens at later ages, to increase to 5.5% at age 60.[24] We also find significant negative general war effects (standardized z-score of WWII casualties) on earnings, but these are considerably smaller than the Nazi raid effects.

The persistent differences in earnings for the exposed could be due to differences in the skill level and the type of jobs held. 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 started retiring as early as age 55, and by age 60 only 22% were still at work. This retirement effect may be selective with respect to earnings and the type of job.

Table 5 shows the results for the type of job. The prenatally exposed are more likely to work in lower-skilled blue-collar jobs. As in Table 4, the strongest effects are found at age 60. This supports the idea that retiring prior to age 60 is more likely for white-collar workers (with on average higher earnings).

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. Information on educational attainment is available for workers whose employment contracts changed after 2010 (Section 3.2.3). A contract change might be related to prenatal exposure to a Nazi raid. We, therefore, regressed being present in the education sample on in-utero exposure to a Nazi raid and other

24. Note that this is remarkably similar to the results in Table 3.

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	0.0013	0.0450**	−0.0357***
in utero	(0.0026)	(0.0188)	(0.0121)
WWII casualties	0.0005	0.0104	−0.0035
(z-score) 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

Notes: The samples refer to individuals born in the nine-month window around the Armistice (January 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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

controls. Column (1) of Table 6 shows no significant association between the two variables (t-statistic of 0.014). In columns (2) and (3), we show the effects of prenatal exposure on educational attainment: The exposed have lower levels of education, suggesting that the traumatic experience of a Nazi raid affects cognition and education, which in turn may affect job skill levels and earnings. This is also in line with findings from the medical and economic literature (see, for instance, Gitau et al. 2001; Aizer, Stroud, and Buka 2016).

To further support this interpretation, we quantify the impact of education as a mediator for earnings at age 50.[25] For this, we performed a mediation analysis as in Heckman, Pinto, and Savelyev (2013); Adhvaryu, Fenske, and Nyshadham (2019); Huber (2014).[26] These results, reported in Table 7, show that 42% of the treatment effect on wage earnings can be explained by educational attainment and that having a tertiary education accounts for 40% of the total treatment effect.[27] This provides support for the assumed mechanism: The traumatic event has an important effect on education, which in turn affects later life wage earnings.

In Table 8, 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 ever having received a disability insurance

25. At later ages, the number of workers drops rapidly.

26. This involves estimating two sets of weighted regressions, one for the degree to which the impact of a Nazi raid varies by educational attainment and a second for the effect of the Nazi raid on education attainment levels. The inverse probability weights are derived from a regression of the Nazi raids on educational attainment.

27. The quantitative importance of education for wage earnings can be due to the Italian national bargaining system, where educational attainment almost exclusively determines the level of job qualifications in labor contracts.

TABLE 7. Impact of Nazi raids in utero on education attainment levels and their contribution to the total treatment effect on log wages.

	Mediation model	Baseline model
Nazi raid	−0.004	−0.021**
in utero	(0.078)	(0.009)
No education	−0.506***	
	(0.040)	
Primary education	−0.573***	
	(0.031)	
Tertiary education	0.542***	
	(0.043)	
Nazi×No education	−0.021	
	(0.094)	
Nazi×Primary education	−0.085	
	(0.087)	
Nazi×Tertiary education	−0.194*	
	(0.116)	
R^2	0.479	0.137
N	10,274	158,232
Contribution of mediators		
No education	4.61%	
Primary education	−6.57%	
Tertiary education	−40.47%	
Total contribution of mediators	−42.43%	

Notes: The top panel of the table contains weighted regression models for the mediation model (column 1) and for the baseline model (column 2). Following Huber (2014), we construct inverse probability weights from the probability of treatment as a function of the full set of education attainment mediators, and the usual set of controls and fixed effects. In the bottom panel of the table, we calculate the contribution of each mediating level of education to the total treatment effect on log wages at 50 (Adhvaryu, Fenske, and Nyshadham 2019). The sample refers to individuals born in the nine-month window around the Armistice (January 1943–May 1944) in municipalities with fewer than 200,000 residents working in the private sector at the age of 50 (column 2) and with data on education attainment (column 1). All regressions include controls for WWII casualties, year × month fixed effects, municipality fixed effects, and region-specific time trends. Standard errors (in parentheses) are clustered at the municipality level. *** $p < 0.01$,** $p < 0.05$,* $p < 0.1$.

benefit and dying before age 60. The pension benefit is defined as the logarithm of the first monthly pension benefit at retirement.

The results in Table 8 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 may be due to interruptions in their careers. There is no differential effect with respect to retirement timing, but we do find that the exposed have lower pension benefits. Pension benefits are related to average earnings in the years prior to retirement. It, therefore, seems that the earnings penalty extends to old age. Finally, Table 8 shows that there is no differential mortality according to treatment status.

TABLE 8. 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	0.0033***	0.0006	0.0117	−0.0051	−0.0025
(z-score) 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

Notes: The sample refers to 283,975 individuals born in the nine-month window around the Armistice (January 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 outcomes 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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

5.2. *What if Lightening Strikes Twice?*[28]

The evidence presented above shows that the earnings gap increases with age and suggests that work interruptions due to unemployment may play a role. It could also be that the exposed suffer more from job loss later in life. What are the effects on earnings of a second shock (job loss) for the prenatally exposed? Or, what if "lightning strikes twice"? Cunha and Heckman (2007) and Almond, Currie, and Duque (2018) argue that dynamic complementarities may be important. Dynamic complementarities, 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 negative consequences of job loss later in life may be stronger for those prenatally exposed to a raid.

To address the sensitivity of later life shocks, following an adverse shock early in life, we use the information on mass layoffs available in the linked worker-firm INPS data. Specifically, we examine whether the effect of job loss due to a mass layoff on subsequent earnings and mortality is different for the prenatally exposed. Contract terminations resulting from mass layoffs are convenient in this setting, as this type of job separations is more likely to be exogenous from an individual point of view. We also formally test this assumption and find 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. See Table B.3 of Online Appendix B.2 for the results.[29]

28. Sub-section title adapted from Almond, Currie, and Duque (2018); Section 5.

29. This also holds for blue-collar status (results available upon request).

The literature on the effects of job loss due to mass layoffs shows that displaced workers tend to experience significant long-term earning losses (Ruhm 1991; Jacobson, LaLonde, and Sullivan 1993), lower-employment rates (Chan and Stevens 2001), strong increases in mortality for male workers, persisting up to 20 years after job displacement (Sullivan and von Wachter 2009) and higher suicide rates and hospitalization due to traffic accidents, alcohol-related diseases, and mental illness (Browning and Heinesen 2012).

We use the mass layoff definition of Sullivan and von Wachter (2009): a reduction of at least 30% in employment between period $t-1$ and t in a firm with more than 25 workers. These mass layoff events are matched with individual contract terminations between 1983 (age 40) and 2004 (age 61).

To assess the causal effect of job loss due to mass layoff (LO), we specify the following triple DiD model:

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

where LO^a_{it} is an indicator that equals 1 if an individual i, born at time t loses his job due to a mass layoff at age $a-1$. As in equation (1), β^a_1 measures the average effect of being exposed prenatally to a Nazi raid. The parameter β^a_2 is the effect of a layoff, the estimate usually reported in the existing literature on mass layoffs (Ruhm 1991; Jacobson, LaLonde, and Sullivan 1993; Sullivan and von Wachter 2009). The triple DiD parameter β^a_3 measures the *additional* effect of a job loss due to a mass layoff for the prenatally exposed. So the sum β^a_1 and β^a_3 is the *total* effect of a layoff for the prenatally exposed. When our outcome is earnings, we measure the effect on the next year's earnings. For mortality, we look at the effect on the probability of dying within the next 10 years.

Table 9 presents the estimates for next year earnings for workers aged 45–60. The effect of prenatal exposure to a Nazi raid (β^a_1 in the first row) is very similar to the general treatment effect presented in Table 4, hinting that the assignment of the mass layoff is not associated with prenatal exposure to a Nazi raid. The estimates of β^a_2 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, LaLonde, and Sullivan (1993) find immediate losses of more than 40%. Importantly, the estimates of β^a_3 in Table 9 show that the prenatally exposed suffer an additional earnings penalty at ages 45, 50, and 55 of about 8%, 10%, and 15%, respectively.[30] Hence, an average worker faces an immediate earnings loss after job loss (due to a mass layoff) of about 31%–33%, which can increase to more than 47% for those prenatally exposed to a Nazi raid. This is in line with Baradwaj et al. (2019) who find for private sector workers that differences in

30. Note that the effect at age 55 rather than at age 60 is most relevant for the large earnings penalty at age 60 in Table 4.

TABLE 9. 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	−0.0063*	−0.0063*	−0.0097*	−0.0058
(z-score) 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

Notes: The samples refer to individuals born in the nine-month window around the Armistice (January 1943–May 1944) in municipalities with fewer than 200,000 residents, and refer 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. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

TABLE 10. The effect of in-utero exposure to a Nazi raid: Effect of mass layoffs on the 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	−0.0004	−0.0006	0.0007	−0.0005
(z-score) 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

Notes: The sample refers to individuals born in the nine-month window around the Armistice (January 1943–May 1944) in municipalities with fewer than 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. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

early health (as measured by birthweight) lead to increased inequality in employment outcomes.

Regarding the mortality effects, Table 10 shows that job loss increases mortality in the next 10 years. However, we find no evidence of additional effects for the prenatally exposed.

Our findings on the interaction between prenatal exposure to a traumatic event and another exogenous shock later in life suggest that dynamic complementarities may be important (Cunha and Heckman 2007; Almond, Currie, and Duque 2018). However, formally testing for dynamic complementarities is difficult. As argued by (Almond and Mazumder 2013), causal inference on dynamic complementarities requires "lightning to strike twice", that is, it requires exogenous variation in the baseline stock as well as in the second shock later in life. Such settings are rare in practice, but our empirical design and the extremely long follow-up of worker-firm matches potentially allow us to address this issue.

However, exogeneity of the two shocks is a necessary, but not sufficient condition (Malamud, Pop-Eleches, and Urquiola 2016), as parents may respond with investments in their children to counter the adverse effects of a bad start. Specifically, following Yi et al. (2015); Almond, Currie, and Duque (2018), the total effect of a prenatal shock (e) on the child's human capital at adult ages θ can be decomposed into two parts:

$$\underbrace{\frac{\mathrm{d}\theta}{\mathrm{d}e}}_{A} = \underbrace{\frac{\partial\theta}{\partial e}}_{B} + \underbrace{\frac{\partial\theta}{\partial I}}_{C} \times \underbrace{\frac{\partial I}{\partial e}}_{D}. \tag{3}$$

The term (A) on the left-hand side of equation (3) is the reduced form effect of the early-life shock and corresponds to the usual reduced form estimate in the empirical literature on long-run effects of early-life shocks. The first term on the right-hand side (B) is the biological effect directly operating through the human capital production function. The second term ($C \times D$) is the behavioral effect from the parental investment response, where (C) is the productivity effect of the investment (the marginal efficiency of investment) and D the resource allocation effect.[31] When parents make compensating investments to counter the adversities of the initial shock, the term ($C \times D$) will be negative, which will bias the effect of the second shock (layoff) toward 0. With reinforcing investments, parents may decide to invest resources in other, better-endowed children in the family, or even to reduce spending on the disadvantaged child. In this case, $C \times D \geq 0$, which will lead to an upward bias of the effect of the second shock. Unfortunately, we do not have information on parental investment decisions and therefore cannot formally check this. However, the negative and significant effect of the triple DiD parameter β_3^a indicates that parents do not make fully compensatory investments in their children.

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

31. Note that the resource allocation effect D may be affected by the effect of the initial shock on the parents.

6. Robustness Checks, Falsification Tests, and Selection Effects

This section starts with estimates from an alternative model to tests for the relevance of trauma-related changes in parental behavior and parenting skills (Section 6.1). In Section 6.2, we examine the assumption of conditional random assignment and Section 6.3 tests for spillover effects. We next perform several robustness checks (Section 6.4). The results of these analyses are summarized in Panels B–G of Tables 11 (earnings), 12 (blue-collar status), and 13(mass layoffs). The baseline results are repeated in Panel A. The tables only report the 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 falsification tests. Section 6.6 addresses issues of selective fertility, mortality, and mobility.

6.1. PTSD and the Common Trend Assumption

As discussed in Section 4, witnessing violence may lead to lasting psychological trauma for the parent(s) (PTSD), which in turn may influence parental (health) behavior and parenting skills (Akresh et al. 2012b, 2012a).[32] Furthermore, property destruction and income loss following a raid may also have longer-term consequences. In the presence of such effects, the estimate of in-utero exposure in equation (1) includes 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 analysis period to the left, that is, the time window is [−24,9] around September 8, 1943. This alternative specification is equivalent to a test on the common trend assumption.

The results of these analyses are reported in Panel B of Tables 11–13. 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 second-year effects show small and insignificant coefficients. This indicates that the common trends assumption is satisfied[33] and that it is primarily prenatal exposure and not reduced income and/or altered parental and parenting behavior post-birth that drives our findings.

6.2. Conditional Random Assignment Assumption / A Different Identification Strategy

It was argued in Section 2 that the Nazi raids were idiosyncratically distributed in time and space. While the exact timing of the raids was hard to predict, one could argue that the assumption of conditional random assignment across municipalities may

32. Note that in the case of PTSD, the changed behavior will also affect the parental investment decision (the term D in equation (3)).

33. We additionally estimate a monthly event study that confirms that the common trend assumption cannot be rejected (results available upon request).

TABLE 11. 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 —5 km 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 six 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

TABLE 11. Continued

	Age 30	Age 35	Age 40	Age 45	Age 50	Age 55	Age 60
(I) Changing the municipality size cut-off—municipalities under 500 k residents [−9,9] window							
Nazi raid in utero	−0.0244***	−0.0268***	−0.0140**	−0.0191**	−0.0294***	−0.0310***	−0.0526***
	(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 in utero	−0.0158**	−0.0159**	−0.0077	−0.0116	−0.0203**	−0.0252**	−0.0463***
	(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

Notes: Unless otherwise specified, all estimates are based on municipalities with fewer than 200,000 residents, are obtained for age-specific subsamples (30–60), and 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. $^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.1$.

not hold. In Online Appendix A, we more formally conduct a test for the assumption of conditional random assignment and find that this assumption cannot be rejected. Finally, we estimate equation (1) on 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 (Persson and Rossin-Slater 2018). The results of this exercise for earnings, blue-collar status, and mass layoffs are reported in Panel C of Tables 11–13. The results are very similar for blue-collar status and job loss due to mass layoffs but become smaller and less precise for earnings at ages 55 and 60.

6.3. Testing for Spillover Effects

Information about a Nazi raid may transmit to neighboring villages/municipalities and fear of a Nazi raid may also impact their residents. If this is the case, some control municipalities may also have been affected. This will downward bias estimates of β_1 (equation (1)) and β_3 (equation (2)). To examine the relevance of such effects, we exclude the municipalities that underwent a raid. Next, in the remaining sample (of the controls), we define the municipalities in the vicinity (within a distance of 5 km) of treated municipalities as treated. Panel D Tables 11–13 show that all estimates are small and not significant, indicating that there are no spillover effects.[34,35]

34. We also perform analyses where we keep the treated municipalities in the sample, but exclude nearby (5 km radius) municipalities. The results are virtually identical to the baseline results (Panel A). Results available upon request.

35. Further, note that spillover effects are not relevant when we estimate the model on treated municipalities only. The results of Section 6.2 also suggest that spillover effects are not important.

TABLE 12. 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 —5 km 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 six 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

TABLE 12. Continued

	Age 30	Age 35	Age 40	Age 45	Age 50	Age 55	Age 60
(I) Changing the municipality size cut-off—municipalities under 500 k 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

Notes: Unless otherwise specified, all estimates are based on municipalities with fewer than 200,000 residents, are obtained for age-specific subsamples (30–60), and 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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

6.4. *Flexible Province Trends and Different Samples*

Flexible Province Trends. The control variable War_{pt} in equation (1) may not be able to fully control for general war effects. This may affect the estimate as well as the interpretation of β_1. We therefore use more flexible models where we replace War_{pt} and the regional trends by *province* × *year* × *month* fixed effects. The results in Panel E of Tables 11–13 show that this does not affect our effect estimates.

Changes in the Length of Gestation. As a further sensitivity test, we explore the effect of changes in the gestation length. We defined our exposure variable $Nazi_{mt}$ assuming a nine-month gestation period (see Section 3.4). However, maternal stress associated with a traumatic event may lead to preterm birth, which in the medical literature is defined as a gestation period of fewer than 37 weeks (Lilliecreutz et al. 2016). 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 gestation length or preterm status. We, therefore, proceed differently and examine the sensitivity of our results by estimating equations (1) and (2) assuming both an eight-month and a seven-month gestation period.[36] We report the results of this exercise in Panel F of Tables 11–13. Reducing the gestation period increases the magnitude and significance of the estimates.

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

TABLE 13. 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	0.0069	0.0045	−0.0084	0.0437
in utero (β_1^a)	(0.0155)	(0.0163)	(0.0320)	(0.0487)
Layoff (β_2^a)	−0.3351***	−0.3217***	−0.3355***	−0.3159***
	(0.0141)	(0.0121)	(0.0166)	(0.0239)
Layoff × Nazi raid	−0.0452	0.0075	0.0731	0.0244
in utero (β_3^a)	(0.1293)	(0.1155)	(0.1200)	(0.3137)
N	143,800	135,032	78,326	35,658
(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

TABLE 13. Continued

	Age 45	Age 50	Age 55	Age 60
(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 six 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 500 k 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

Notes: Unless otherwise specified, all estimates are based on municipalities with fewer than 200,000 residents, are obtained for age-specific subsamples (45–60), and 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. *** $p < 0.01$,** $p < 0.05$,* $p < 0.1$.

Using only Information from the First Six Months after the Armistice. As the Armistice was unexpected, 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 Section 6.6). However, as the war went on, more information about the raids and WWII, in general, may have been disseminated. This may imply that with time, fear, and stress may have gained importance in control municipalities, leading to a downward bias in our effect estimates. Further, Table B.2 in Online Appendix B.1 shows that in the first six months after September 8, the raids involved primarily civilian victims. For these reasons, we re-estimate our main model using the data up to six 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). The results are reported in Panel H of Tables 11–13 and show that the effects are very similar and quantitatively even gain in importance.

Resistance Fighters and other Specifications. Related to the above, some of the raids may have been targeted at resistance fighters. This may confound the findings. Similarly, from September 1943 to April 1945, Italy was divided into two separate states: the Northern part that was still under Mussolini, which was effectively a German puppet state, while the rest of the country remained under the kingdom of Italy. Finally, the Apennine mountains in central Italy were a natural defense front that was not broken until the end of the war and the Nazis may have been particularly violent in these areas.[37] We therefore also estimate the main model excluding raids that involved resistance fighters, that excluded the northern regions (Piemonte, Valle d'Aosta, Lombardia, Trentino Alto Adige, Veneto, Friuli Venezia Giulia, and Liguria) and municipalities with median altitude above 400 meters. These alternative specifications do not change our main findings. For the results, we refer to Tables D.16 and D.17 of Online Appendix D.6.

Changing the Municipality Size Cut-off. The nature of the war was different in large cities (see Section 3.3). 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 varying the municipality size. The results are reported in Panels H and I of Tables 11–13. Panel H shows the results if we use a cap of 500,000 and Panel I when there is no cap. As expected (see the discussion in Section 3.3), relaxing the cap leads to smaller and less precise parameter estimates of β_1 (and β_3).

6.5. Falsification Test

Finally, we verify our identification assumptions and demonstrate the statistical power of our inferences by falsification tests. More precisely, we randomly assign the Nazi

37. We thank an anonymous referee for these comments.

raids to individuals in our sample. If our identification strategy is valid, we would expect estimates using those pseudo-samples to be centered around zero. 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 B.3, and B.4 of Online Appendix B.4 (results for other ages are available upon request). As expected, both distributions are centered around zero and the *t*-statistics of our main analyses (indicated with the vertical dotted red line) are at 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%.

6.6. Selection Effects

Private Sector Selection. Our analyses are based on a sample of private sector workers, which may question external validity. The INPS pension data has information on all workers, both working in the private and public sectors. This allows us to examine whether the prenatally exposed were more (or less) likely to sort into the private sector. We find that they were not. More specifically, we regress an indicator for working in the private sector on in-utero exposure to a Nazi raid, municipality fixed effects, the number of war casualties, $year \times month$ fixed effects, and regional trends. The coefficient of the Nazi raid is small (−0.0015) and not significant (s.e. 0.0011, p-value of 0.17).

Selection Issues at Conception, in Utero, and in Later Life. Confounding due to selection can occur at several stages in the life cycle: (i) at conception; (ii) in utero; (iii) between birth and 1974 (age 30); and (iv) after 1974. With respect to selection at conception, our sample pertains to all births conceived *before* the date of the unexpected proclamation of the Armistice. Moreover, should conception rates vary with socio-economic status and structural differences between municipalities, the municipality fixed effects would account for this.

Further, selective mortality due to reasons (ii)–(iv) is likely to lead to the survival of the stronger individuals. In this case, our estimates are likely to be a lower bound of the true effects. We nevertheless examined this further. To deal with in-utero selection, we 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. We regress regional mortality rates due to pregnancy complications from 1941 to 1946 on Nazi raids, the number of WWII casualties (War_{pt}), and year and region-specific fixed effects. Table B.4 of Online Appendix B.5 shows that Nazi raids did not increase mortality due to pregnancy complications.

Mortality selection after conception can occur in utero (miscarriages), between birth and the first time we observe individuals at age 30 and later. The results in column (2) of Table 8 show that there is no mortality selection between ages 30 and 60 (iv). To test for selective mortality due to miscarriages and between birth and age 30 (ii and iii), we estimated the following regression model on the sample of treated and control municipalities:

$$CS^a_{mt} = \beta^a_0 + \beta^a_1 Raid_{mt} + \beta^a_2 Cas_{pt} + \alpha^a_m + \gamma^a_t + \delta^a_{tr} + \varepsilon^a_t, \quad (4)$$

where CS^a_{mt} is the standardized cohort size at age a ($a = 30$) in municipality m at time t (for t = January 1943, ..., May 1944), $Raid_{mt}$ equals 1 if municipality m was exposed to a Nazi raid in the nine months prior to time t, Cas_{pt} the number of casualties in province p in the nine months prior to time t, α^a_m are municipality fixed effects, γ^a_t are *year* × *month* fixed effects, δ^a_{tr} regional trends, and ε^a_{mt} an idiosyncratic error term.

Our main coefficient of interest is β^a_1, which we report in Table B.5 of Online Appendix B.5.[38] 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. The unexpected declaration of the Armistice is likely to rule out endogenous residential mobility in the short time window of our analyses. Moreover, movements across Italian provinces during WWII were very difficult due to the destruction of railroads and transportation networks (Baldoli and Knapp 2012). To empirically test for selective mobility, we use an event study model that relates standardized municipality cohort sizes to leads and lags of Nazi raid exposure, province level general war effects War_{pt}, municipality fixed effects, *year* × *month* effects, and regional trends. We take the normalization at the month of a Nazi raid. Residential mobility may affect our effect estimates if prior to the raid people moved out of the affected municipality, leading to lower cohort sizes. Figure B.5 in Online Appendix B.5 plots the estimated lead and lag coefficients. The figure shows that the lead and lag effects are very small (less than 0.05 of a standard deviation) and not significantly different from 0, suggesting that endogenous residential mobility is not likely to bias our estimates.

7. Interpreting our Results: Is it Sstress?

The onset of the armistice brought the war to Italy. A war may lead to food shortages, outbreaks of infectious diseases, and sub-optimally functioning healthcare systems. Further, the *potential* threat of bombings produces fear and stress among the population (Becker and Rubinstein 2011). To account for such effects, we control for general war effects (War_{pt}) and other time-varying province characteristics (*year* × *month* fixed effects). Therefore, $Nazi_{mt}$ measures the 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 the Nazi raid effect. However, it must be noted that any study attempting to interpret effect estimates as being driven by stress can never rule out other mechanisms. This holds even for studies that measure actual stress hormone (cortisol) levels.

38. Note that β^a_1 may also pick up fertility effects.

The clinical literature offers exhaustive reviews of the damage related to prenatal maternal stress (PNMS) resulting from PTEs. PNMS is found to negatively affect the mental, cognitive, emotional, and immunological functioning of the offspring (Checkley 1996; Selten et al. 1999; Hansen, Lou, and Olsen 2000; Matthews 2000; Gitau et al. 2001; Heffelfinger and Newcomer 2001; Weinstock 2001; Mulder et al. 2002; Lederman et al. 2004; de Kloet, Joëls, and Holsboer 2005; Leeners et al. 2007, among others). PNMS may lead to abnormal activity of the Hypothalamic-Pituitary-Adrenocortical-axis, which not only exposes the fetus to altered stress hormone levels, but may also increase the permeability of the placental barrier (Van den Bergh et al. 2005), affecting placental functioning and exposing the fetus to higher maternal glucocorticoids (a.o. cortisol), hormones that affect fetal brain development (Zhang et al. 2018).

Additionally, PNMS stimulates the production of Corticotropin-Releasing Hormone (CRH) (Majzoub and Karalis 1999). Amongst others, CRH affects responses to stress, addiction, and depression. Boersma and Tamashiro (2014) demonstrated how PNMS may modulate offspring stress-coping phenotype. Yehuda et al. (2016) show that PNMS is associated with epigenetic alterations that are evident in both the exposed parent and the offspring.

Recent economic literature supported these claims by identifying the effects of PNMS on birth outcomes, education, and mental health (Camacho 2008; Aizer, Stroud, and Buka 2016; Black, Devereux, and Salvanes 2016; Quintana-Domeque and Rodenas-Serrano 2017; Persson and Rossin-Slater 2018). Our findings for educational attainment and the mediation analysis (Section 5.1) are in line with these studies. Below we examine whether, even after controlling for general war effects, $Nazi_{mt}$, may still pick up other effects such as hunger, worse maternal health and health behaviors, and family income.

7.1. Supporting Evidence in Favor of a Stress Interpretation

Is it Prenatal?. In Section 4, we refer to studies showing that the effects of psychological trauma may persist over a longer period and affect parental health (behaviors) and parenting skills. Moreover, the Nazi raids may also have affected family incomes. Consequently, the effects of the raids may extend beyond the prenatal period. In Subsection 6.1 we report the results allowing for exposure in the first and second year of life and found small and not significant effects. This means that the effect of $Nazi_{mt}$ is primarily driven by prenatal conditions and not by behavioral and/or income effects that extend into childhood.

Is it Stress or other War-Related Conditions such as Malnutrition?. To examine this, we use the Health Search/CSD Patient Database, a nationally representative sample of adult patients, that contains electronic clinical records from General Practitioners (GPs). The records include ATC (Anatomic Therapeutic Chemical) drug classification codes, which we use to compute drug expenditures for specific

TABLE 14. Effect of prenatal exposure to Nazi raids on health expenditure types.

	(1) Neuro/ Mental	(2) Cardio/ Diabetic	(3) Respiratory system	(4) Hormone system	(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	−2.4008	−.1176307	−.0043448	.8692845	−4.107407	−.2607241	−.8289242*
(*z*-score) 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

Notes: The sample refers to individuals born in the nine-month window around the Armistice (January 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. $^{***} p < 0.01, ^{**} p < 0.05, ^{*} p < 0.1$.

disease types at the patient level.[39] We regress ATC health code expenditures 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. In Online Appendix C, we also present some descriptives and the results of alternative specifications (Tables C.1, C.2, and C.3). The results in Table 14 show a significant and sizable effect of Nazi raids only 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; Selten et al. 1999; Heffelfinger and Newcomer 2001). This finding also aligns with the results of Persson and Rossin-Slater (2018) who found the effects of maternal grief on adolescent mental health.

What about food shortages? During and just after WWII some countries (Greece (1941–1942), Russia (Leningrad, 1942), The Netherlands (1944–1945), and Germany (1945–1946)) experienced severe famines, with daily food intake dropping to 500–1,000 calories per day (Van den Berg and Lindeboom 2018). The medical and epidemiological literature generally agrees that famine exposure has a strong association with BMI, diabetes, and hypertension (Van den Berg and Lindeboom 2018). Lumey, Stein, and Susser (2011) reviewed epidemiological studies based on the Dutch, Chinese, and Leningrad famines and concludes that there is a persistent association between early-life famine exposure and BMI, diabetes, and schizophrenia.

39. More information about this database and the Italian healthcare system can be found in Online Appendix C.

Up to 1942 food production in Italy was at the pre-war level (2600 calories per day), but food shortages became more apparent in 1942 and supply dropped to about 2,000 calories per day and remained at these levels until the end of the war (Daniele and Ghezzi 2017; Vecchi 2017; Ó Gráda 2019).

This is substantially higher than the caloric intake during the famines of the countries mentioned above. Also, there was a stark rural–urban distinction. Rural nutrition was quantitatively not significantly dissimilar to pre-war years, while urban households were more likely to suffer from food shortages (Daniele and Ghezzi 2017).[40] In our identification strategy, we wash out the effects of war-related shortages by controlling for the intensity of the war and focusing on the effect of short-lived Nazi raids in smaller municipalities. Indeed, the regression results in Tables 14 (and Tables C.2 and C.3 of Online 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. Lastly, famine studies also find effects for children who have been exposed post-birth (see Lumey, Stein, and Susser 2011). We do not find any effect for exposure in the first or second year of life (see Section 6.1).

8. Conclusion and Discussion

This paper complements the existing literature by examining the causal effect of a traumatic and stressful event experienced by a pregnant mother on her offspring's life-long labor market outcomes. We exploit a unique natural experiment involving short-lived, randomly placed violent Nazi raids across municipalities after the Armistice of September 8, 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 and time.

In a generalized DiD model, we find that prenatally exposure to a Nazi raid leads to an earnings penalty of about 2% at the start of their working career. The prenatally exposed also hold lower qualification jobs. The earnings penalty persists over time and increases to about 6% at age 60. These lower earnings translate into lower pensions. About 42% of the earnings effect is mediated through education. 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 event. We thus find that a bad start may amplify the adverse effects of negative shocks later in life. This suggests that dynamic

40. Larger cities generally, for 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 are mostly rural municipalities.

complementarities (Cunha and Heckman 2007) may be important. Our findings are robust against alternative specifications and falsification tests.

We argue that stress is likely to be an important mechanism driving our findings. Previous work in medical sciences, epidemiology, and economics has documented the negative effects of potentially traumatic events and the associated stress on various psycho-pathologies later in life such as decreased learning function, cognition, education, and mental health. Indeed, we find lower educational attainment for those who were prenatally exposed to a Nazi raid. We additionally find higher medical care expenditures on diseases of the nervous system and mental disorders. We also show that the effects of Nazi raids are not driven by hunger, changes in maternal health behaviors, and reduced family income.

While our findings are in accordance with results from earlier economic studies that focused on cognition, years of education, and the mental health of children and adolescents (Black, Devereux, and Salvanes 2016; Persson and Rossin-Slater 2018), the raids used in our study are meaningfully different from the bereavement measure used in previous work.

In addition to a different treatment, the labor market outcomes in our study go beyond outcomes in previous work, and the subjects involved are much older. Nevertheless, the effect estimates on prescription drug consumption are comparable. We find that males aged 60–65, who were prenatally exposed to a traumatic event, are about 3.6 percentage points more likely (about a 13% increase) to purchase drugs for diseases of the nervous system and mental disorders, and have 17% higher medical expenditures. Persson and Rossin-Slater (2018) find a 13% and 8% increase in the likelihood of consuming drugs for anxiety and depression.

Besides prescription drugs use, prenatal exposure to a traumatic event may also affect other medical costs, social security spending, productivity, and lifetime income. The per-person present discounted value (PDV) of income losses (in 2005 euros) due to a traumatic experience is estimated at 14,219 euros. This is equal to about one year of earnings at age 30.[41]

What about external validity? Our study concerns a historical context that could be described as extreme and rare, but we know that in developing countries like Syria, Venezuela, Afghanistan, South Sudan, and Myanmar war, millions are affected by conflict and terror. The recent war in Ukraine is a grim reminder that also in the developed world violence against civilians extends to settings observed today. Besides, stressful conditions are not only limited to war but also hold for deprived neighborhoods, where crime, unemployment, and poverty rates are high.

41. The per-person PDV of life-long income loss is derived using:

$$PDV^{loss} = \sum_{a=30}^{100} \frac{\beta_1^a \times \overline{income}_a}{(1+i)^{a-30}} \times P(a+1|a), \tag{5}$$

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.

During the COVID-19 pandemic, fear for health, economic security, and well-being pervaded families with poor qualifications and limited resources dis-proportionally. Consequently, traumatic and stressful events may play an important role in the persistence of low socio-economic status across generations (see also Aizer, Stroud, and Buka 2016). Our findings suggest that public programs targeted at (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.

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Supplementary Data

Supplementary data and Appendices are available at *JEEA* online.

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