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Missing Men: World War II Casualties and Structural Change

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Abstract:
A shock to the sector composition of the local labor market can affect long-run economic development of a location. Because structural change ultimately shifts labor from agriculture to services, an early transition to manufacturing may hamper longrun prosperity. The identification strategy exploits military World War II (WWII) casualties in Austrian municipalities as an exogenous shock to the local labor market. WWII casualties shifted labor out of agriculture into manufacturing in the short-run, which eventually led to a differential path of structural change. In the long-run, I find a strong and robust negative effect of WWII casualties on subsequent economic output.

Keywords: Spatial equilibrium, local labor markets, structural change, World War II, Austria.

JEL classification: R11, R12, J40, N14.

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1 Introduction

A well-known feature of economic growth is structural change: the reallocation of economic activity away from agriculture towards manufacturing and later the service sector (Kuznets, 1957; Herrendorf *et al.*, 2014). The New Economic Geography literature has theoretically worked out that production features inherent to different sectors can cause divergent development paths (Krugman, 1991; Krugman and Venables, 1995). It seems therefore natural to inquire if structural change, as a period of rapid change in sectors of production, affects the long-run distribution of economic activity in space. However, the design-based empirical literature on economic geography has only looked at shocks to the size of the population and not of the sector composition (Davis and Weinstein, 2002; Brakman *et al.*, 2004; Miguel and Roland, 2011; Schumann, 2014).[1]

In this paper, I look at the long-run consequences of a shock to the sector composition of a local labor market through its effect on local structural change. Previous research has shown that workers earn higher wages and produce more output in the medium run if they switch from agriculture to manufacturing (Bauer *et al.*, 2013; Braun and Kvasnicka, 2014; Sarvimäki *et al.*, 2016). However, there is no research if these findings also hold in the long run. Since structural change ultimately shifts labor from agriculture towards the service sector, while the size of the manufacturing sector remains relatively stable, a local economy could benefit in the long run if the labor reallocation takes place at a later point in time but shifts labor directly from agriculture to the service sector. Adding the service sector to the picture is especially interesting because Desmet and Rossi-Hansberg (2009) argue that there is a clear difference between manufacturing and services in terms of agglomeration economies in the second half of the 20th century.

I argue that the short-run adjustment of a location to a labor endowment shock can have large and unexpected consequences if the path of structural change of a location depends on the initial sector composition. In the short-run, a negative labor endowment shock leads to a shift of production resources away from labor intensive agriculture to the more capital intensive manufacturing sector (Rybczynski, 1955). Over the process of structural change small sector switching costs for workers imply that they make as few sector switches as possible. Hence, the majority of labor movements take place directly from agriculture to the growing service sector. The location with the earlier negative labor endowment shock requires less labor reallocation and remains over-represented in manufacturing. If, however, a large service sector generates local positive agglomeration effects, these locations could grow faster

[1] A notable exception is Davis and Weinstein (2008) who look at specific industries within manufacturing. However, they do not look at the service sector.

and attract more economic activity in the long-run. Desmet and Rossi-Hansberg (2009) argue that for a "young" industry, in this case services, the benefits from agglomeration exceed congestion costs, while for a "mature" industry, like manufacturing, the opposite is true. A small initial overhang in service sector employment can therefore make the difference in the long-term location of economic activity.

For the empirical analysis, I exploit military WWII casualties as a natural experiment for a labor endowment shock at the municipality level in Austria. Conscription and location of deployment of Austrian men during WWII was based on birth cohort rather than social or economic background (Morawek and Neugebauer, 1989; Overmans, 1999). Thus, the relative number of casualties that a municipality suffered during the war was arguably exogenous to local development-enhancing factors.

To explore the plausibility of the identifying assumption, I show that the share of military WWII casualties of the male population (henceforth, WWII casualty rate) is uncorrelated with pre-WWII municipality characteristics and measures of education of the local population. Additionally, the WWII casualty rate can not explain pre-WWII economic development, which speaks in favor of a causal interpretation of the estimated effects.

I find that the short-run adjustment to WWII casualties at the municipality level is indeed, as predicted by the *Rybczynski-Theorem*, a shift of labor from the agricultural to the manufacturing sector. Over the next 40 years, low-casualty municipalities experienced faster growth of the service sector than similar areas with more soldiers killed during WWII. The larger agricultural sector in low-casualty municipalities therefore transformed into a larger service sector through structural change.

In the long run, I show that WWII casualties have a large negative effect on local economic output, measured by the total wage bill of a municipality. The difference in economic output comes from a larger service sector in low-casualty municipalities (extensive margin) and higher mean wage in, both, manufacturing and services (intensive margin). This composition of the total effect on economic output is consistent with local productivity spill-overs from the service sector and consequent agglomeration of economic activity. An increase of the WWII casualty rate of one percentage point reduces today's economic output, measured by the total wage bill, by about 5 percent. The magnitude of the effect suggests that the difference in economic activity built up over time through an advantage of low-casualty municipalities in attracting economic activity.

The negative long-run effects prove robust to the inclusion of various control variables. Within-district estimates rule out many political, economic, and geographic confounding factors. As a robustness check, I estimate the effect of WWII casualties in a sample of bordering municipalities and include neighbor fixed effects to rule out unobserved regional confounding factors. The unchanged negative effect on variables of economic performance in this particular setup suggest a high degree of internal validity of the estimates.

A threat to identification that I can not directly rule out, is a different size of drafted birth cohorts. Because of data limitations, I employ the size of age groups measured either imprecisely before or after WWII as control variables and check the robustness of the main results. It turns out that the inclusion of age group variables measured in the censuses 1939, 1951, and 1961 and detailed gender-specific demographic variables from 1971 does not change the estimated short- and long-run effects.

In an attempt to rule out alternative channels how WWII casualties affect today's local economic output, I look at the effect of WWII casualties on educational outcomes of the post-war population and differences in fertility and migration. In line with the main argument of the paper, there is no effect of WWII casualties on the education level of the local population. Additionally, I can also show that fertility and migration streams were not affected by WWII casualties.

Although one can never be sure, the evidence in this paper points towards a causal effect of WWII casualties on the long-run economic development that works through the path of structural change. To the best of my knowledge, this is a channel of persistence of historic events that has not been shown before in the literature. Nunn (2014) recently surveyed the broad literature on the long-term effects of historic events and addresses a wide array of historic events and mechanisms underlying persistence.

This paper is closely related to a body of work on the long-term effects of shocks to the local population. Acemoglu *et al.* (2011) show the effect of the Holocaust in Russia on subsequent economic and political development. The authors document a negative relationship between the reduction of the Jewish population and subsequent population growth, wages, and the communist vote share. The Holocaust eliminated the middle class, that consisted mostly of Jews, and contributed in this way to a long-lasting difference between Russian cities and regions. A number of contributions argue that labor shortages can be the driving force behind adjustment processes (Habakkuk, 1962; Acemoglu, 2010). Empirical research shows that labor shortages in agriculture can affect long-run economic outcomes through the land tenure system and public good provision (Dell, 2010) and increased capital intensity in agriculture (Hornbeck and Naidu, 2014). As an addition to this literature, I show that a labor endowment shock can have an effect on long-run development through labor adjustment between sectors and consequently a different path of structural change.

This research also relates to the empirical literature on labor movement between sectors. Braun and Kvasnicka (2014) show that refugees after WWII facilitated the growth of industrial production in Germany over the next 25 years. Bauer *et al.* (2013) argue that forced migrants arriving in Germany after WWII could increase their income by leaving agriculture. Sarvimäki *et al.* (2016) confirm these results for displaced Finns. However, this literature only looks at short- to medium-run effects of labor reallocation between sectors. I show that a change to the initial sector composition can have long-run effects that are, at an aggregate level, contrary to the findings of these papers.

Finally, this paper speaks to the literature that tests for multiple equilibria in the spatial distribution of economic activity by using war-time bombings as shocks to the local economy (Davis and Weinstein, 2002, 2008; Brakman *et al.*, 2004; Miguel and Roland, 2011). These papers analyze bombings of Japanese or German cities during WWII and bombings during the Vietnam War, respectively, but fail to find long-term effects on a range of outcome measures. The interpretation of my results stand in contrast to this line of research, which suggests the relative importance of human versus physical capital in the determination of the location of economic activity. This finding is in line with Waldinger (forthcoming), who shows the persistence of human capital shocks to universities before WWII, but not physical capital shocks during WWII, in the creation of scientific knowledge.

Section 2 of the paper describes the channel of persistence and derives empirical predictions. Section 3 gives an overview of the historical context of Austria and Section 4 introduces the empirical strategy, describes the data, and explores the plausibility of the identifying assumption. The empirical results are shown in Section 5, Section 6 presents robustness checks, while Section 7 inquires alternative mechanisms of persistence. Section 8 concludes.

2 Theoretical Considerations

This paper considers a new channel, how a historic event can lead to long-run consequences. For that matter, I describe the channel in this section and present empirical predictions. The mechanism describes in which way a short-term adjustment in labor allocations between sectors can generate a long-run difference through its effect on the path of structural change.

2.1 Setup

Assume an economy with three sectors (agriculture, manufacturing, and services) and two locations. Households have preferences over agricultural, manufacturing, and service sector goods such that an increase in income leads to structural change: the expenditure share on agriculture decreases while the expenditure share on services increases. Let households also have idiosyncratic location preferences (see Moretti (2011) for details), so that a wage differential between locations attracts additional households to the high-wage location.

All locations have access to the same production technologies. The labor allocation to each sector as well as the sector composition of each location is an equilibrium outcome. However, in the described setup the allocation of labor to a specific sector and location is, in general, not uniquely determined. With four assumptions, however, we can predict the effect of a temporary shock on the evolution of the local economies.

First, I assume that both locations have the same initial conditions. *Second*, let there be small sector switching costs for workers such that workers will only switch to another sector if they can avoid a wage reduction. *Third*, agriculture and manufacturing use a constant returns to scale production technology that uses labor and capital as inputs. The difference between these two sectors is that agriculture is more labor intensive than manufacturing.

Fourth, for the service sector I impose a different assumption: the size of the local service sector generates agglomeration economies for all sectors in that locations, but only in the long-run. This assumption is not entirely new to the literature as a number of papers assume increasing returns to scale in manufacturing and constant returns to scale in agriculture in two-sector models (see, for instance, Krugman (1991), Matsuyama (1992), Krugman and Venables (1995)). Desmet and Rossi-Hansberg (2009) provide evidence that benefits from agglomeration outweigh dispersion forces in the service sector, while in manufacturing the opposite is true. The authors argue that firms benefit from agglomeration after the introduction of a new general purpose technology (GPT). The last GPT in services (information and communication technology) was only introduced after 1970, while the last GPT in manufacturing (electricity) has been in use for almost 100 years already.

2.2 Adjustment to WWII Casualties

With the basic environment and the assumptions laid out, I now look at the adjustment and development of the local economies after a reduction of the population in one location due to WWII casualties. I will derive three predictions: (a) the short-term adjustment when agriculture and manufacturing are the dominant sectors; (b) the path of structural change in the local labor markets; and (c) the long-run differences due to the agglomeration economies generated by the service sector.

Without loss of generality, let there be a *high-casualty location* and a *low-casualty location.* The negative shock to the population of the high-casualty location increases the capital-labor ratio in that location and generates a comparative advantage in the capital-intensive manufacturing sector (Rybczynski, 1955). The high-casualty location therefore shifts labor into the manufacturing sector while the low-casualty location increases its labor allocation to the agricultural sector. Hence the first empirical prediction:

Prediction 1 (Short-run adjustment) *In the short-run, the high-casualty location will experience a drop in agricultural employment and an increase in manufacturing employment relative to the low-casualty location.*

Through the process of structural change labor reallocates from agriculture to the service sector, while the labor share in manufacturing remains relatively constant. The sector switching costs introduced in the second assumption imply that the low-casualty location, with the majority of the agricultural workers, experiences a larger transition of labor into the service sector.

Prediction 2 (Structural change) *Labor adjustments between sectors over the process of economic growth are such that the low-casualty location increases the number of workers in the service sector faster than the high-casualty location.*

Notice that Predictions 1 and 2 only mention differences in the labor allocation between sectors, but not in population size between locations. If there is no wage difference between locations, there is also no population movement towards one location. That, however, changes in the long-run, when agglomeration economies manifest itself. By the fourth assumption, a larger service sector in the low-casualty location increases location-specific total factor productivity and hence local wages. The increase in wages in that location will attract workers and hence leads to differences in economic activity, measured by local output, between locations. The next empirical prediction summarizes that insight.

Prediction 3 (Long-run adjustment) *The larger service sector in the low-casualty location generates agglomeration economies. Hence, wages in the low-casualty location exceed wages in the high-casualty location. As a consequence the population and economic activity in the low-casualty location increase relative to the high-casualty location.*

Population movement should not be taken literally in this section. Since Austrian municipalities are small geographic units, workers do not necessarily move to a municipality if they accept a job there. A more realistic option would be a redirection of commuting streams. A high-wage municipality should have higher local employment, ie. more workers operate in firms located within the borders of the municipality, but the residential population does not necessarily increase.

3 Historical Context

3.1 World War II

The annexation of Austria through Nazi-Germany in March 1938, generally referred to as *Anschluss*, meant the temporary disappearance of Austria as a nation state. When WWII broke out in the summer of 1939, Austrian forces were already fully integrated in the German army (Morawek and Neugebauer, 1989, p.43). In June 1938 compulsory military service was introduced and 95,000 Austrian men were drafted before the outbreak of WWII. Austrian soldiers were recruited into two regional divisions, which were employed in all major attacks throughout the war side by side with German soldiers. Over time, additional cohorts were drafted at less than yearly intervals (Morawek and Neugebauer, 1989, p.43f). Few cases of refusal are known among the Austrian army (Morawek and Neugebauer, 1989, p.49).

Over the course of the war, an estimated 1.2 million Austrian men were drafted, out of which over 250,000 died (Hagspiel, 1995, p.329). More than 100,000 returned with severe injuries, and estimates of Austrian prisoners of war are between 500,000 and 600,000 (Vocelka, 2010, p.110; Hagspiel, 1995, p.329). About 24,300 civilians died due to air raids (Bukey, 2000, p.227). Nevertheless, the population of Austria increased from 6.7 million to 7 million between 1938 and 1945, mostly due to increased birth rates before and during the first years of the war (Hagspiel, 1995, p.57f) and incoming refugees from the East (Stanek, 1985, p.17f).

3.2 Structural Change

The Austrian economy underwent fundamental changes during the inter- and post-war periods. The mechanization of agriculture and the economic crisis of the inter-war period led to a large number of unemployed workers. Only after the *Anschluss* of Austria to the German Empire did the economy recover. The rearmament of the German Empire and the resulting industrialization also dominated the Austrian economy in the following years. During WWII, the economy mostly followed the German pattern and unemployment became an unknown phenomenon (Thalmann, 1954).

After WWII, Austria was split into four different occupation zones for 10 years. The main problems of the immediate after-war period were food shortages and lack of intact physical capital. While the Soviet occupation forces dismantled remaining machines and extracted economic resources, the economy in the zones of the Western Allies benefited from the occupation forces and the introduction of the *Marshall Plan*. By 1949 the economy started to recover and production increased significantly (Thalmann, 1954, p.504). In 1955, when all four occupation forces left Austria, the boom was in full effect and the economy grew at an annual rate of about 2.5 percent for the following decades.

The most fundamental change in the post-war economy was the massive movement of labor out of agriculture and into the service sector. While almost 60 percent of the labor force were in agriculture in 1934, this number dropped to below 10 percent today. The sector share of services followed an opposite trend, while the manufacturing sector remained relatively stable with peak employment of just over 40 percent in the 1970s.

For this paper, the timing of WWII at the beginning of the large labor movements out of agriculture is of great advantage. The shock of WWII casualties is early enough in the process of structural change that I can study the effect of changes in the initial sector composition of the local labor market on long-term economic development. On the other hand, WWII is close enough to the onset of the large sectoral reallocation of labor that the initial shock does not peter out before the process of labor movement begins.

4 Empirical Strategy and Data

In this paper, I employ Austrian municipality-level data between 1900 and 2011. In this unique dataset, I combine data from 9 waves of population censuses that enables me to track the economic development of small geographic units over a century. For this research I also collected primary data on WWII casualties at the municipality level. In this section, I first present the empirical strategy and explain the measure of WWII casualties. Only then I describe the data as the specifics of the WWII casualty variable stem directly from the identification strategy. In the last part of the section I inquire if the identifying assumption is reasonable.

4.1 Empirical Strategy

I estimate a reduced-form relationship between the WWII casualty rate (as defined below) and measures of economic activity at a later point in time. In this way, I aim to estimate the long-run consequences of the loss of men in WWII on the local economy and uncover the mechanism at work.

The main estimating equation is:

$$Y_{ij}^t = \alpha_{1j} + \beta_1^t S_{ij}^{1934} + \mathbf{X}_{ij}^{1934}\gamma_1 + \varepsilon_{1ij}^t, \tag{1}$$

where Y_{ij}^t is the outcome variable in municipality i, district j, measured in year t, eg. local wages and the number of workers. S_{ij}^{1934} is the measure of WWII casualties as described below. X_{ij}^{1934} is a set of control variables measured in 1934, α_{1j} is a district fixed effect and ε_{1ij}^{t} is the error term. The effect of interest is β_1^t.

S_{ij}^{1934} is the share of military casualties in WWII of the male population in 1934, hereafter referred to as the *WWII casualty rate*. The variable is defined as:

$$S_{ij}^{1934} = \frac{\text{Military WWII casualties}_{ij}}{\text{Male population}_{ij}^{1934}}$$

Importantly, for this variable I only use military casualties. These soldiers either died in battle, as prisoners of war in a detention camp, of a disease, etc. Importantly, they did not die in their home town, but rather, as far away as the Soviet Union, France or Africa. Austrian men were drafted by birth cohort and employed alongside German soldiers in all major battles. Initially, men could get an exemption from military service if they could prove their importance in agriculture or industrial production. However, by the time the attack against the Soviet Union started in the summer of 1941, most exemptions were canceled and additional older and younger cohorts were recruited (Morawek and Neugebauer, 1989, p.46f). At that time, millions of prisoners of war had been brought into Germany and Austria to reduce labor shortages in agriculture and industrial production (Hagspiel, 1995, p.66f). Even though a later time of conscription meant a shorter period participating in the war, the probability of death did not necessarily go down (Overmans, 1999, p.244f). New conscripts had to replace fallen soldiers in high-intensity battles and the training period was significantly reduced for later conscripts. With the formation of the *Volkssturm* in September 1944 all men of age 16 to 60 years were drafted and deployed in defense against approaching Allied forces. Finally, in February 1945 the cohort of 1929 was drafted (Hagspiel, 1995, p.84f).

The variation in the WWII casualty rate comes from two sources: *(a)* sampling variance, that is the natural randomness in the mean share of war casualties, and *(b)* from a within-municipality correlation of location of employment. Anecdotal evidence reports that a birth cohort of a municipality was enlisted together and then assigned to various units so that soldiers knew some other members of their unit. This is consistent with the pattern of conscription described in Abramitzky *et al.* (2011).

Because soldiers were drafted based on birth cohorts and sent to the front irrespective of their home municipality, the probability of death of a soldier should be uncorrelated with municipality characteristics. This same argument is not true for all WWII casualties. Consider an Allied air raid with the goal to destroy a factory. The destruction of the factory claimed a certain number of civilian deaths. The total WWII casualties including the civilian deaths would then be correlated with the destruction of physical capital in a municipality.

But since soldiers were exposed to the risk of death far away from their home municipality, their passing should not depend on the events in their home municipality.

4.2 Data

Austrian Municipalities are the lowest administrative units and have an average population of about 3,500 people. However, municipality sizes vary greatly from 53 people in Gramais in the province of Tyrol to Vienna with a population of 1.7 million. Originally established in 1849 as an administrative unit to replace the feudal system, the municipalities have always been the government agencies with the most contact to citizens. The municipalities have an elected mayor and municipal council, which is elected every 5 or 6 years, depending on the province. The responsibilities of a municipal government include land use planning, energy and water supply, provision of schools, etc. (Bauer *et al.*, 1977, p.65f).

The data for this paper come mostly from two sources. The number of dead WWII combatants was collected from war memorials in each municipality. The outcome and control variables are from various publications of *Statistik Austria*, the federal statistical agency, and its predecessors.

WWII Casualties

In this paper WWII casualties are defined as the number of men who served in the military and did not return home after the war. To the best of my knowledge, a complete death registry for soldiers of Nazi-Germany is not available with the location of residence before the war. Overmans (1999) is the most comprehensive work on casualties of German forces during WWII and reports on massive problems in the official death registries.

Given the lack of available data on war casualties, I take another avenue to compile a dataset. Many municipalities have constructed a memorial to remember their dead and missing soldiers. Most of them list the names of the dead and sometimes also date/location of birth and/or death. I use the number of names from these war memorials for the casualty data. Various websites for genealogist provide photographs and transcripts of memorials from many places in Austria.[2]

Two issues arise from this approach. First, I need to ensure that civilian casualties are not included on the memorials. The memorials are usually divided in fallen or dead, missing and sometimes civilian casualties. I never include the civilian casualties and do not use the data from a memorial if there is a female name among the list of dead, as this indicates that civilian casualties are included. The war memorials should be a complete list of all dead

[2]The websites I used are `www.denkmalprojekt.org` and `www.kriegerdenkmal.co.at`

and missing soldiers from a municipality, because relatives of non-returning soldiers had an interest in ensuring that their fathers, sons, and husbands were honored on the memorial. Often names were added to the end of the list, when missing soldiers did not return from war captivity after a number of years. In other cases names were erased when the person unexpectedly returned.[3]

The second issue with these data is how to link a war memorial to a municipality and avoid double counting. Austrian municipalities are subdivided into one or more localities, which can range from a cluster of houses to a city. Localities often were independent municipalities but have been merged earlier. In most cases, the largest locality has the same name as the municipality, so it is not entirely clear if a memorial addresses the municipality or the locality. There are also memorials that refer to the area that belongs to a rectorate of the church. To reduce measurement error in the WW casualty rate variable, I impose three restrictions. First, I restrict attention to municipalities whose borders have not changed since 1934, since I do not know if the war memorial corresponds to the old borders of a municipality or the one after the border change. Second, I drop municipalities with city status in 1945 since those often have several memorials with a different number of names on it. Finally, I focus on municipalities that have only one locality to ensure that the casualty data correspond to the whole municipality. These restrictions reduce the number of potential municipalities to 528. Out of the potential sample of municipalities, I managed to collect WWII casualty data for 314 or almost 60 percent.

Panel A of Table 1 describes the war casualty data. An average of 11.7 percent of the male population in 1934 died as soldiers during WWII, with a standard deviation of 3 percent. The total number of deaths ranges between 6 and 483, with a mean of 69.3 soldiers. The population size of the sample municipalities is on average 1,180 in the year 1934 and has grown to 2,200 people by 2011.

The lack of official and accurate data on military war casualties makes it likely that the explanatory variable includes measurement error. As commonly know, as long as the measurement error is uncorrelated with the error term, the estimate of β_1^t suffers from attenuation bias and I estimate a lower bound of the true effect.

Pre-War Municipality Characteristics

Control variables of pre-war municipality characteristics are mostly drawn from the printed version of the census in 1934. The census in 1934 is the only reliable source of municipality data in the inter-war period as the censuses of 1920 and 1923 were not even fully analyzed and published due to the inferior quality of the recorded data. The census of 1939 conducted

[3] In one instance a note was added to a name when the person returned in 1958 from imprisonment.

in the entire German Empire is of good quality, but used a different principle to record the residential population. In that census, drafted soldiers were accounted to the municipality of service and not to their home municipality. Since the affected young men are central to this paper, it is important that they are accounted to the residential population, to which eventually they fail to return.

The vector X_{ij}^{1934} in equation (1) include the log of the residential population, the share of the population in agriculture, and the share of Jews in the population, all measured in 1934. In addition, I control for the market status of a municipality in 1945, the first year on record of this variable. To account for the political attitude in a municipality before the war, I include the vote share of the Social Democrats and the NSDAP in the 1930 general elections, which were the last free elections before the war. Since the geography of a municipality could influence its post-war development, I also control for time-invariant factors such as the mean slope, the mean elevation, and its interaction. Panel B of Table 1 shows the descriptive statistics of these control variables. The share of Jews and the vote share of the NSDAP are on average low values, but have a few outliers.

Economic Outcomes

Panels C and D of Table 1 report the main outcome variables. Panel C lists the share of workers in agriculture, manufacturing, and services for 1951, 1981, and 2011. These three points in time give a good overview of the evolution of sector shares in the sample municipalities. The share of workers in agriculture decreased constantly, while the share in services increased. The manufacturing share followed a hump-shaped pattern. The data come from the decennial population censuses of 1951 to 2011.

The other long-term economic outcomes in Panel D should represent measures of economic activity of a municipality. The total wage bill in 2011 is the sum of all wages and salaries paid by all firms located within a municipality in 2011. The data come from tax reports of municipalities, as they levy a tax of 3 percent on all wages and salaries paid by firms (*Kommunalsteuer*). I interpret this measure as a proxy of local GDP.

The number of local workers is another measure of economic activity, as it counts the workforce that is actually employed within the borders of a municipality. This measure differs from the working population of a municipality insofar that it also includes in-commuters, but subtracts out-commuters of a municipality. The measure has the advantage that it can also be divided into sectors and measures the size of a sector. The number of firms comes from the firm censuses, which were conducted in 1973 and every 10 years since 1981. These numbers are complementary to the measure of local workers as the number of local workers in a municipality could be high due to more firms or due to larger firms.

Local wages are drawn from the *Austrian Social Security Database* (ASSD).[4] The database contains all employment relationships in Austria since 1972 and records some worker and firm information as well as annualized earnings for each employment spell. I construct a local wage variable to measure the mean wage in a specific municipality from all workers aged 25 to 60 years. Because the wage information comes from social security data, the variable is top-coded. However, this restriction binds for few observations only and the mean wage of a municipality changes little if estimated by a Tobit model. For efficiency reasons, I weight the regressions with the mean wage as the dependent variable by the number of workers out of which the mean wage is estimated.

4.3 WWII Casualties as Random Events

The identification in this paper rests on the assumption that WWII casualties are as good as random. This is admittedly a strong assumption. I now explore if this assumption is reasonable. To this end I show that the WWII casualty rate is uncorrelated with observed pre-war municipality characteristics, the education level of the local population, and pre-war economic development. As Abramitzky *et al.* (2011) already show for WWI casualties in French regions, the WWII casualty rate in Austrian municipalities does not appear to depend on observed or unobserved factors of an area.

The estimation of β_1^t in equation (1) relies on the presumption, that the WWII casualty rate is uncorrelated with any uncontrolled factors influencing post-war municipality development. For sake of the argument, let there be an unobserved factor Z_{ij} that is correlated with, both, S_{ij}^{1934} and ε_{1ij}^t. This would lead to omitted variable bias such that $E[\hat{\beta}_1^t] \neq \beta_1^t$. In general, it is untestable if such a factor Z_{ij} exists since it is unobserved. In two cases, however, we can get evidence that there is no such omitted factor Z_{ij}.

First, if the unobserved factor Z_{ij} is correlated with an observed pre-war municipality characteristic X_{it}^{1934}, then there should also be a correlation between the observed pre-war municipality characteristics X_{ij}^{1934} and the WWII casualty rate S_{ij}^{1934}. When I estimate equation (2),

$$S_{ij}^{1934} \quad = \quad \alpha_{2j} + X_{ij}^{1934}\gamma_2 + \varepsilon_{2ij}, \qquad (2)$$

and $\gamma_2 = 0$, then this is strong evidence against the existence of such a factor Z_{ij}.

For instance, assume that highly educated soldiers were employed in offices far away from the battle fields and less educated soldiers were instead sent to the front line. Of course a municipality with a more educated population would then suffer fewer WWII casualties and

[4] Zweimüller *et al.* (2009) describe the dataset in detail.

likely perform better economically after WWII. The proposed check exploits that the education level of the population is likely correlated with observed municipality characteristics like the size of the agricultural sector or the urbanization of the municipality. A missing correlation between the size of the agricultural sector and the WWII casualty rate is one piece of evidence that WWII casualties are not driven by observed and unobserved factors.

Second, if the unobserved factor Z_{ij} is not only correlated with the post-war error term, ε^t_{1ij}, but also with the pre-war counterpart, $\varepsilon^{pre-war}_{1ij}$ in equation (1), then S^{1934}_{ij} should also be correlated with $\varepsilon^{pre-war}_{1ij}$ and hence $Y^{pre-war}_{ij}$. I therefore estimate equation (1) with economic outcome variables measured in 1900 on the left hand side. If $\beta^t_1 = 0$, then it is unlikely that an unobserved factor Z_{ij} drives WWII casualties and post-WWII economic development, since it does not drive pre-WWII development.

Let me continuing with the previous example. If there is no correlation between the education level of the population and pre-war municipality characteristics like the size of the agricultural sector, then the earlier check would not find any evidence of an endogeneity problem. However, if the education level is important for *post-WWII* economic development, it should also have been important for *pre-WWII* economic development. The second test checks exactly that. If any economic variable measured in 1900 is significantly correlated with the WWII casualty rate, then there is likely an unobserved factor at play, since in this setting I can rule out a causal relationship.

I now use the available data to perform these two checks of the identifying assumption. In particular, I employ the check for pre-war municipality characteristics, the mean education level of a municipality, and economic variables measured in 1900.

Pre-War Municipality Characteristics

For visual inspection of the relationship between WWII casualties and pre-war municipality characteristics, a graph of correlations of the WWII casualty rate with all control variables is shown in Figure A.2. In line with the argument that the WWII casualty rate is not affected by social, geographic, or economic features of a municipality, there is no clear relationship between the WWII casualty rate and any other variable.

For the first check described above, I estimate equation (2). Although a correlation of the WWII casualty rate, S^{1934}_{ij}, with the set of observable control variables X^{1934}_{ij} does not bias the estimate of β^t_1 in equation (1) because X^{1934}_{ij} is included in the regression, the results are informative whether the WWII casualty rate is actually random. If the WWII casualty rate would be significantly correlated with observed pre-war municipality characteristics, one cannot rule out an omitted factor that drives the results.

Columns (I)-(VI) of Table 2 show the results of regressions of the WWII casualty rate on each set of observable municipality characteristics separately. District fixed effects are always included. Only the share of Jews in 1934 is separately statistically significant at the 10 percent level. In column (VII) I include all explanatory variables together. The significant relationship with the share of Jews in 1934 disappears, but the vote share of the NSDAP, the party of Hitler, in the general elections in 1930 is significant at the 5 percent level. However, this result is entirely driven by one outlying observation with a vote share of the NSDAP of about 25 percent. Excluding that observation decreases the point estimate of the NSDAP vote share by about two thirds, as shown in column (VIII).

A F-test of the estimates of all observable municipality characteristics being jointly zero in column (VII) results in a p-value of 0.197 and increases to 0.503 when the outlying observation is dropped in column (VIII). The general picture of the relationships in Table 2 suggests that WWII casualties are generally uncorrelated with pre-war municipality characteristics. This also implies that no unobserved factors that are correlated with the observed pre-WWII municipality characteristics drive the main results.

Education

Another possible source of endogeneity that I can not rule out entirely works through education. Educated soldiers might have been assigned to less lethal tasks in the armed forces and hence had a lower probability of death. That, of course, would reduce the WWII casualty rate in more educated municipalities. Since an educated workforce is a good predictor of the local income level (Gennaioli *et al.*, 2013, 2014), all post-war results could be biased. Unfortunately, no data on education is available at the municipality level before WWII.

To tackle this issue I use the post-WWII data on the education level of a sub-group of the population that finished its formal education before the outbreak of WWII. The first age-specific education data comes from the population census in 1971. The age-group of 55 to 64 years (birth cohorts 1907 to 1916) had likely finished their education by 1939.

The correlation between the WWII casualty rate and the education level of that age group is shown in Table 3. All the estimates are close to zero and statistically insignificant, suggesting that the education level of a soldier did not influence the probability of death. However, this evidence can only be suggestive since *(a)* the home municipality before WWII need not be the same as the location of residence after the war, and *(b)* if the probability of death of a soldier depends on his education level, the age-group observed in 1971 would be selected based on the education level. If one expects that a higher education level reduces the probability of death, the estimated effect of the education level of the surviving population on the WWII casualty rate is slightly biased towards zero because the highly educated would be over-represented in 1971 compared to the pre-war situation.

Pre-War Economic Development

For the second check if WWII casualties are correlated with an unobserved factor, I look at pre-war economic development. If an unobserved municipality feature drives post-war development, it is unlikely that this characteristics just emerged right around WWII or gained importance only in the post-war period. The effect of the unobserved characteristic should therefore be measurable already before WWII. For this purpose I preform a falsification test to check if the pre-war development is correlated with the WWII casualty rate. Of course such an effect can not be causal, but must stem from omitted variable bias, which then likely also drives development after WWII.

Table 4 reports the results on a number of available outcome measures. Column (I) uses the log population in 1900 as a proxy of the economic situation at that time, but fails to find a significant effect. For all other outcomes the size of the dataset is reduced because the province of Burgenland in the East was not part of Austria at that time and corresponding census data are not available. Columns (II)-(V) use the livestock of horses, cattle, pigs, and sheep, respectively, in 1900 as a proxy for agricultural productivity. None of the specifications show a correlation with the WWII casualty rate. In the last two columns the number of large land holdings and the number of factories in the municipality are used as outcome variables. For all these economic outcome variables I fail to find a correlation with the WWII casualty rate.

From the tests in this section I conclude that there is no evidence of an omitted variable that drives the WWII casualty rate and also affects the local economy. In none of the checks there is any sign that the WWII casualty rate is systematically affected by a municipality-specific factor. Hence, any correlation of the WWII casualty rate and post-war development is likely a causal relationship.

5 Results

5.1 Short-Run Results

The short-term prediction of the theoretical considerations in Section 2 is a switch of workers out of agriculture into manufacturing due to WWII casualties (Prediction 1). Since agriculture is the labor-intensive sector compared to manufacturing, the reduction of the labor force in high-casualty municipalities generates a comparative advantage in manufacturing.

The first post-war statistics recording the size of the agricultural sector at the municipality level are from 1951. I use data from the population census and the agricultural census to test the effect of WWII casualties on the size of the agricultural sector.

The regression results are presented in Table 5. Column (I) shows that WWII casualties reduced the share of workers in the agricultural sector in 1951 by 0.49 percentage points. Column (II) shows the same effect, where the difference in log population in agriculture between 1951 and 1934 is the dependent variable. While the mean number of people in agriculture dropped by 18 percent, a one percentage point higher WWII casualty rate increased that number by another percentage point. The agricultural census of 1960 split up the number of agricultural workers into family-internal and -external. The reduction in both groups is significantly different from zero.

Note that it is highly unlikely that reverse causality drives this result. One could argue that agricultural workers were less likely to be killed in battle because they were more comfortable with being outside all the time and possibly had a better skill set for survival in a war. But then we should find the same negative correlation between WWII casualties and the share of the agricultural sector before the war. However, Table 2 actually reports an insignificant positive correlation between the 1934 agricultural share and the WWII casualty rate. That correlation remains positive and insignificant even when I include the full set of control variables in column (VII). It is therefore unlikely that reverse causality drives the result, because then we would see a consistent pattern in the correlations between these variables.

5.2 Sector Adjustment

The suggested channel of persistence in this paper is the effect of WWII casualties on the evolution of sector shares in Austrian municipalities. Does the short-run effect of a smaller agricultural sector fade out quickly or does it lead to a different development path of labor allocations across sectors? For an answer to this question I estimate the effect of WWII casualties on the shares of labor in agriculture, manufacturing, and services using data from the decennial censuses of 1951–2011.

Table 6 shows the estimated effect of the WWII casualty rate on the sector shares in agriculture (Panel A), manufacturing (Panel B), and services (Panel C) for 1951–2011. Each estimate is the result of a separate regression.[5] The reduction in the agricultural share described in the previous section can be traced until the census of 1971, but with reduced significance. After that there is no significant or meaningful effect of WWII casualties on the share of workers in agriculture. The fading out of this effect is not surprising because *(a)* the overall share of workers in that sector drops from 52 percent in 1951 to only 5 percent

[5] Although sector shares are correlated between sectors, a "Seemingly Unrelated Regressions" (SUR) model would not lead to efficiency gains as the regressors are the same in each equation and hence SUR is equal to OLS (Greene, 2008, p.257f).

in 2011, and *(b)* Prediction 2 implies that the difference in agricultural workers transfers to a difference in service sector workers.

The short-run increase in the manufacturing sector share due to WWII casualties remains surprisingly stable with only a small drop in 2011. This finding is consistent with the theoretical part in Section 2, where the relatively stable aggregate labor input in the manufacturing sector does not require adjustments over time. The stable size of the manufacturing sector is also visible in the summary statistic, where the sector shares range consistently around 30 to 40 percent.

An assumption in the discussion of the mechanism in Section 2 is that sector switches due to structural change take place between the agricultural and the service sector. So over time the negative effect of WWII casualties on the agricultural sector should transfer to a negative effect on the service sector (Prediction 2). This is exactly what the data shows in Panel C. Since the census in 1981, WWII casualties have a significant negative effect on the size of the service sector.

The effect of WWII casualties on sector shares is also economically meaningful as a one percentage point increase in the WWII casualty rate increases the share in manufacturing in 1971 by 0.6 percentage points, which is the largest estimated effect in this table. Even 60 years after the end of WWII, the local labor market is shaped by men who did not return from the war.

5.3 Long-Run Results

Of course the big question remains: Do WWII casualties matter for economic development in the long-run? In this section I look at indicators of economic activity more than 60 years after the end of WWII. As we will see, in the long-run WWII casualties matter a lot.

Total Wage Bill

Local economic activity can be approximated by several different variables. The total wage bill in 2011 measures the sum of all salaries and wages paid out by firms within the border of a municipality. I interpret this measure as a proxy for local GDP.

The results in Table 7 clearly show that WWII casualties reduce economic activity in the long-run. Column (I) reports a reduction of the total wage bill in 2011 by about 7 percent if the WWII casualty rate increases by one percentage point without any control variables or district fixed effects. Column (II) includes the full set of of control variables and the estimated effect decreases to 4.8 percent. The preferred specification with the full set of control variables and district fixed effects in column (III) shows a reduction of the total wage bill by about 5 percent. The inclusion of pre-war control variables and controlling for unobserved district characteristics does not significantly change results, which suggests little correlation of the WWII casualty rate with cofounding factors.

A simple explanation for the observed pattern is explored in column (IV). A municipality with a high WWII casualty rate had a drop in its population and slower population growth over time. Since fewer people also generate less economic activity, the result would be trivial. In column (IV) I include the population in 2011 as a control variable instead of the population in 1934 to rule out this channel of persistence. Since the effect of WWII casualties is left almost unchanged and is even significant at the 1 percent level, I rule out that a reduction of the contemporary population is the reason for the reduction in economic activity.

As a direct results of WWII, Austria was occupied by the the Allied forces for 10 years. Eder and Halla (2015) present evidence that the economic development over the entire post-WWII period was different in the former Soviet occupation zone than in the zones of the Western Allies. It is therefore natural to inquire if the long-run effect of WWII casualties was different in the former Soviet occupation zone. Column (V) includes an interaction term of the WWII casualty rate and a Soviet zone indicator. The insignificant estimate of the interaction term suggests that the effect of the WWII casualty rate is not different in the former Soviet occupation zone.

The magnitude of the estimated effect is remarkable, as a one percentage point increase in the WWII casualty rate reduces the total wage bill of a municipality by about 5 percent. A reasonable explanation is that the effect built up over time through agglomeration economies. If firms are more productive in an area, they grow over time and additional firms open up. This attracts more workers and increases local economic activity. Eder and Halla (2015) also use data from Austrian municipalities and show that a population shock can lead to an even larger difference in economic activity 50 years later. Hence the relatively small short-run shock can plausibly have generated the large long-run effect through agglomeration economies.

Intensive Margin: Local Wages

The total wage bill can be lower in high-casualty municipalities for two reasons: workers get paid less or fewer employed workers. With data from the *Austrian Social Security Database* (ASSD) I now investigate the first reason. The outcome variable is the log of mean annualized wages paid out by firms within a municipality to workers of age 25–60.

In Panel A of Table 8 the effect of WWII casualties on mean municipality wages are shown for 10-year intervals since the 1970s. WWII casualties reduced wages significantly at least since 1981. The negative effect of -1.4 percent in 2011 is large in economic terms:

the average worker in a municipality with a WWII casualty rate of one standard deviation below the mean earns about 8.4 percent less than the average worker in a municipality with a WWII casualty rate of one standard deviation above the mean.

The effect of WWII casualties on different subgroups of workers are shown in Panel B of Table 8 for 2011. The effect is quite similar for males and females. The effect on local wages in agriculture in column (III) is not very meaningful, since there are few workers in an employment relationship in that sector. Only 187 of the 314 municipalities in the sample have at least one employed worker in agriculture. Hence it is not surprising, that there is no significant effect of WWII casualties on wages in that sector. Both, the manufacturing and service sector show a significant negative effect of similar size due to WWII casualties.

However, these numbers do not imply that the average worker in a high-casualty municipality is comparable to the average worker in a low-casualty municipality. The demographic information in the ASSD allow me to rule out that differences in the age structure, sex ratio, foreigners, and education level of the workers generate the effect on local wages. Although an effect of WWII casualties on wages that works through educational attainment would also be causal, in this way I can eliminate this channel of persistence.[6]

Table B.1 in the Appendix shows the effect of the WWII casualty rate of the municipality of employment on wages at the individual level. Controlling for personal characteristics like age, gender, education level, foreigner status, and a set of industry and job type indicators does not change the estimated effect much. This suggests that the effect of WWII casualties on today's economic performance does not run through an effect on the composition of the workforce like educational attainment.

Extensive Margin: Local Employment

The second reason for a higher local wage bill in low-casualty municipalities is that there are more workers employed. I test this with two different outcome measures: the number of local workers[7] and the number of firms. The two outcome measures also enable me to explore the difference in economic activity at the sector level.

In Panel A of Table 9, the number of local workers is regressed on the WWII casualty rate, the usual municipality control variables, and district fixed effects. The effect on the total number of workers is -2.8 percent and is significantly different from zero. Columns (II) and (III) show no effect of WWII casualties on the number of workers in agriculture or manufacturing. The full difference in the number of local workers stems from the service

[6]For a detailed discussion of this potential mechanism, see Section 7.

[7]Local workers are all people who are employed in a municipality, irrespective of their municipality of residence.

sector as column (IV) shows. A one percentage point increase of the WWII casualty rate reduces the number of workers in the service sector by 3.3 percent.

In Panel B, I use the number of firms as the outcome measure and find a very similar pattern. The total number of firms is lower in high-casualty municipalities, but to a smaller extent than the number of local workers. Again the service sector is responsible for the overall difference, while the effect on the number of agricultural and manufacturing firms is statistically insignificant.

I conclude that the reduction in economic activity due to WWII casualties is robust to the outcome measure used. The total wage bill shows a large difference, which comes to about one third from a difference in the wage level and about two thirds from the number of workers. More specifically, the effect seems to be driven by differences in the size of the service sector, which is consistent with the theoretical considerations.

6 Robustness Checks

6.1 Neighboring Municipalities

I now present an additional check to support the causal interpretation of the estimated effect of WWII casualties. The robustness checks exploits the fact that neighboring municipalities share a large degree of possible confounding factors of economic development.

The credibility of the estimated effect of WWII casualties on long-run economic outcomes depends crucially on the identifying assumption that unobserved pre-war municipality characteristics are uncorrelated with the WWII casualty rate. As this assumption is not directly testable, an additional robustness check should underline that the results are not driven by endogenous factors.

For this purpose, I group municipalities into pairs that share a common municipality border and estimate the effect of WWII casualties within this pair. Since neighboring municipalities share many geographic features and enjoy the influence of external factors, such as the proximity to a city, in a similar way, a possible endogeneity problem should matter less in such a pair. The disadvantage of this approach is that the sample size decreases as I need to find pairs of neighboring municipalities for which I have data on WWII casualties. I then run a regression of the outcome variable on the WWII casualty rate, the usual control variables and a pair fixed effect for each municipality pair. These pair fixed effects account for all unobserved factors that matter for both municipalities in the same way and hence result in a high degree of internal validity of the findings.

The effect of WWII casualties on the total wage bill in 2011 is shown in Table 10. The results are almost identical to the comparable specification in column (III) in Table 7. In column (II) I restrict the sample to bordering municipalities of the same province (Sample I), in column (III) to neighboring municipalities of the same district (Sample II), and in column (IV) to municipality pairs with the same market status in 1945 (Sample III). The estimated effect does not change much in any of these samples.

Table B.2 in the Appendix shows the same robustness exercise for the local wage level. The sample of all neighboring municipalities in column (I) shows an almost identical effect than the preferred specification in Table 8 of -1.3 percent. The restriction to municipality pairs of the same province (column (II)) or the same district (column (III)) does not change the result much. The estimated effect drops to -0.8 percent if only municipalities of the same market status in 1945 are used, but it remains significant at the 5 percent level.

This evidence suggests that unobserved external factors or geographic features are not correlated with the WWII casualty rate and hence do not drive the main results of the paper.

6.2 Age Structure

A serious concern about the results presented in this paper is that the variation in the WWII casualty rate originates in a difference in the age structure of the municipality population. Large cohorts of young men led to more drafts and more casualties relative to the total male population, even if the same share of drafted men did not return from the war. Of course the demography of a municipality population can have an effect on the post-war economic development, which would cause a serious omitted variable bias.

The preferred method to rule out an effect of the age structure of the population on economic development would be to control for the pre-war demographic composition of the municipality population or check the robustness of the results if the WWII casualty rate would be defined over the drafted men only. But the census of 1934 does not contain any age specific data to be used. Only in the census of 1939 and later some variables on the relative size of age groups are available. Finally, in the census of 1971 the population is divided into gender-specific 5-year age groups. As mentioned before, the census of 1939 does not account drafted soldiers to their home municipalities but to the municipality of their assigned barrack. The post-WWII censuses have the problem that they were, of course, affected by WWII casualties.

Notwithstanding these drawbacks, I use the age group variables to check the robustness of the main results when the share of certain age groups of the total municipality population are included in the regressions. If the high WWII casualty rate is only due to a large cohort of drafted men, then the estimated effect of the WWII casualties should shrink significantly if the size of the remaining age group after WWII is included in the specification. Table 11 reproduces the effects of WWII casualties on the size of the agricultural sector in 1951 (Panel A), the total wage bill in 2011 (Panel B), and mean wages in 2011 (Panel C) in column (I) for reference. In column (II) and (III) I include the share of the population of age 0 to 14 years, 14 to 18 years, and 18 to 65 years in 1951 and 1961, respectively. Column (IV) includes a set of control variables measuring the relative size of 5-year age groups of the male population in 1971.

In none of the specifications the estimated effect deviates much from the reference estimate in column (I), which suggests that the size of various cohorts either do not matter for economic development or it is not the driving force of the WWII casualty rate. In either case, it seems that the pre-war age structure of a municipality does not drive the results of the paper.

7 Alternative Mechanisms

I now discuss some potential mechanisms through which the WWII casualty rate could alternatively drive the results of the paper. However, the evidence presented here shows that these mechanisms were of second order and do not change the main results much. The potential mechanisms concern the education, fertility, and migration.

7.1 Education

Gennaioli *et al.* (2013, 2014) show that differences in human capital are the main explanatory factors of differences in income levels between regions of a country. The effect of human capital greatly outweighs differences in geography, institutions, and culture in these papers. It is therefore natural to inquire if the persistent effect of WWII casualties operates through a human capital channel. While Austrian municipalities feature minimal differences in geography, institutions, and culture, especially in within-district estimates, differences in educational attainment in the 1950s and 1960s could have set in motion a process of regional divergence.

A possible channel could be that children whose fathers did not return home after the war could not get higher education because of financial constraints. The consequently better educated population of low-casualty municipalities was able to establish firms and make better use of economic opportunities. A difference of the average education level could therefore be the link between WWII casualties and the level of economic activity today.

The highest level of education was first recorded in the population census of 1971. I use these data to construct variables measuring the educational attainment and regress these on the WWII casualty rate. Table B.3 features the mean years of schooling as the dependent variable in Panel A, while Panel B uses the share of the population which only finished mandatory education. In none of these regressions there is a significant effect of WWII casualties on the educational outcome variable. All point estimates are small compared to the standard errors. Table B.4 shows that there is also no significant difference in the educational attainment in the age group of 20 to 34 years measured in 1971. That age group (birth cohorts between 1937 and 1951) have completed almost their entire education after WWII and should therefore give a clear picture if the educational attainment differed in high- vs. low-casualty municipalities.

To conclude, a difference in formal education does not seem to be the mechanism behind the long-run results of this paper. At least since the 1970s, there is no significant effect of WWII casualties on the education level of the residential population.

7.2 Fertility and Migration

The interruption in the gender-specific age distribution due to WWII casualties might also had an effect on the fertility of the local population that might influenced the local economy in the long-run. The birth cohorts with the highest casualty rates for all of Nazi-Germany were 1914–1924 (Overmans, 1999, p.234). These cohorts would have been between 21 and 29 years old at the end of the war and would likely have started a family in the immediate post-war period.

I explore this line of argument by examining the relationship between WWII casualties and population change (net birth and net migration) in the period 1961–1971 and the share of children in the population in the three population censuses following WWII. Table B.5 in the Appendix shows that the WWII casualty rate did not have a significant effect on the total population change between 1961 and 1971 (column I), net births (column II), or net migration (column III). The share of children (age 0–14 years) in 1951, 1961, and 1971 was also not significantly affected by war casualties as columns (IV)-(VI) show.

The results suggest that the local fertility pattern was not influenced by war casualties. The young population in neighboring municipalities seem to have had enough social interaction so that a local shock to a certain age-group of males could be absorbed.

Another line of argument for the estimated results could be that the population losses due to WWII in a municipality were absorbed by foreign refugees. If foreigners were less successful in generating economic activity, the long-term results found in this paper could also stem from this channel. This is especially relevant as there was a large inflow of refugees from the East into Austria in the years following WWII.

In Table B.6, I regress the share of foreigners of the total population in 1951, 1971, and 2001 on the WWII casualty rate, including the usual control variables and district fixed effects. There is no evidence of a concentration of foreigners in high-casualty municipalities.

8 Conclusion

This paper exploits military WWII casualties as a plausibly exogenous shock to local labor markets. In the short-run, high-casualty municipalities experience a shift of labor from agriculture towards manufacturing. Over the next 40 years, this initial difference in the sector labor allocation transforms into a lower share of workers in the service sector through the process of structural change. In the long-run, economic activity, measured by the total wage bill, is concentrated in municipalities with relatively few WWII casualties. This effect is large as a one percentage point increase in WWII casualties reduces the total wage bill in 2011 by almost 5 percent. The output reduction originates in, both, lower local wages and fewer workers.

The main contribution of the paper is the identification of a differential path of structural change as a, to the best of my knowledge, new mechanism through which past events can shape today's economic landscape. A temporary shock that alters the initial sector composition of the labor market can cause large long-term differences in the spatial distribution of economic activity. This, however, also implies that the temporary shock has to happen during a phase of structural change.

An important limitation to the results in this paper is that the sample consists of small and rural municipalities. These municipalities are small geographic areas and often consist of only one thousand people. Future work needs to show if the identified channel of persistence is also important in cities or larger forms of aggregation.

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Tables

Table 1: Summary Statistics of Main Variables

Variable	Obs	Mean	Std. Dev.	Min	Max
PANEL A: WWII casualties					
% WWII casualties of 1934 male pop.	314	0.117	0.030	0.052	0.220
WWII casualties	314	69.318	59.521	6	483
Population in 1934	314	1,180.057	983.179	73	8,733
Population in 2011	314	2,200.873	2,401.980	53	21,181
PANEL B: Control variables					
Log population in 1934	314	6.770	0.802	4.29	9.075
% population in agriculture in 1934	314	0.571	0.248	0.008	0.971
Market status in 1945 (dummy)	314	0.178	0.383	0	1
Log elevation	314	6.531	0.840	4.761	7.762
Log slope	314	2.322	1.221	−1.122	3.583
Log elevation × log slope	314	16.096	9.020	−5.347	27.172
% Jewish population in 1934	314	0.001	0.007	0	0.117
% Social Democrats in 1930	314	0.202	0.193	0	0.710
% NSDAP in 1930	314	0.013	0.027	0	0.247
PANEL C: Sector shares					
% workers in agriculture in 1951	314	0.520	0.241	0.012	0.974
% workers in agriculture in 1981	314	0.120	0.113	0.004	0.603
% workers in agriculture in 2011	314	0.052	0.049	0.004	0.360
% workers in manufacturing in 1951	314	0.328	0.179	0.026	0.869
% workers in manufacturing in 1981	314	0.413	0.131	0.074	0.730
% workers in manufacturing in 2011	314	0.268	0.076	0.045	0.468
% workers in services in 1951	314	0.152	0.093	0	0.518
% workers in services in 1981	314	0.467	0.130	0.155	0.881
% workers in services in 2011	314	0.680	0.086	0.390	0.898
PANEL D: Economic outcomes					
Log total wage bill in 2011	314	8.809	1.542	4.200	12.140
Log mean yearly wage in 2011	312	10.051	0.239	9.228	10.580
Log local workers in 2011	314	5.961	1.229	2.079	9.191
Log local workers in agriculture in 2011	314	3.404	0.918	0	5.914
Log local workers in manufact. in 2011	314	4.526	1.620	0	8.034
Log local workers in services in 2011	314	5.556	1.302	1.609	8.892
Log number of firms in 2011	314	4.821	0.900	1.609	7.330
Log number of firms in agricult. in 2011	314	2.976	0.867	0	5.394
Log number of firms in manufact. in 2011	314	2.715	1.054	0	5.733
Log number of firms in services in 2011	314	4.360	1.049	0.693	7.061

Table 2: Pre-War Municipality Characteristics and WWII Casualties

	WWII casualty rate							
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
Log population in 1934	−0.004						−0.003	−0.002
	(0.003)						(0.004)	(0.004)
% pop. in agriculture in 1934		0.013					0.016	0.013
		(0.008)					(0.014)	(0.014)
Market status in 1945 (dummy)			−0.005				−0.004	−0.002
			(0.005)				(0.006)	(0.006)
Log elevation				0.003			0.006	0.006
				(0.018)			(0.018)	(0.018)
Log slope				0.014			0.021	0.020
				(0.021)			(0.022)	(0.022)
Log elevation × log slope				−0.002			−0.004	−0.004
				(0.004)			(0.004)	(0.004)
% Jewish pop. in 1934					−0.458*		−0.353	−0.372
					(0.237)		(0.243)	(0.242)
% Social Democrats in 1930						−0.016	0.005	0.004
						(0.013)	(0.018)	(0.018)
% NSDAP in 1930						0.132	0.186**	0.064
						(0.089)	(0.094)	(0.121)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
NSDAP outlier excluded								Yes
No. observations	314	314	314	314	314	314	314	313
R-squared	0.24	0.24	0.23	0.23	0.24	0.24	0.27	0.25
Mean of dep. var.	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12
S.d. of dep. var.	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03
P-value	0.104	0.109	0.328	0.824	0.054	0.180	0.197	0.503

This table summarizes estimation results based on municipality-level data. The dependent variable is equal to military WWII casualties divided by the male municipality population in 1934. The control variables in each specification are listed. In addition, district fixed effects are included. The method of estimation is least squares. Robust standard errors are in parentheses below. The row "P-value" reports the p-values of a two-sided hypothesis test of the estimates of all listed control variables being jointly zero. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table 3: Pre-War Education and WWII Casualties

	WWII casualty rate					
	(I)	(II)	(III)	(IV)	(V)	(VI)
Independent variable:						
% of age group 55–64 in 1971 with …						
… only mandatory education	0.019	0.001				
	(0.020)	(0.026)				
… apprenticeship			−0.011	0.020		
			(0.030)	(0.038)		
… secondary schooling					−0.055	−0.030
					(0.041)	(0.046)
Control variables		Yes		Yes		Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes
No. observations	314	314	314	314	314	314
R-squared	0.23	0.27	0.23	0.27	0.24	0.27
Mean of dep. var.	0.12	0.12	0.12	0.12	0.12	0.12
S.d. of dep. var.	0.03	0.03	0.03	0.03	0.03	0.03

This table summarizes estimation results based on municipality-level data. The dependent variable is equal to military WWII casualties divided by the male municipality population in 1934. The education variables of the age group 55–64 is measured in 1971. The control variables (if included) in each specification are listed in Panel B of Table 1. In addition, district fixed effects are included. The method of estimation is least squares. Robust standard errors are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table 4: Pre-War Development and WWII Casualties

	Log Population in 1900	Log Horses in 1900	Log Cattle in 1900	Log Pigs in 1900	Log Sheep in 1900	Log Large Land holdings in 1900	Log Factories in 1900
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
WWII casualty rate	0.536	−0.486	−0.691	0.390	4.159	0.269	−0.186
	(0.359)	(1.861)	(0.727)	(1.201)	(3.182)	(1.211)	(0.686)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. observations	314	259	259	259	259	259	259
R-squared	0.96	0.76	0.81	0.79	0.71	0.53	0.67
Mean of dep. var.	6.62	2.87	6.16	4.64	3.30	0.41	0.18
S.d. of dep. var.	0.78	1.45	0.64	0.99	2.25	0.67	0.45
P-value	0.137	0.794	0.343	0.746	0.193	0.825	0.787

This table summarizes estimation results based on municipality-level data. The dependent variable is mentioned in the column heading. The control variables in each specification are listed in Panel B of Table 1. In addition, district fixed effects are included. The method of estimation is least squares. Robust standard errors are in parentheses below. The row "P-value" reports the p-values of a two-sided hypothesis test of the estimate of the WWII casualty rate being zero. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table 5: Short-Run Effects of WWII Casualties on Agricultural Outcomes

	Share of workers in agriculture in 1951	Diff. in log pop. in agric. btw. 1934 and 1951	Share of family-internal agric. workers of pop. in 1960	Share of family-external agric. workers of pop. in 1960
	(I)	(II)	(III)	(IV)
WWII casualty rate	−0.490***	−1.050***	−0.233*	−0.089**
	(0.138)	(0.376)	(0.121)	(0.044)
Control variables	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
No. observations	314	314	314	314
R-squared	0.94	0.33	0.84	0.47
Mean of dep. var.	0.52	−0.18	0.20	0.02
S.d. of dep. var.	0.24	0.19	0.13	0.03

This table summarizes estimation results based on municipality-level data. The dependent variable is mentioned in the column heading. The control variables in each specification are listed in Panel B of Table 1. In addition, district fixed effects are included. The method of estimation is least squares. Robust standard errors are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table 6: WWII Casualties and the Evolution of Sector Shares

PANEL A	Share of workers in agriculture in						
	1951	1961	1971	1981	1991	2001	2011
WWII casualty rate	−0.490***	−0.289	−0.358*	0.040	−0.016	0.111	0.049
	(0.138)	(0.180)	(0.204)	(0.149)	(0.107)	(0.090)	(0.071)
R-squared	0.94	0.89	0.76	0.70	0.68	0.61	0.65
Mean of dep. var.	0.52	0.39	0.21	0.12	0.08	0.06	0.05
S.d. of dep. var.	0.24	0.23	0.17	0.11	0.08	0.06	0.05
PANEL B	Share of workers in manufacturing in						
	1951	1961	1971	1981	1991	2001	2011
WWII casualty rate	0.573***	0.592***	0.656***	0.517***	0.459***	0.422***	0.249**
	(0.141)	(0.196)	(0.224)	(0.191)	(0.170)	(0.150)	(0.124)
R-squared	0.89	0.79	0.66	0.64	0.58	0.52	0.54
Mean of dep. var.	0.33	0.37	0.43	0.41	0.37	0.30	0.27
S.d. of dep. var.	0.18	0.18	0.16	0.13	0.11	0.09	0.08
PANEL C	Share of workers in services in						
	1951	1961	1971	1981	1991	2001	2011
WWII casualty rate	−0.084	−0.303	−0.299	−0.557***	−0.443**	−0.532***	−0.298**
	(0.126)	(0.190)	(0.225)	(0.199)	(0.183)	(0.165)	(0.130)
R-squared	0.69	0.60	0.57	0.60	0.54	0.51	0.61
Mean of dep. var.	0.15	0.24	0.36	0.47	0.55	0.64	0.68
S.d. of dep. var.	0.09	0.12	0.14	0.13	0.11	0.10	0.09
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. observations	314	314	314	314	314	314	314

This table summarizes estimation results based on municipality-level data. The dependent variable is mentioned in the column heading. The control variables in each specification are listed in Panel B of Table 1. In addition, district fixed effects are included. The method of estimation is least squares. Robust standard errors are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table 7: WWII Casualties and Long-Run Development I

	Log total wage bill in 2011				
	(I)	(II)	(III)	(IV)	(V)
WWII casualty rate	−6.901**	−4.825**	−5.038**	−4.772***	−5.671**
	(2.880)	(2.013)	(1.964)	(1.689)	(2.193)
Log population in 1934		1.173***	1.113***		1.114***
		(0.103)	(0.114)		(0.114)
Log population in 2011				1.237***	
				(0.083)	
WWII casualty rate × Soviet zone					3.135
					(4.816)
% pop. in agriculture in 1934		−2.822***	−2.453***	−1.684***	−2.467***
		(0.375)	(0.442)	(0.386)	(0.443)
Market status in 1945 (dummy)		−0.271	0.075	−0.030	0.077
		(0.183)	(0.193)	(0.165)	(0.193)
Log elevation		0.662**	0.546	0.651	0.534
		(0.326)	(0.567)	(0.487)	(0.568)
Log slope		−0.115	0.678	0.557	0.647
		(0.473)	(0.691)	(0.594)	(0.693)
Log elevation × log slope		−0.039	−0.174	−0.123	−0.169
		(0.085)	(0.126)	(0.108)	(0.126)
% Jewish pop. in 1934		−0.983	−2.261	0.953	−1.258
		(8.563)	(7.709)	(6.638)	(7.870)
% Social Democrats in 1930		−2.147***	−1.653***	−1.298***	−1.665***
		(0.474)	(0.571)	(0.492)	(0.572)
% NSDAP in 1930		0.581	1.162	0.481	1.163
		(2.351)	(3.001)	(2.579)	(3.004)
District FE			Yes	Yes	Yes
No. observations	314	314	314	314	314
R-squared	0.02	0.58	0.72	0.80	0.72
Mean of dep. var.	8.81	8.81	8.81	8.81	8.81
S.d. of dep. var.	1.54	1.54	1.54	1.54	1.54

This table summarizes estimation results based on municipality-level data. The dependent variable is mentioned in the column heading. The control variables in each specification are listed. In addition, district fixed effects are included. The method of estimation is least squares. Robust standard errors are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table 8: WWII Casualties and Long-Run Development II

PANEL A	Log mean yearly wages				
	1972	1981	1991	2001	2011
	(I)	(II)	(III)	(IV)	(V)
WWII casualty rate	−0.772*	−1.144***	−1.512***	−1.513***	−1.396***
	(0.424)	(0.338)	(0.334)	(0.446)	(0.421)
Control variables	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
No. observations	275	300	311	313	312
R-squared	0.59	0.61	0.59	0.54	0.63
Mean of dep. var.	8.45	9.27	9.70	9.97	10.19
S.d. of dep. var.	0.18	0.16	0.15	0.20	0.21

PANEL B	Log mean yearly wages in 2011				
	Males	Females	Agriculture	Manufacturing	Services
	(I)	(II)	(III)	(IV)	(V)
WWII casualty rate	−1.366***	−1.290***	1.054	−1.180**	−1.085***
	(0.408)	(0.380)	(1.357)	(0.484)	(0.387)
Control variables	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
No. observations	311	312	187	298	312
R-squared	0.67	0.58	0.67	0.52	0.54
Mean of dep. var.	10.36	9.90	9.06	10.37	10.08
S.d. of dep. var.	0.21	0.18	0.52	0.19	0.17

This table summarizes estimation results based on municipality-level data. The dependent variable is mentioned in the column heading. The control variables in each specification are listed in Panel B of Table 1. In addition, district fixed effects are included. Each observation is weighted by the number of workers that mean wage is based on. The method of estimation is least squares. Robust standard errors are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table 9: WWII Casualties and Long-Run Development III

PANEL A	Log local workers			
	Total	Agriculture	Manufacturing	Services
	(I)	(II)	(III)	(IV)
WWII casualty rate	−2.827**	0.059	−0.552	−3.308**
	(1.268)	(1.161)	(2.119)	(1.364)
Control variables	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
No. observations	314	314	314	314
R-squared	0.82	0.73	0.71	0.81
Mean of dep. var.	5.96	3.40	4.53	5.56
S.d. of dep. var.	1.23	0.92	1.62	1.30

PANEL B	Log number of firms			
	Total	Agriculture	Manufacturing	Services
	(I)	(II)	(III)	(IV)
WWII casualty rate	−1.887**	0.297	−1.366	−2.675**
	(0.845)	(1.015)	(1.075)	(1.056)
Control variables	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
No. observations	314	314	314	314
R-squared	0.85	0.77	0.82	0.83
Mean of dep. var.	4.82	2.98	2.72	4.36
S.d. of dep. var.	0.90	0.87	1.05	1.05

This table summarizes estimation results based on municipality-level data. The dependent variable is mentioned in the column heading. The control variables in each specification are listed in Panel B of Table 1. In addition, district fixed effects are included. The method of estimation is least squares. Robust standard errors are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table 10: Robustness Check: WWII Casualties and Local Economic Activity

	Log total wage bill in 2011			
	(I)	(II)	(III)	(IV)
WWII casualty rate	−5.355***	−5.021***	−5.279***	−5.858***
	(1.716)	(1.742)	(1.839)	(1.918)
Control variables	Yes	Yes	Yes	Yes
Pair FE	Yes	Yes	Yes	Yes
Sample I		Yes		
Sample II			Yes	
Sample III				Yes
No. observations	652	638	564	526
R-squared	0.88	0.88	0.88	0.86
Mean of dep. var.	8.99	9.00	8.97	8.85
S.d. of dep. var.	1.55	1.55	1.55	1.47

This table summarizes estimation results based on municipality-level data. The dependent variable is mentioned in the column heading. The control variables in each specification are listed in Panel B of Table 1. In addition, pair fixed effects (where pairs are given by neighboring municipalities) are included. The method of estimation is least squares. Robust standard errors are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table 11: Age Structure Control Variables

PANEL A	Share of workers in agriculture in 1951				
	(I)	(II)	(III)	(IV)	(V)
WWII casualty rate	−0.49***	−0.43***	−0.48***	−0.49***	−0.49***
	(0.14)	(0.14)	(0.14)	(0.14)	(0.14)
R-squared	0.94	0.95	0.95	0.95	0.95
Mean of dep. var.	0.52	0.52	0.52	0.52	0.52
S.d. of dep. var.	0.24	0.24	0.24	0.24	0.24
PANEL B	Log total wage bill in 2011				
	(I)	(II)	(III)	(IV)	(V)
WWII casualty rate	−5.04**	−5.03**	−5.34***	−5.35***	−4.58**
	(1.96)	(1.96)	(1.93)	(1.96)	(1.92)
R-squared	0.72	0.73	0.74	0.73	0.77
Mean of dep. var.	8.81	8.81	8.81	8.81	8.81
S.d. of dep. var.	1.54	1.54	1.54	1.54	1.54
PANEL C	Log mean yearly wages in 2011				
	(I)	(II)	(III)	(IV)	(V)
WWII casualty rate	−1.40***	−1.43***	−1.32***	−1.38***	−1.38***
	(0.42)	(0.42)	(0.42)	(0.42)	(0.42)
R-squared	0.63	0.64	0.64	0.63	0.68
Mean of dep. var.	10.19	10.19	10.19	10.19	10.19
S.d. of dep. var.	0.21	0.21	0.21	0.21	0.21
Age structure 1939 controls		Yes			
Age structure 1951 controls			Yes		
Age structure 1961 controls				Yes	
Age structure 1971 controls					Yes
Controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
No. observations	314	314	314	314	314

This table summarizes estimation results based on municipality-level data. The dependent variable is mentioned in the column heading. Different control variables for the age structure in 1939, 1951, 1961 or 1971 ar included in columns (II)-(V), respectively. The usual control variables in each specification are listed in Panel B of Table 1. The method of estimation is least squares. Robust standard errors are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

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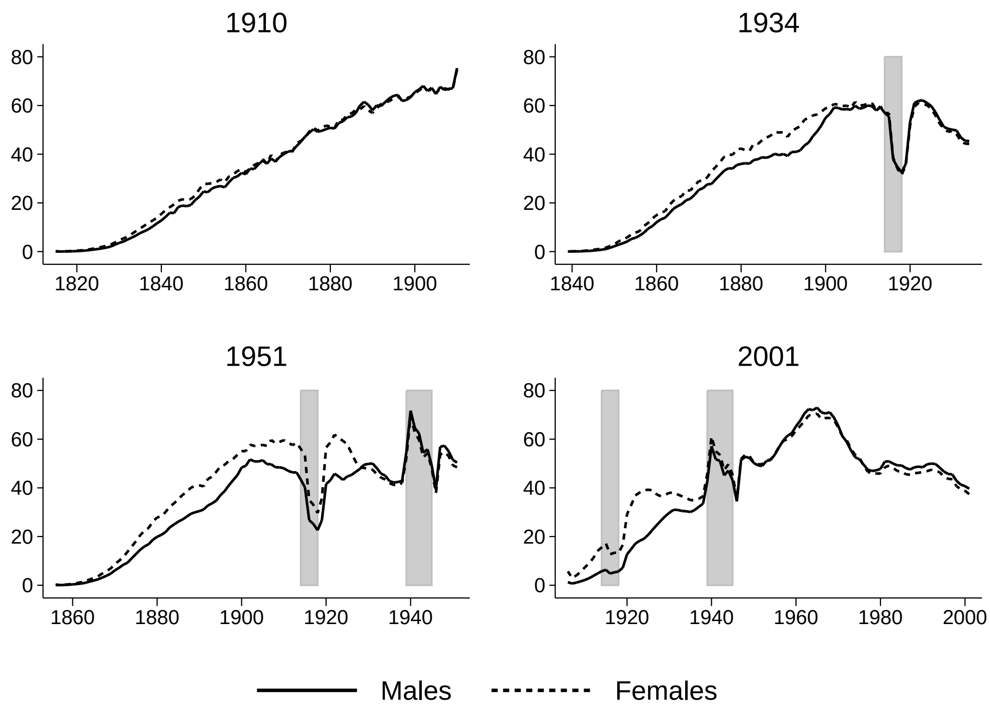
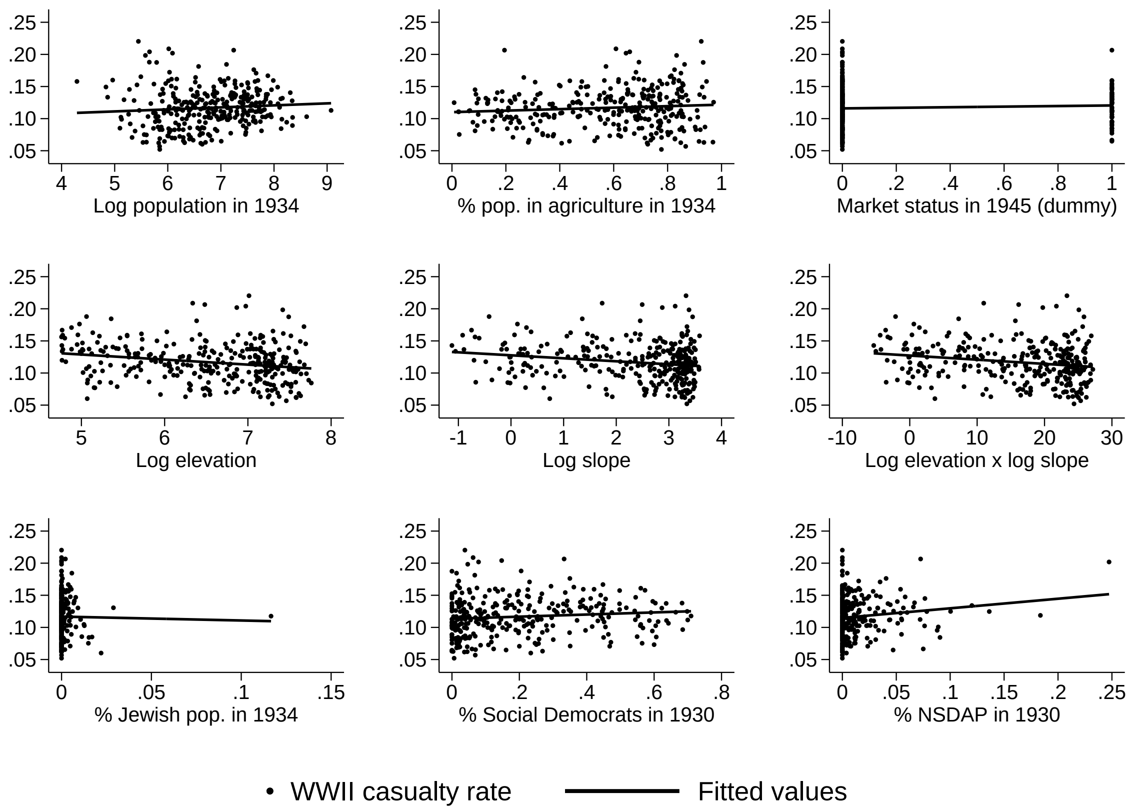
Appendix A Additional Figures

Figure A.1: Male and Female Population by Birth Cohort in Austria



The horizontal axis shows the year of birth and the vertical axis the population in thousands. The title of each diagram reports the year of the census. The grey bars mark WWI and WWII. Data is from the Austrian population censuses.

Figure A.2: Correlations of the WWII Casualty Rate with Pre-war Municipality Characteristics



Appendix B Additional Tables

Table B.1: WWII Casualties and Individual Wages

	Log yearly wages in 2011				
	(I)	(II)	(III)	(IV)	(V)
WWII casualty rate	−1.919***	−1.725***	−1.328***	−1.748***	−1.141***
	(0.675)	(0.485)	(0.380)	(0.664)	(0.316)
Municipality controls	Yes	Yes	Yes	Yes	Yes
Individual controls		Yes			Yes
Firm controls			Yes		Yes
Job controls				Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
No. observations	146,540	146,540	146,540	146,540	146,540
R-squared	0.07	0.22	0.24	0.11	0.36
Mean of dep. var.	9.99	9.99	9.99	9.99	9.99
S.d. of dep. var.	0.79	0.79	0.79	0.79	0.79

This table summarizes estimation results based on individual-level data. The dependent variable is mentioned in the column heading. The set of control variables in each specification are indicated. Municipality control variables are listed in Panel B of Table 1. Individual control variables are dummy variables for each year of age, gender, highest education level achieved, and foreigners. Firm controls are industry indicators at the two-digit NACE level. Job controls are indicators for apprentices, blue and white collar jobs. In addition, district fixed effects are included. The method of estimation is least squares. Standard errors are clustered at the municipality level and are shown in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table B.2: Robustness Check: WWII Casualties and Wages

	Log mean yearly wages in 2011			
	(I)	(II)	(III)	(IV)
WWII casualty rate	−1.332***	−1.327***	−1.413***	−0.833**
	(0.335)	(0.342)	(0.366)	(0.391)
Control variables	Yes	Yes	Yes	Yes
Pair FE	Yes	Yes	Yes	Yes
Sample I		Yes		
Sample II			Yes	
Sample III				Yes
No. observations	649	635	561	524
R-squared	0.91	0.90	0.91	0.90
Mean of dep. var.	10.21	10.21	10.21	10.17
S.d. of dep. var.	0.21	0.21	0.22	0.22

This table summarizes estimation results based on municipality-level data. The dependent variable is mentioned in the column heading. The control variables in each specification are listed in Panel B of Table 1. In addition, pair fixed effects (where pairs are given by neighboring municipalities) are included. Each observation is weighted by the number of workers that mean wage is based on. The method of estimation is least squares. Robust standard errors are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table B.3: WWII Casualties and Educational Outcomes

PANEL A	Mean years of education of the pop. 15+				
	1971	1981	1991	2001	2011
	(I)	(II)	(III)	(IV)	(V)
WWII casualty rate	0.298	0.379	0.413	−0.312	−0.183
	(0.593)	(0.628)	(0.628)	(0.610)	(0.627)
Control variables	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
No. observations	314	314	314	314	314
R-squared	0.67	0.64	0.60	0.59	0.57
Mean of dep. var.	10.04	10.64	11.21	11.69	12.16
S.d. of dep. var.	0.43	0.43	0.41	0.39	0.40

PANEL B	Share of pop. 15+ with only mandatory education				
	1971	1981	1991	2001	2011
	(I)	(II)	(III)	(IV)	(V)
WWII casualty rate	−0.066	−0.098	−0.073	0.061	0.037
	(0.141)	(0.139)	(0.130)	(0.105)	(0.089)
Control variables	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
No. observations	314	314	314	314	314
R-squared	0.67	0.63	0.57	0.54	0.51
Mean of dep. var.	0.75	0.60	0.47	0.38	0.30
S.d. of dep. var.	0.10	0.09	0.08	0.06	0.05

This table summarizes estimation results based on municipality-level data. The dependent variable is mentioned in the column heading. The control variables in each specification are listed in Panel B of Table 1. In addition, district fixed effects are included. The method of estimation is least squares. Robust standard errors are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table B.4: WWII Casualties and Age-Specific Educational Outcomes in 1971

	Share of 20 to 34 year olds in 1971 with		
	only mandatory education	apprenticeship	secondary schooling
	(I)	(II)	(III)
WWII casualty rate	−0.285	0.252	0.016
	(0.211)	(0.159)	(0.120)
Control variables	Yes	Yes	Yes
District FE	Yes	Yes	Yes
No. observations	314	314	314
R-squared	0.61	0.64	0.35
Mean of dep. var.	0.59	0.27	0.12
S.d. of dep. var.	0.14	0.11	0.06

This table summarizes estimation results based on municipality-level data. The dependent variable is mentioned in the column heading. The control variables in each specification are listed in Panel B of Table 1. In addition, district fixed effects are included. The method of estimation is least squares. Robust standard errors are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table B.5: Population Changes

	Change in population 1961-71			Share of pop. 0–14 years old in		
	Total	Natural	Migration	1951	1961	1971
	(I)	(II)	(III)	(IV)	(V)	(VI)
WWII casualty rate	−0.011	0.136	−0.147	−0.003	0.077	−0.044
	(0.253)	(0.115)	(0.210)	(0.065)	(0.066)	(0.063)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes
No. observations	314	314	314	314	314	314
R-squared	0.58	0.68	0.52	0.58	0.62	0.72
Mean of dep. var.	0.14	0.13	0.01	0.25	0.27	0.28
S.d. of dep. var.	0.16	0.08	0.13	0.04	0.04	0.05

This table summarizes estimation results based on municipality-level data. The dependent variable is mentioned in the column heading. The control variables in each specification are listed in Panel B of Table 1. In addition, district fixed effects are included. The method of estimation is least squares. Robust standard errors are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Table B.6: Foreigner population

	Share of foreigners in population		
	1951	1971	2001
	(I)	(II)	(III)
WWII casualty rate	−0.001	−0.055	−0.015
	(0.048)	(0.047)	(0.077)
Control variables	Yes	Yes	Yes
District FE	Yes	Yes	Yes
No. observations	314	314	314
R-squared	0.43	0.56	0.56
Mean of dep. var.	0.03	0.02	0.07
S.d. of dep. var.	0.03	0.03	0.05

This table summarizes estimation results based on municipality-level data. The dependent variable is mentioned in the column heading. The control variables in each specification are listed in Panel B of Table 1. In addition, district fixed effects are included. The method of estimation is least squares. Robust standard errors are in parentheses below. *, ** and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Appendix C Data Sources

The data for the economic variables come from various publications of *Statistik Austria*, the Austrian statistical agency (earlier names of the agency: *K. K. Statistischen Zentralkommission*, *Bundesamt für Statistik*, and *Österreichisches Statistisches Zentralamt*):

- "Gemeindelexikon der im Reichsrate vertretenen Königreiche und Länder: Bearbeitet auf Grund der Ergebnisse der Volkszählung vom 31. Dezember 1900"
- "Die Ergebnisse der österreichischen Volkszählung vom 22. März 1934"
- "Die Ergebnisse der österreichischen Volkszählung vom 1. Juni 1951"
- "Ergebnisse der land- und forstwirtschaftlichen Betriebszählung vom 1. Juni 1951"
- "Die Ergebnisse der österreichischen Volkszählung vom 21. März 1961"
- "Land- und forstwirtschaftliche Betriebszählung vom 1. Juni 1960"
- "Die Ergebnisse der österreichischen Volkszählung vom 12. Mai 1971"
- "Volkszählung 1981"
- "Volkszählung 1991"
- "Ein Blick auf die Gemeinde" (`www.statistik.at/blickgem`)