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Author(s): Andrea Ichino and Rudolf Winter-Ebmer
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The Long-Run Educational Cost of World War II

Andrea Ichino, *European University Institute, CEPR, IZA, and CESifo*

Rudolf Winter-Ebmer, *University of Linz, CEPR, IZA, and WIFO*

An important component of the long-run cost of a war is the loss of human capital suffered by school-age children who receive less education. Austrian and German individuals who were 10 years old during the conflict, or were more directly involved through their parents, received less education than comparable individuals from nonwar countries, such as Switzerland and Sweden. We also show that these individuals experienced a sizable earnings loss some 40 years after the war, which can be attributed to the educational loss caused by the conflict. The implied consequences in terms of gross domestic product loss are calculated.

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I. Introduction

Wars are costly in several dimensions, most of which are fairly obvious. One of these dimensions is perhaps less evident: wars disrupt the educational process, making it harder for the population of school age to achieve the desired level of education. This is likely to be true not only for the older cohorts forced to join the army but also for the younger cohorts of primary school age. For these cohorts, particularly during wars that severely hit the civilian population, physical access to schools may be less easy because of bombings, fighting, army requisitions, and transportation difficulties. In addition, casualties among older family members may increase constraints and prevent an otherwise feasible transition into higher education even when the war is over.

In this article, we provide evidence for these effects, comparing Austria and Germany, where the civilian population was severely affected by World War II, with Sweden and Switzerland, which did not enter the conflict directly. We find that Austrian and German individuals who were 10 years old during or immediately after the conflict went to school for a significantly shorter period than equivalent individuals in other cohorts. This is also true if secular trends in educational attainment are controlled for. In contrast to this, war cohorts in Sweden and Switzerland follow a smooth upward educational trend with no war disruption. We discuss whether other reasons, different from the war, might explain these facts, and we conclude, also on the basis of additional specific information on Germany, that the disruption of the educational process caused by military events is the most likely explanation of the observed evidence.

Having established that these educational effects of World War II exist, we also try to evaluate their relevance. We do so by measuring the average earnings loss suffered by those children who, because of the war, received less education. The total amount of these losses in a given year indicates how much higher gross domestic product (GDP) could have been if the war had not had the observed educational effect. The local average treatment effect (LATE) interpretation of instrumental variables (IV) techniques suggested by Imbens and Angrist (1994) allows us to identify and estimate precisely the effect that we would like to measure, that is, the average return to 1 year of education for an individual who had to reduce his educational attainment because of the war.[1] Note that under the conditions required by this interpretation of IV, this is the only average return to schooling that we can identify with our instruments and our samples. However, far from being a limitation, this is precisely the average return

[1] For closely related concepts in previous studies, see also Björklund and Moffit (1987) and Heckman and Robb (1985).

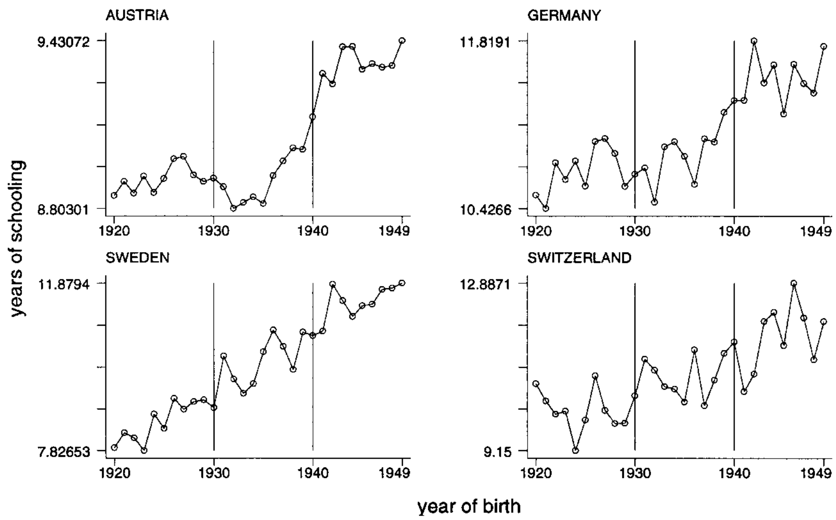

FIG. 1.—Education

in which we are interested, given that our goal is to measure the educational cost of World War II.[2]

The article is organized as follows. Section II describes the evidence on the educational effect of World War II. Section III evaluates the relevance of this educational effect by measuring the average earning loss suffered by those children who, because of the war, received less education. Section IV computes the implied loss of GDP for Austria and Germany. Section V concludes.

II. The Effect of World War II on Educational Attainment

Figure 1 describes the evolution of educational attainment by year of birth in four countries: Austria, Germany, Sweden, and Switzerland.[3] The measure of educational attainment is the average number of years of schooling completed by the individuals born in each given year between 1920 and 1949.[4] Over this period, all of these countries experienced an increase in mean educational attainment. However, in Austria and Germany individuals born during the 1930s appear to have completed approximately the same (or even a lower) number of years of schooling than individuals born in the 1920s. This is not the case in Sweden and

[2] Following our example, Gregory and Meng (1999) perform a similar exercise to investigate the educational cost of the Chinese Cultural Revolution.

[3] See app. A for a description of the data.

[4] In Austria and Germany, for persons with an apprentice training—which usually takes 2–3 years—1 year of formal education was added.

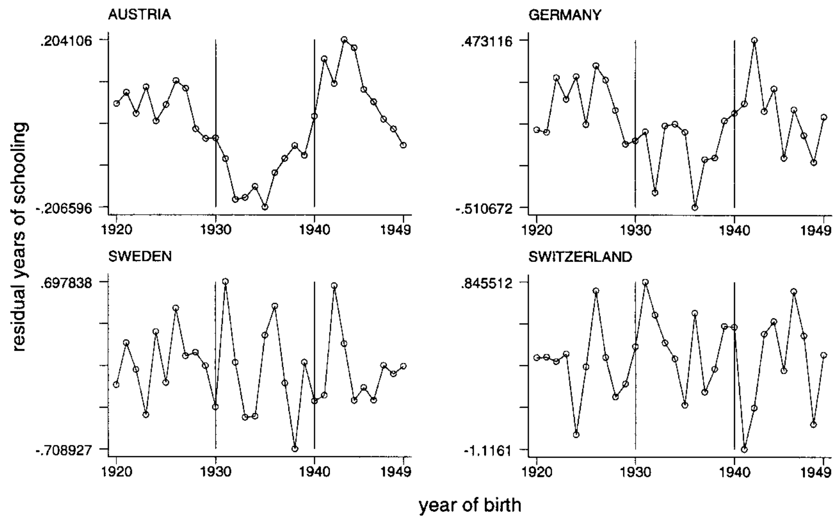

FIG. 2.—Education after controlling for a cubic age profile

Switzerland. In other words, during the intermediate decade, the secular progress toward higher educational attainment slowed down significantly in Austria and Germany but not in the other two countries.

To further explore the cross-country and cross-cohort differences suggested by figure 1, we assume that the secular educational trend can be captured in each country by a cubic polynomial in age estimated separately for males and females. The residuals from these gender- and country-specific cubic trends are displayed in figure 2.[5] In Austria and Germany, the thirties are evidently characterized by negative deviations from the secular trend, while there is no evidence of similar negative deviations in Sweden or Switzerland during the same period.

We test more formally for the existence of significant breaks in the evolution of these residuals, following a procedure proposed by Andrews (1993) that allows us to evaluate the existence and the timing of a change with an unknown break point in a stationary time series. Leaving to appendix B the formal characterization of the test, the statistic we consider in our implementation is the supremum of the likelihood ratio (LR) test statistics for all the potential break points occurring in a central interval

[5] Detrending is done by using persons born between 1910 and 1960 and calculating residuals from a cubic trend—separately for gender groups and for the respective sample years in Austria, Germany, Switzerland, and Sweden. Results using residuals from quartic, quadratic, and linear trends are very similar in flavor and are available on request. See, e.g., Bound and Jaeger (1996), who stress the importance of proper detrending in earnings studies when possible instruments are cohort related.

of the time series (i.e., trimming a fraction of the observations at both ends of the series).[6]

As the test is constructed for one break in a time series, we can first investigate the existence and timing of a change in the educational attainment residuals by looking at the period between 1925 and 1935. Figure 3 plots the value of the LR for all the potential breaks occurring in this period. The horizontal lines are, respectively, from the lowest to the highest, the 10%, 5%, and 1% critical values of the Andrews test statistics.[7] For Austria, the maximum LR is way above the critical values in the year 1930. Also in Germany, the maximum is above the critical values but corresponds to the year 1928. There is, instead, no evidence of a significant break between 1925 and 1935 in Sweden and Switzerland. Figure 4 plots the analogous statistics for a potential break occurring in the four countries during the period 1935–45. The maximum for Austria is above the critical values for the year 1939. In Germany, the most likely break takes place in 1938, but its existence can only be accepted at the 10% significance level. Again, no significant breaks seem to characterize the Swedish and Swiss series.

The evidence jointly provided by figures 3 and 4 supports the hypothesis that, for the cohorts born during the thirties, the secular trend toward greater educational attainment slowed down in Austria and Germany but not in Sweden and Switzerland. The size of this educational loss can be evaluated using the estimates presented in table 1. For each country, this table displays the coefficients of a regression of the residuals plotted in figure 2 on dummies for the three birth decades and on a gender dummy, without a constant.[8] The difference between the coefficients of two consecutive cohorts can be interpreted as the difference between the

[6] Clearly, it is not possible to search for a break from the very beginning of the sample or until the very end because there must be a sufficient number of observations on each side of the potential break to establish whether there is a difference between the period "before" and the period "after." For further details, see app. B.

[7] These critical values are computed for a 0.25 trimming parameter. See the appendix and table 1 in Andrews (1993).

[8] While the Andrews test suggests that for Austria the most plausible break points are precisely the years 1930 and 1939, which delimit the intermediate decade, for Germany, the most likely break points seem to occur slightly before, being situated, according to figs. 3 and 4, in 1928 and 1938, respectively. All our results are qualitatively and quantitatively similar if these slightly different break points are used for Germany; the difference between the war and the nonwar cohorts and, thus, the effects of the war are actually slightly higher in this case. Nevertheless, since the Austrian estimates are based on a larger sample, to simplify the exposition and to facilitate the comparison across countries (see, e.g., table 4 below), we present results for all countries that consider the cohort born in the thirties as the one whose education was potentially affected by the war. The alternative set of results is available from the authors.

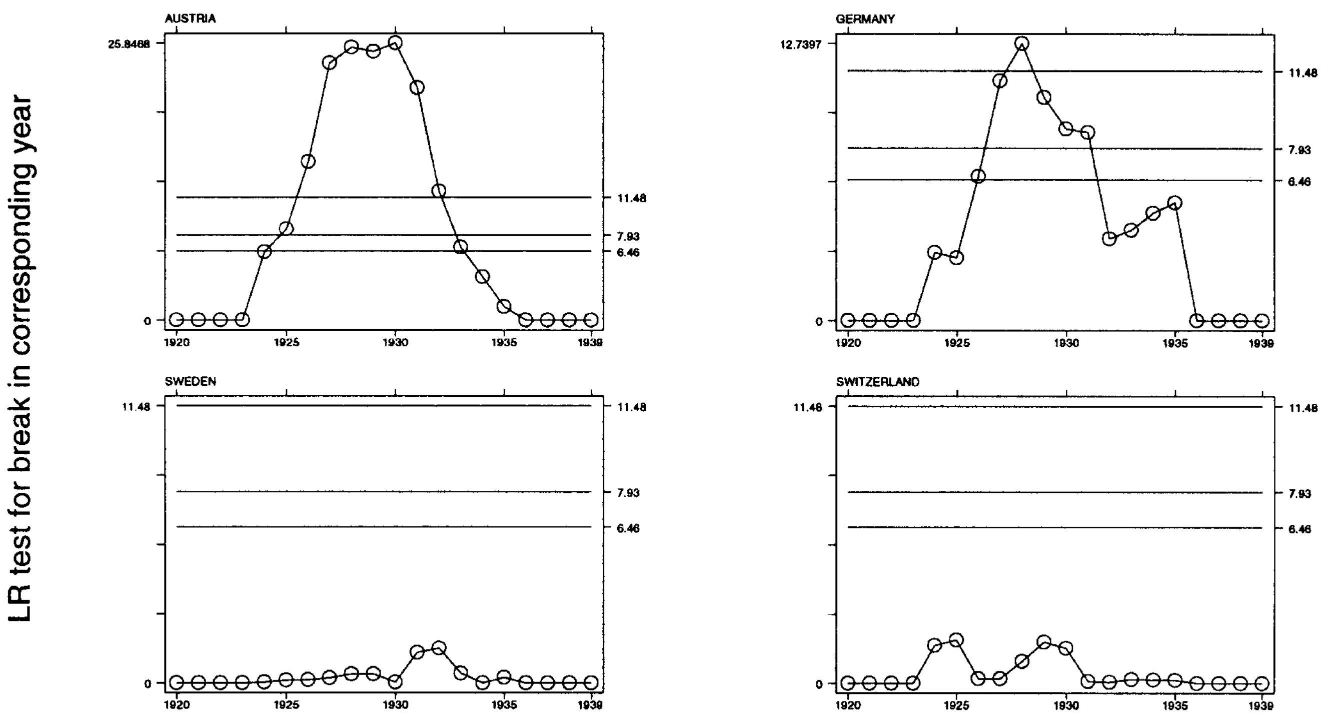

FIG. 3.—Testing for a break in residual years of education between 1925 and 1935

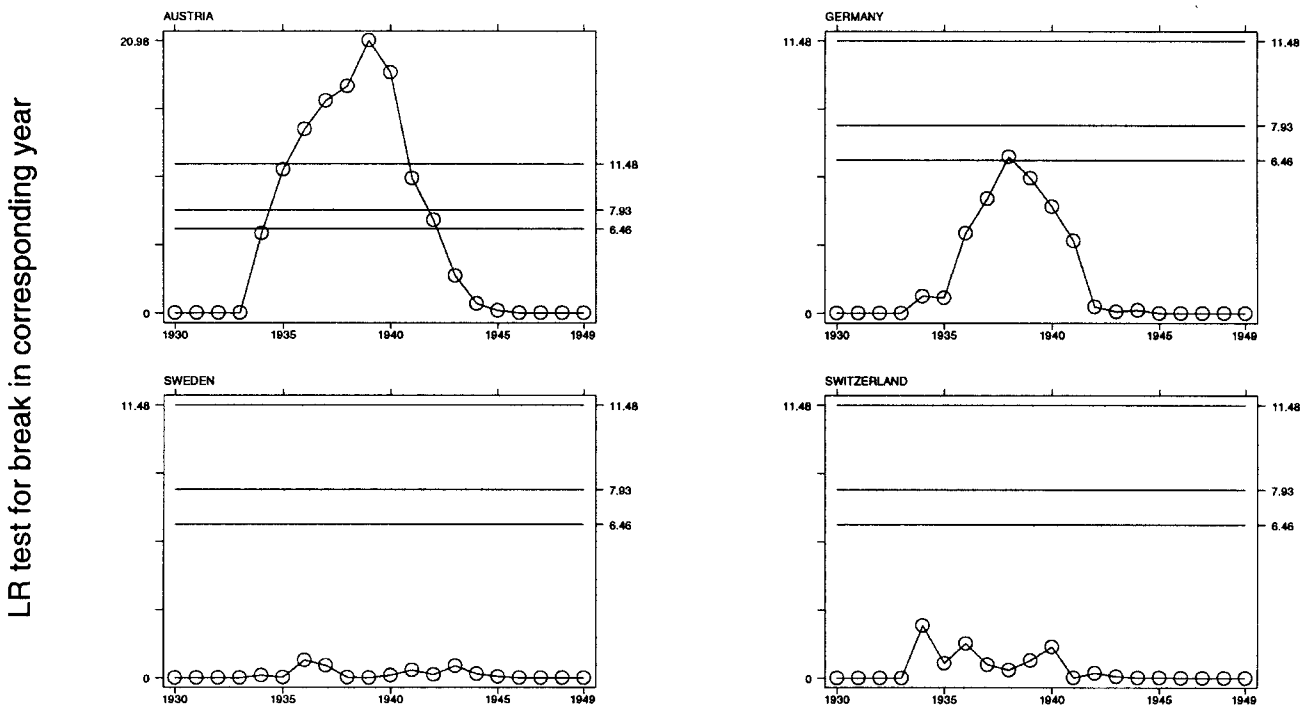

FIG. 4.—Testing for a break in residual years of education between 1935 and 1945

Table 1
Cohort of Birth and Educational Attainment

	Austria	Germany	Sweden	Switzerland
Born 1920–30	.047	.098	.020	−.118
	(.014)	(.076)	(.121)	(.194)
Born 1930–39	−.112**	−.163**	−.068	.105
	(.013)	(.073)	(.126)	(.183)
Born 1940–49	.078**	.060*	−.049	−.029
	(.015)	(.080)	(.120)	(.164)
Number of observations	72,128	3,572	3,364	1,236

NOTE.—For each country, the dependent variable is the residual of a regression of years of education on a cubic polynomial in age estimated separately for males and females. The table reports the coefficients of the regression of these residuals on the three cohort dummies (without a constant) and on a gender dummy. The coefficient of the gender dummy, not reported, is always negative and insignificant. Robust standard errors are in parentheses.

* The coefficient of the corresponding cohort is significantly different from the coefficient of the previous cohort at the 5% level.

** The coefficient of the corresponding cohort is significantly different from the coefficient of the previous cohort at the 1% level.

numbers of years of schooling completed, on average, by the two cohorts controlling for the secular trend in educational attainment.

Looking at the first column of table 1, the average educational loss of the Austrian cohort born in the thirties amounts to approximately 16% of a year of schooling with respect to the previous cohort and to 19% with respect to the following cohort, and both differences are significantly different from zero. The corresponding losses for Germany are 26% and 23% and are again statistically significant.[9] As expected from the previous evidence, no statistically significant difference can, instead, be found for the other two countries.[10]

There is one major event that might explain this set of facts, and this is World War II. Indeed, while Austria and Germany were heavily involved in the war, and their civilian populations suffered significant disruptions of normal civilian life, Sweden and Switzerland remained out of the conflict, and their civilian populations were significantly less affected

[9] To put this estimate into perspective, it may be interesting to compare it with the analogous one reported in the Angrist and Krueger (1991) paper, which uses quarter of birth as an instrument for educational attainment. There, completed years of schooling for men born in the first quarter of the year are approximately one-tenth of a year lower as compared to men born in the last quarter of the year.

[10] Goldin (1998) shows lower high school attainment, as well as lower college graduation rates, even for the United States in the wartime years, and she explains this fact by the increasing attractiveness of civilian jobs, as wages rose disproportionally for unskilled labor.

by war events.[11] We claim that the extent to which the civilian population in Austria and Germany was affected by the war might explain the slowdown of the educational process in these two countries for the cohorts born in the thirties. Note that these are the cohorts who reached age 10 during or immediately after the war.[12] Age 10 was—and still is—a crucial age in Germany and Austria for educational decisions: pupils had to decide at age 10 if they wanted to go to high school (*Gymnasium*), which was the only way to get access to universities later on. The other option was junior high school (*Hauptschule* or, in limited cases, *Realschule* in Germany), where compulsory schooling stopped at age 14 or 16. Several reasons may induce pupils to reduce schooling attainment during wars. Financial means for schools in general are lowered, transportation becomes more difficult, and so forth. Moreover, if the father serves actively in the war, the family situation is certainly unfavorable with respect to schooling. Due to these constraints, the children might also act as substitute breadwinners and start working earlier.

Direct effects of the war on educational opportunities in Germany and Austria can be assumed due to several factors leading to fewer school buildings and teachers. Confessional schools were closed down, and Jewish teachers were evicted. In Austria's early Nazi years, 17% of general secondary schools were closed because they were Catholic (Engelbrecht 1988, p. 312). In Germany, the number of teachers in lower secondary schools decreased by 23% between 1931 and 1939; those in academic secondary schools decreased by 5% (Petzina, Abelshauser, and Faust 1978,

[11] In a previous version of this article (Ichino and Winter-Ebmer 1998), we also used data for other countries: the Netherlands, Hungary, Finland, the United Kingdom, Northern Ireland, and the United States for war countries; Ireland, Thailand, India, and Brazil for nonwar countries. The evidence there relates to the proportion of students attending high school and above and supports our claims. In this version, we present evidence only on Austria, Germany, Sweden, and Switzerland because, among the countries for which reliable data are available, these four are the most homogeneous set in which war countries and nonwar countries are both included. Thus, they offer a relatively good quasi-experimental situation. DeGroot (1948, 1951) also describes early evidence on a negative effect of World War II on educational performance of youth in the Netherlands.

[12] This choice reduces the problems generated by focusing on veterans, who might suffer several additional consequences of the war beyond educational losses. Moreover, many military jobs may provide skills that are also transferable to the civilian labor market. See Angrist and Krueger (1994) for an assessment of earnings effects in the case of U.S. World War II veterans, and Maas and Settersen (1999) for an assessment of the effects on occupational trajectories and economic well-being of German World War II veterans. The problem is further complicated by the fact that in some cases veterans are entitled to preferential treatment in education after conscription. See Bound and Turner (1999) for an analysis of the U.S. G.I. Bill, and Lemieux and Card (2001) for the Canadian G.I. Bill. In Germany and Austria, no such programs existed.

Table 2
World War II and Educational Attainment in Germany

	1	2	3	4	5	6
Born 1930–39	−.214	−.191	−.189	−.163	.248	.372
	(.068)	(.076)	(.070)	(.073)	(.388)	(.390)
Born 1930–39 and living in big city		−.098				−.090
		(.167)				(.167)
Born 1930–39 and father in war			−.254			−.245
			(.242)			(.251)
Father in war			−.575			−.594
			(.183)			(.183)
Born 1930–39 and father died in war				−.329		−.319
				(.194)		(.195)
Father died in war				−.026		−.016
				(.136)		(.136)
Born 1930–39 and father without high school degree					−.502	−.534
					(.393)	(.390)
R^2	.182	.182	.186	.183	.183	.188
F-test (p-value)		.558	.000	.040	.202	.000

Note.—The dependent variable is the residual of a regression of years of education on a cubic polynomial in age, estimated separately for males and females. All the regressions also include a constant and dummies for gender, high school degree of the father, blue-collar status of the father, and for living in a big city at age 14. The last row reports the p-values of the F-test on the joint significance of the coefficients reported in the table other than the 1930–39 cohort dummy. Robust standard errors are in parentheses. The number of observations in all the columns of this table is 3,572.

p. 166). Starting from 1944, most prime-age teachers were sent to war and were—if possible—replaced by retired teachers or not-yet-graduated students. Bombing in the cities posed an immediate threat to both students and schools. Up to 5 million German and Austrian children were sent to the countryside (so-called *Kinderlandverschickung*) starting in 1940, which led to serious disruptions in educational careers and opportunities (Engelbrecht 1988, p. 335). Moreover, in the later years of the war, many school buildings had to be closed down due to bombing. In Vienna, out of 413 city schools in 1944, 9% were completely destroyed, a further 43% were mildly or severely damaged, and, finally, 23% were occupied as hospitals or offices (Engelbrecht 1988, p. 654).

One could argue that at least some of the individuals born between 1930 and 1939 experienced not only the effect of attending elementary schools during the war but also the effect of being born during the Great Depression. However, if the Great Depression were causing the educational loss observed in Austria and Germany, we should observe similar losses for the 1930–39 cohort in Sweden and Switzerland, which is clearly not the case.

Additional support for the hypothesis that the 1930–39 cohort effect captures a real war effect is provided in table 2, thanks to the more detailed information available in the German data set. Our goal here is to show that the cohort effect diminishes once we control for variables that may be thought to capture more directly the effect of World War II on the educational attainment of children. In all these regressions, the dependent variable measures the residual years of education plotted in figure 2 for Germany. In addition to the variables indicated in the table, we also include in all instances a constant and dummies for gender, high school degree of the father, blue-collar status of the father, and for living in a big city in childhood.

The first column replicates our analysis of table 1: individuals born in the thirties experienced an average loss of education, with respect to all the other cohorts, that amounts to 21% of a year of schooling, controlling for the secular increase in education.[13] In the second column, we include also the effect of the interaction between being born in the thirties and having lived in a big city for most of childhood up to age 15. If the measured cohort effect is ultimately caused by the war, the coefficient on this interaction should be negative because the civilian population was hit by the war more severely in big cities than in small villages or in the countryside. We do actually find a negative estimate, although not statistically significant, which supports our claim.

In column 3 we look at the implications of having a father involved in the war, which can be considered a more direct way in which World War II might have affected educational attainment. Here we measure if the father of the student served actively in the war or was kept as a prisoner of war at the time the student was age 15. Ceteris paribus, a German student whose father was involved in the conflict experienced an educational loss that amounts to 57% of a year of schooling and increases to 83% if the student was also born between 1930 and 1939. The fact that the interaction exacerbates the loss and that the pure cohort effect decreases when the father-in-war effect is taken into account provides additional support to the hypothesis that the cohort effect is indeed capturing the educational consequences of World War II.

The effects associated with the 1930–39 cohort dummy decrease further in absolute values in column 4, where we proxy the direct effect of the war with the interaction between being born in the thirties and having lost the father between 1940 and 1945. This variable can be taken as a proxy for the father's having died during the war. If the 1930–39 cohort dummy were not capturing the effect of the war, it would be hard to explain why this interaction should have a negative effect. It is, on the contrary, reasonable that the loss of a father might have had a bigger impact in conjunction with the war. Interestingly, the death of the father does not seem to have any impact on education for individuals not born

[13] Hence, this loss is an average of the losses with respect to the previous and following cohorts described for Germany in table 1.

in the 1930–39 war cohort. Similarly, in column 5 we show that being born in the thirties implies a slightly larger loss for students whose father had no high school education. This negative interaction can be explained by the fact that families in which fathers have low education are likely to be hit more severely by a war inasmuch as education is positively correlated with income and wealth. Note that, abstracting from statistical significance, which is low in any case, given the sample size, in column 5 the coefficient of the 1930–39 cohort dummy captures the war effect for the case of highly educated fathers and is positive. The average cohort effect, independent of the education of the father, is, instead, negative (see col. 1) because the vast majority of the observations in our sample (92%; see table A1 in app. A) are characterized by low parental education. Thus, the evidence clearly suggests that the few children whose father had at least a high school degree suffered less or not at all in terms of education during the war. It is plausible that this is due to the fact that the war imposed more constraints on poorly educated, and therefore probably less affluent, families. The results of column 6, in which all interactions are included, can be interpreted along similar lines. We take this as additional evidence that the 1930–39 cohort dummy is capturing the educational consequences of the war.

The evidence presented in this section supports the hypothesis that constraints due to the war are of first-order importance for an explanation of the slower trend toward greater educational attainment observed in Austria and Germany for the cohorts born between 1930 and 1939. Our next goal is to evaluate the economic relevance of this effect by measuring the average earning loss suffered by those children who, because of the war, received less education.

III. The Effect of World War II on Earnings

Figure 5 shows that the Austrian and German cohorts that experience a loss in educational attainment also experience a labor income loss that is noticeable as late as 40 years after the war.[14] In the case of Sweden and Switzerland, instead, the absence of educational losses for the cohort born in the thirties appears to be matched by an equivalent absence of labor income losses some 40 years later.[15] For all countries, the income measure used in the figure is the average log of hourly labor earnings in the year of the survey for individuals born in each given year between 1925 and 1949.[16]

[14] The income data are for the years 1981–85 in Austria and 1984–86 in Germany.

[15] The income data are for the years 1981 and 1984 in Sweden and 1982 in Switzerland.

[16] For the analysis of earnings contained in this section, the initial year is 1925, instead of 1920, to avoid sample selection problems due to retirement.

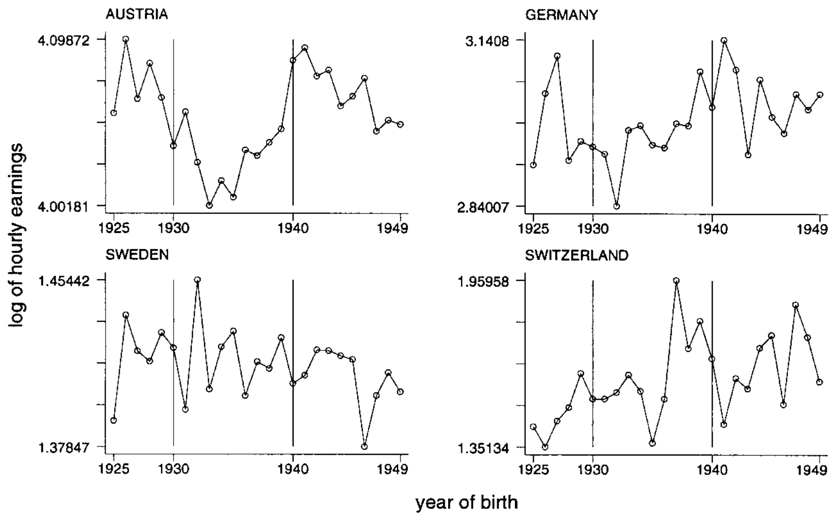

FIG. 5.—Earnings

Since age may also be an important confounding factor in the case of earnings, we follow the same procedure used in the case of education, and we assume that the earning-age profile can be captured in each country by a cubic polynomial in age estimated differently for males and females.[17] The residuals from these gender- and country-specific age profiles are displayed in figure 6. They confirm that the 1930–39 cohorts experience an earnings loss in Austria and Germany that does not have a counterpart in Sweden and Switzerland.

In table 3, we measure the size of this loss in a way similar to what we did in table 1 for the case of education. For each country, the table displays the coefficients of the regression of the residuals plotted in figure 6 on dummies for the three birth decades and on a gender dummy without a constant. The difference between the coefficients of two cohorts can be interpreted as the percentage change in labor earnings between the two cohorts, controlling for age-earning profiles. In Austria, the 1930–39 cohort experiences statistically significant losses with respect to the previous and following cohorts. These losses amount in both cases to 2.5%. In Germany, the income losses of the intermediate cohort are not statistically significant, but the point estimates are larger: 5.1% with respect to the previous cohort and 2.6% with respect to the following one. No signif-

[17] Also, the evidence for the war effect on earnings is robust with respect to the use of residuals from quartic, quadratic, and linear trends, and these results are available from the authors.

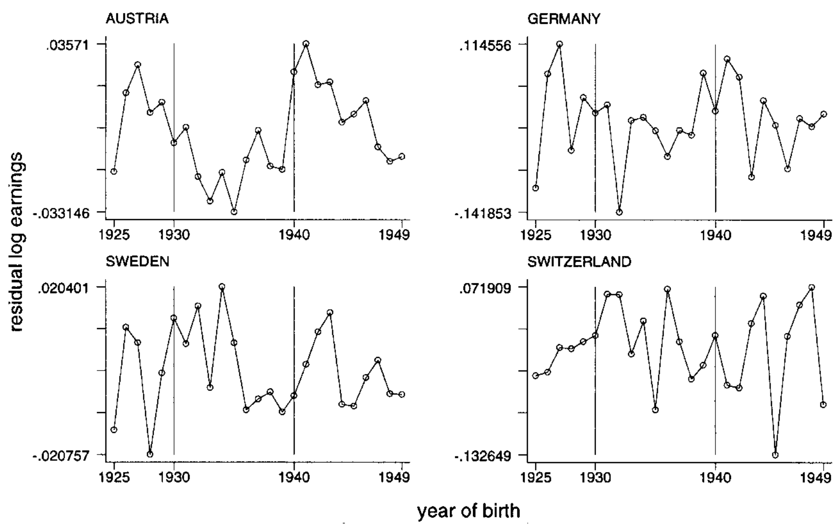

FIG. 6.—Earnings after controlling for a cubic age profile

icant income loss is observed for the intermediate cohort in Sweden and Switzerland.[18]

The evidence jointly provided by table 3 and by figures 5 and 6 suggests that in Austria and Germany, the war had not only an effect on education but also on earnings. Under a set of assumptions to be discussed below, the ratio between these two effects is the IV estimator of the returns to schooling obtained using the 1930–39 cohort indicator as an instrument.[19] Note that, given the likely heterogeneity of returns to schooling in the population, it is appropriate to interpret this IV result as an estimate of a local average treatment effect: specifically, the average earnings loss experienced by those individuals who received less education because they belonged to the 1930–39 war cohort.[20] As we will argue, these individuals are likely to be characterized by high returns to schooling in comparison to the rest of the population. Hence, what we obtain is not an estimate of the average return to schooling in the population, but it is, nonetheless, precisely an estimate of the long-run education cost of the war in which we are interested.[21]

[18] Again, we can compare our effect with the Angrist and Krueger (1991) study. The variation in earnings there is much lower: individuals born in the first quarter have wages 1% lower than those born in the fourth quarter of the year.

[19] See Imbens and Angrist (1994) and Angrist et al. (1996).

[20] See Card (1995, 2000).

[21] For further details on the justification of this interpretation, see Ichino and Winter-Ebmer (1998).

Table 3
Cohort of Birth and Labor Earnings

	Austria	Germany	Sweden	Switzerland
Born 1925–29	.010	.032	−.005	−.013
	(.007)	(.036)	(.006)	(.017)
Born 1930–39	−.015**	−.019	.002	.010
	(.004)	(.021)	(.004)	(.014)
Born 1940–49	.010**	.007	−.001	.000
	(.004)	(.021)	(.003)	(.011)
Number of observations	22,871	1,302	2,474	742

NOTE.—For each country, the dependent variable is the residual of a regression of log hourly earnings on a cubic polynomial in age, estimated separately for males and females. The table reports the coefficients of the regression of these residuals on the three cohort dummies (without a constant) and on a gender dummy. The coefficient of the gender dummy, not reported, is always insignificant. Robust standard errors are in parentheses.
** The coefficient of the correspondent cohort is significantly different from the coefficient of the previous cohort at the 1% level.

In table 4 we show ordinary least squares (OLS) estimates and IV estimates obtained using the 1930–39 cohort indicator as an instrument. To eliminate disruptions from the trend over time, we use the same detrending measures as in tables 1 and 3. The earnings measure we use is therefore the residual of log hourly wages around a cubic gender-specific trend. Likewise, years of education are detrended by a cubic gender-specific trend. These estimates are comparable across countries. According to the OLS regression for Austria, workers obtain a wage premium of 9.5% for each year of education; this return increases to 10.1% in the IV regression. In Germany, the results are similar: 7.6% for OLS and 11.3% for IV. Although the IV estimates for the two countries are not far apart, they are not significant for Germany, and this may be due to the smaller sample size.

In order to interpret these results as estimates of the return to education for those who received less education because of the war, the assumptions for the identification of a LATE, described in detail by Angrist, Imbens, and Rubin (1996), have to be satisfied in this context. The first potential problem is generated by the possibility that the earnings of individuals born between 1930 and 1939 might have been influenced by other factors not related to the loss of education due to World War II: for example, these individuals were also born during the Great Depression, and this event might have had effects on earnings that should not be confounded with the effects due to the educational loss caused by the war.[22]

To control for this type of confounding factors, we pool the German and Austrian data sets together with the data sets from Switzerland and

[22] Using the terminology of Angrist et al. (1996), such a possibility would imply a violation of the hypothesis of *random assignment* or *ignorability*, because the instrument consisting in the cohort 1930–39 would be correlated with *nonignorable* confounding factors like the Great Depression.

Table 4
The Earnings Loss due to the Effect of War on Education

	Austria		Germany		All Countries	
	OLS	LATE-IV	OLS	LATE-IV	OLS	LATE-IV
Years of education	.095	.101	.076	.113	.068	.101
	(.001)	(.017)	(.005)	(.092)	(.001)	(.043)
Female	−.030	−.032	−.027	−.046	−.020	−.029
	(.004)	(.007)	(.037)	(.059)	(.004)	(.013)
Born 1930–39					−.007	.000
					(.004)	(.010)
Austria					.014	.020
					(.010)	(.015)
Germany					.010	.013
					(.016)	(.018)
Sweden					.010	.014
					(.011)	(.015)
Marginal R^2 for inclusion of instrument in first stage		.005		.003		.003
F-test for inclusion of instrument in first stage		94.8		4.1		77.3
R^2	.258	.257	.145	.111	.186	.145
Number of observations	22,871	22,871	1,299	1,299	27,386	27,386

NOTE.—In each column, the dependent variable is the residual of a regression of log hourly earnings on a cubic polynomial in age, estimated separately for males and females. Likewise, years of education are residuals from a gender-specific cubic trend. For Austria and Germany, the instrument in the IV regressions is the cohort 1930–39 dummy. For the pooled IV regression on all countries, the instrument is the interaction between the cohort 1930–39 dummy and the dummies for Germany and Austria. All the regressions also include a constant. Robust standard errors are reported in parentheses. OLS = ordinary least squares; LATE-IV = local average treatment effect (LATE) interpretation of instrumental variables (IV) techniques.

Sweden.[23] These two countries did not take an active part in the war and can be considered relatively similar to Germany and Austria from several points of view, including the fact that their economies were already fairly integrated with the German and Austrian ones before World War II. Furthermore, as we know from table 1, the war had no effect on the educational attainment of the cohort born in Sweden and Switzerland between 1930 and 1939, but this cohort is likely to have shared with the analogous Austrian and German one many cohort-related confounding factors as, for example, the Great Depression. Therefore, by adding samples from these two countries, the quality of our control group improves considerably, because it now includes not only individuals born in different cohorts of the same country but also individuals born in the same cohort of different countries. Moreover, we control for country-specific effects, and we include a dummy for the cohort 1930–39. This dummy should control for cohort-specific influences on earnings that are unrelated to the war.

In the corresponding IV estimation, the instrument is defined as the intersection of the following two events: "being born in Austria or Ger-

[23] Separate results for Switzerland and Sweden are not shown in table 4 because, as expected, the first-stage regressions turned out to be utterly insignificant.

many" and "being born between 1930 and 1939." The product of the two dummies denoting these conditions is, therefore, the instrument for years of schooling. Note that this specification has the conventional difference-in-difference form, where country effects and cohort effects are eliminated. The IV estimator should therefore give us the earnings loss of individuals who reduced educational attainment because they were born between 1930 and 1939, and were born in Austria or Germany. As far as war-independent cohort effects are similar across countries, this estimator should pick up the impact of the war only.

These pooled-countries results, presented in table 4, corroborate the evidence described separately above for Germany and Austria: our IV estimator based on cohort information is indeed capturing the impact of the war, controlling for potential confounding factors correlated with the cohort dummy. The IV coefficient is very similar to the result for Austria and Germany and is precisely estimated. On the other hand, the cohort effect "being born between 1930 and 1939" is insignificant—actually it is zero—as are the three country dummies.

A second source of problems is represented by the possibility of general equilibrium effects that would bias the LATE interpretation of our IV estimates. For example, the shortage of educated individuals caused by the war may have increased returns to schooling in all cohorts and, for this type of bias, using Sweden and Switzerland as a source of control observations would not help.[24] Welch (1979) and Berger (1985) argue that cohort size might be important for earnings, whereas Card and Lemieux (2001) especially concentrate on the impact of cohort size on returns to education. More specifically, if individuals born in different years are imperfect substitutes, the small size and the low average education of a specific cohort could significantly raise the returns to education for precisely that cohort. This is relevant in our case because we are interested in returns to education for the war cohort in particular.

To address this issue we use the Austrian census (total population), where we can construct measures of cohort size and average educational attainment of different cohorts and types of workers. For each individual in our data set we define the relevant labor market (i.e., the one whose supply conditions matter for the individual) as a cell defined by 2 genders, 76 industries, and 91 counties.[25] Using data for 1981, we construct for each of these cells the mean years of education of the war cohort and of the nonwar cohort, as well as the size of the war cohort relative to total

[24] Again using the terminology of Angrist et al. (1996), we would have in this case a violation of the Stable Unit Treatment Value Assumption (SUTVA).

[25] Similar results emerge if the relevant labor market is defined more broadly, i.e., along the dimensions of gender and industry or gender and county alone. These results are available from the authors.

Table 5
The Earnings Loss due to the War, Controlling for Cohort Effects in Austria

	1	2	3	4	5
A. OLS:					
Years of education	.094	.094	.094	.094	.093
	(.001)	(.001)	(.001)	(.001)	(.001)
Female	−.029	−.027	−.029	−.033	−.032
	(.005)	(.005)	(.005)	(.005)	(.005)
Mean years of education of war cohort		.019			.005
		(.003)			(.004)
Mean years of education of nonwar cohort			.031		.028
			(.004)		(.005)
Relative size of war cohort				.132	.128
				(.025)	(.025)
R^2	.259	.261	.262	.261	.263
B. LATE-IV:					
Years of education	.097	.094	.092	.096	.091
	(.017)	(.018)	(.018)	(.018)	(.018)
Female	−.030	−.027	−.029	−.033	−.032
	(.007)	(.007)	(.007)	(.007)	(.007)
Mean years of education of war cohort		.019			.005
		(.004)			(.004)
Mean years of education of nonwar cohort			.031		.028
			(.004)		(.005)
Relative size of war cohort				.129	.131
				(.032)	(.032)
R^2	.259	.261	.262	.26	.263

Note.—In each column the dependent variable is the residual of a regression of years of education on a cubic polynomial in age, estimated separately for males and females. All models also include a constant term. Robust standard errors are reported in parentheses. Mean years of education of the war and the nonwar cohort, as well as the relative size of the war cohort, are based on the Austrian census data (total population) for 1981. These indicators are constructed for each cell defined by a combination of 2 genders, 76 industries, and 91 counties. The smaller sample size as compared to table 4 arises from missing data in some cells. OLS = ordinary least squares; LATE-IV = local average treatment effect (LATE) interpretation of instrumental variables (IV) techniques. The number of observations in all the columns of this table is 20,769.

labor supply in the cell. The OLS and IV estimates from table 4 are thus augmented with these three cohort indicators. Results in table 5 are reassuring.[26] In panel A, the OLS estimates of the effect of years of education do not change when these cohort indicators are included alone or all

[26] Because of missing information for some cells, these results are based on a slightly smaller number of observations with respect to table 4, with no substantial consequences for our estimates. In the case of OLS, the comparison between col. 1 (panel A) in table 5 and col. 1 (Austria, OLS) in table 4 allows one to assess the effect of the loss of observations, while the comparison across columns of table 5 informs on the consequences of controlling for the cohort indicators. Similarly, for the IV estimates, in which case the comparison between col. 1 (panel B) of table 5 and col. 2 (LATE-IV, Austria) of table 4 indicates the effect of the loss of observations.

together. A slightly larger, but still insignificant, effect of these indicators on returns to schooling appears in the IV estimates of panel B, particularly when the cohorts indicators are jointly included. Comparing columns 1 and 5 of this panel, the estimated coefficient drops from 9.7% to 9.1%. Within a LATE interpretation, this finding is expected, given that the cohort indicators are precisely meant to control for general equilibrium effects that might overstate the returns to schooling of the cohort that received less education because of World War II.[27]

A crucial further assumption to interpret our IV results as estimates of the long-run educational cost of the war requires that World War II must have no effect on future labor earnings other than through the reduction of schooling. To be more precise, on the one hand, the war should not have any effect on the workers whose education decision would be the same independent of the war. On the other hand, for those workers whose education decision would be changed by the war, the reduction in years of schooling should be the only channel of effects on earnings. Psychological disorders and malnutrition of children growing up during the war could be a cause of failure of this assumption inasmuch as they represent potential channels through which the war directly influences future labor incomes independent of schooling. This problem could be serious immediately after the war. But in our sample, we observe earnings only in the 1980s. Therefore, it seems implausible to imagine earnings consequences of psychological disorders and malnutrition still in effect some 40 years after the war.[28]

A final assumption for the LATE interpretation of IV requires that no one would be induced by the war to receive more schooling but would, instead, take less schooling in the counterfactual case in which he or she were not affected by the war.[29] This assumption is supported, in our context, by the evidence of Section II, which indicates that our war instruments are associated with a significantly lower educational attainment. In any case, it seems implausible that children who would have chosen

[27] Note also that the coefficients of the cohort indicators are positive and generally significant but cannot be easily interpreted in terms of structural effects. For example, the positive effect of the mean years of education of the two cohorts might suggest the existence of positive human capital externalities, but Acemoglu and Angrist (2000) as well as Moretti (1999) warn against this interpretation, particularly in the absence of credible exogenous sources of variation of local average education.

[28] In the terminology of Angrist et al. (1996), this would be a violation of the *exclusion restriction* assumption.

[29] This is called the *monotonicity* assumption.

Table 6
The Earnings Loss due to the Effect of War on Education in Germany

	OLS 1	LATE-IV 2	LATE-IV 3	LATE-IV 4	LATE-IV 5
A. Regressors:					
Years of education	.072	.094	.096	.087	.162
	(.003)	(.033)	(.034)	(.027)	(.055)
Father with high school degree	.060	−.005	−.009	−.017	−.197
	(.026)	(.099)	(.10)	(.080)	(.159)
Father blue-collar worker	.008	.025	.026	.020	.075
	(.017)	(.027)	(.028)	(.025)	(.044)
Living in big city until age 15	.055	.045	.045	.049	.017
	(.017)	(.022)	(.022)	(.021)	(.030)
Female	−.036	−.042	−.043	−.040	−.060
	(.022)	(.023)	(.024)	(.023)	(.026)
Born 1930–39			.012	.011	.023
			(.017)	(.017)	(.019)
B. Instruments:					
Father in war		Yes	Yes	Yes	
Father in war and father without high school degree				Yes	
Born 1930–39 and father without high school degree					Yes
Marginal R^2 for inclusion of instrument in first stage		.005	.004	.007	.004
F-test for inclusion of instrument in first stage		28.3	26.8	17.0	20.7
R^2	.121	.112	.11	.117	

Note.—These regressions pool together the available observations for the years 1984, 1985, and 1986 of the German sample. In each column, the dependent variable is the residual of a regression of log hourly earnings on a cubic polynomial in age, estimated separately for each year and for males and females. Likewise, years of education are residuals from a gender-specific cubic trend. Robust standard errors, adjusted for within-individual correlation, are reported in parentheses. OLS = ordinary least squares; LATE-IV = local average treatment effect (LATE) interpretation of instrumental variables (IV) techniques. The number of observations in all the columns of this table is 4,142.

a lower education level in the absence of the war constraint would reach a higher education level if constrained by the war.[30]

To assess the robustness of our results, we exploit further the German data set in which more information on the impact of the war is available. In table 6, we pool earnings data for the years 1984, 1985, and 1986 to smooth possible variations in earnings of individuals over time.[31] We also

[30] Since the beginning of the war, students could not avoid conscription by studying longer. On the contrary, the only way to escape from the military was to stop school and work in an armament factory or (until 1941) to work as a self-employed farmer. Therefore, violations of this assumption can practically be ruled out. There is, however, the case of young men who were prematurely dispatched to the front without having finished their high school degree. From 1941, in these cases, young men in the final year of high school received permission to continue to study without having formally graduated from high school (Engelbrecht 1988, p. 336). Unfortunately, information on the number of permissions given is missing.

[31] Note that in these regressions standard errors are adjusted for within-individual serial correlation of error terms. The use of instrumental variables should take care of the potential correlation between individual specific fixed effects and the included regressors.

add information on parental background and on the type of community the student grew up in until age 15. In column 1, the OLS estimate is replicated from table 4. In column 2, the fact that the student's father served actively in the war is used as an instrument for education. The LATE-IV coefficient implies that wages are 9.4% higher for each additional year of education, and is highly significant. This result does not change if the 1930–39 cohort dummy is added as a further regressor. Note that, with this additional variable, we can control directly for cohort effects in the earnings regression while still being able to assess the educational attainment effects of World War II.

This direct control for possible cohort effects is again applied in columns 4 and 5, where interactions of instruments are used, and the significance and size of our estimates of returns to schooling are not weakened. In column 4, the instruments are the father-in-war dummy alone and interacted with the indicator of low parental education. In the last column, instead, the cohort dummy is interacted with parental education to obtain an instrument that delivers an estimate of returns to schooling that is quite precise and as high as 16.2% for each year of education.

Since IV estimates may be significantly biased in small samples, we report in tables 4 and 6 the marginal R^2 as well as the F-test statistic for the inclusion of the instruments in the first-stage regressions.[32] Following Staiger and Stock (1997), the reciprocal of this F-test approximates the fraction of the OLS bias with respect to the LATE, of which IV still suffers in a finite sample. When we use only the cohort instrument (table 4), this fraction is approximately 24% for Germany but only 1% for Austria and 1.3% in the pooled-countries sample. The use of more direct war instruments in table 6 reduces this bias considerably for Germany as well, reaching a low of 3.5% in column 2, where the father-in-war dummy is assumed to capture the exogenous source of variation of educational attainment.

Summing up, we find that more direct indicators of war constraints result in somewhat higher returns to education for the respective group. The LATE estimators range between 8.7% and 16.2% and are up to twice as high as the corresponding OLS estimators. These results can easily be reconciled with the idea of heterogeneous returns to education (Card 1995, 2000). Moreover, the local average treatment effect concept gives a precise meaning to these heterogeneous returns: the LATE measures exactly the returns for the group that changes treatment status (i.e., educational attainment) because of the war. In our case, these are predominantly poor individuals, with returns that are probably higher at the margin. If the

[32] See Bound, Jaeger, and Baker (1995).

war constrains their choices, they have to reduce education, losing disproportionately more than the average.[33]

IV. The Long-Run Educational Cost of World War II in Austria and Germany

On the basis of the parameters estimated for Germany and Austria, we are now able to calculate three different measures of the cost of World War II. The first measure, that we indicate with COST1, is the LATE itself in percentage terms: it measures the average income loss due to the war for those who reduced their educational attainment by 1 year just because of the war. Formally, if i denotes individuals, Y_i is labor earnings, S_i is years of schooling, and Z_i is the (binary) instrument capturing the presence of a potential war influence, this measure is given by

$$\mathrm{COST1} = \Delta_Z = \frac{\mathrm{Cov}\,(Y_i;\ Z_i)}{\mathrm{Cov}\,(S_i;\ Z_i)}. \tag{1}$$

This is the measure to be used if we want to interpret our results in a structural way, that is, if we want to estimate what would be the individual earnings loss attributable to a constrained educational decision when the latter is due to an increase of constraints similar to the one produced by World War II. Note again that the effect will be different according to the instrument we use.

The second measure, COST2, calculates the average impact of the war on the earnings of an individual in the group potentially affected by the war. Depending on the specific instrument, this is the group of individuals born between 1930 and 1939, or the group of individuals having a father in the war, and so forth:[34]

$$\mathrm{COST2} = E(Y_i \mid Z_i = 1) - E(Y_i \mid Z_i = 0). \tag{2}$$

[33] See Ichino and Winter-Ebmer (1999) for a more elaborate argument and also for a first attempt to calculate upper and lower bounds of returns to education based on the use of different instruments.

[34] Note that this measure is nothing more than the numerator of the LATE in percentage terms. Without loss of generality, it is easy to see this in the case of a binary schooling indicator. Let Y_{i1} and Y_{i0} denote labor earnings in the two counterfactual situations of high and low education and D_i be the binary schooling indicator. The LATE is then given by

$$\begin{aligned}\Delta_Z &\equiv E(Y_{i1} - Y_{i0})\\ &= \frac{E(Y_i \mid Z_i = 1) - E(Y_i \mid Z_i = 0)}{E(D_i \mid Z_i = 1) - E(D_i \mid Z_i = 0)}\\ &= \frac{\mathrm{Cov}\,(Y_i; Z_i)}{\mathrm{Cov}\,(D_i; Z_i)},\end{aligned}$$

where observed labor earnings can be expressed as $Y_i = D_i Y_{i1} + (1 - D_i) Y_{i0}$.

It therefore measures the effect of the war instruments on the earnings of the individuals at risk of being affected by the war. Under the assumptions that are necessary to identify the LATE, this effect takes place only through the distortion of educational choices.

A third interesting concept is suggested by the comparison between the average earnings loss of all the individuals in the group potentially affected by the war and the average income in the population. The ratio between the sample statistics that correspond to these two quantities, COST3, approximates the fraction of GDP that were lost in the year of the survey, because of the distortion of educational decisions induced by our war instruments:

$$\text{COST3} = \frac{(\text{COST2}\ Y_A)\Pr(Z_i = 1)}{\bar{Y}}, \tag{3}$$

where Y_A is the average income of the individuals at risk of being affected by the war (e.g., those born between 1930 and 1939, when the cohort indicator is used as an instrument) and $\overline{Y}$ is the average income in the population.[35] Of course, a more detailed calculation could, in principle, aggregate the earnings losses in the years from 1946 up to the survey year. This exercise—if it were possible—would only give a spurious increase in precision because from our regressions we know nothing about the time path of the earnings losses.

Table 7 reports these three measures of the cost of World War II for Germany and Austria. Beginning with Germany, the computation of each measure is performed separately for each of the four instruments used. In terms of COST1, the immediate costs for those who reduced schooling because of the war are within a narrow range of 9.4%–16.2%, with higher costs for children of less educated parents. If we look at the average impact on the individuals at risk of being affected by the war, COST2, the results are more diverse: whereas those who had a father in war lose, on average, approximately 9% of their earnings, those born in the cohort 1930–39 lose only 3%–4%. These results suggest that the first group suffered, on average, more binding constraints than the second: either a larger fraction of individuals in the first group were, indeed, forced to refrain from higher education, or those who did it reduced their years of schooling by a larger amount, or both.

If we finally calculate COST3, the percentage loss of GDP due to the

[35] Under the LATE interpretation of IV, instead of Y_A, it would be more appropriate to use the average income of the individuals who actually change behavior because of the instrument (i.e., the compliers in the Angrist, Imbens, and Rubin terminology). As this quantity is not observable, we approximate it by the average income of all those potentially affected by the war. Note that under a more traditional interpretation of IV, this difference should not matter.

Table 7
Three Measures of the Educational Cost of World War II

Instrument	COST1	COST2	COST3
A: Germany:			
1930–39 cohort	11.3	3.12	.88
Father in war	9.4	9.50	.21
Father in war and without high school degree	10.3	8.70	.17
Born 1930–39 and father without high school degree	16.2	4.23	1.06
B: Austria:			
1930–39 cohort	10.1	2.44	.73
1930–39 cohort (controlling for cohort size and education)	9.1	2.20	.66

NOTE.—COST1 is the average percentage income loss for the individuals who lose 1 year of schooling because of the correspondent war instrument. COST2 is the average percentage income loss for all the individuals for which the corresponding war instrument takes the value of one. COST3 is the percentage loss of GDP attributable to the educational effect of the corresponding war instrument in the year of the survey. The formal definitions of these variables are given in Sec. IV. All calculations are based on the data and the estimates described in tables 4, 5, and 6.

educational distortion caused by World War II, we find significantly higher costs if we capture the effect of the war with the simple cohort measure as compared to capturing it with the father-in-war dummy. For the former, a loss of 0.88%–1.06% of GDP in 1984–86 can be attributed to the lower educational attainment of the war cohort. For the latter war indicator, the loss only adds up to approximately 0.2% of GDP. These different estimates of the GDP loss are likely to be due to the fact that the proportion of the students having had a father in war was smaller than the relative size of the 1930–39 cohort. Moreover, the cohort dummy captures a wider set of war effects than the one captured by the other instrument. Having said that, it seems plausible that the losses—calculated with, for example, the father-in-war instrument—represent a lower bound of the overall educational cost of World War II. In addition to the effects that we can observe directly (e.g., having a father involved in the conflict or dead because of it), the war might also have reduced the educational attainment and earnings of those whose father did not serve actively in the war but was imprisoned, or restricted in professional life, and of those who were harmed by bombing, and so forth. All these additional effects go in the same direction but are not captured by our estimates.

Table 7 also shows that the cost measures for Austria are remarkably similar to the German ones, when the 1930–39 cohort dummy is used as an indicator of the war effect. Austrians who were part of this cohort and changed educational attainment because of the war lost 10.1% of earnings per year of reduced education. Moreover, the average effect for the entire group at risk of being affected by the war was 2.44%, whereas the average loss in terms of Austrian GDP during 1981–85 amounted to 0.73%. For Austria, we can also assess the importance of controlling for cohort effects (size and average education) using the results displayed in table 5. Using these somewhat lower estimates reduces the calculated cost of World War II only slightly—for example, the average loss in terms of Austrian GDP falls from 0.73% to 0.66%. The possibility to control for these cohort effects in Austria, which is not available in typical data sets for the estimation of returns to schooling, suggests that the bias generated by the omission of these general equilibrium effects is not large.

The evidence based on the three measures presented in this section conveys one clear message: the cost of World War II in terms of GDP appears substantial even 40 years after the end of the conflict. This conclusion can hardly be disputed, given the evidence. More debatable is, instead, the identification of the channel through which the war caused these GDP losses. Under the assumptions that are necessary for the identification of the LATE, these measures capture the cost—in terms of earnings—of the educational loss induced by the war. If these assumptions are not satisfied, in the sense that our instruments affect earnings also through channels that add to the educational losses (e.g., malnutrition), these measures also incorporate the effects of these additional channels.

V. Conclusions

Apart from all other—human, financial, and emotional—costs, World War II led to a significant drop in the educational attainment of individuals who were of elementary school age during or immediately after the conflict. This is the main conclusion of this article. Comparing the evidence for four countries, of which two were directly involved in the conflict (Austria and Germany) and two were not (Sweden and Switzerland), the magnitude of this educational loss is in the order of approximately 20% of a year of schooling. This is our estimate of the effect of being born during the thirties, as opposed to being born in the previous or subsequent decades for the two German-speaking countries.

We think that this cohort effect captures the effect of World War II because there is no evidence of a similar effect for the same cohort in Sweden or Switzerland, and the direct involvement in World War II is the only major potentially relevant difference between Austria and Germany and these other two countries. If confounding factors such as, for example, being born during the Great Depression were causing the cohort effect observed in Austria and Germany, a similar effect should also be observed in Sweden and Switzerland, but this is clearly not the case. Moreover, our German data set allows us to measure more directly war-related potential causes of educational losses, such as, for example, the father's involvement in the army during the war or his death between 1940 and 1945. When these more direct indicators are included in our regressions, the cohort effect tends to vanish, suggesting that the cohort variable is indeed capturing the educational consequences of the war.

In addition to this significant educational loss, World War II seems to have caused an earnings loss that is still noticeable in the 1980s. In Austria and Germany the magnitude of this effect can be situated in the order of 0.8% of GDP, when we use birth in the thirties as the proxy for the existence of war effects. Although more direct but less encompassing indicators of war involvement—like father's participation in the conflict—lead to lower losses, the persistence of a sizable earnings effect some 40 years after the end of the conflict appears hardly debatable. Note, in particular, that no comparable earnings loss is observable for Sweden and Switzerland.

The channel through which World War II caused this earnings loss is less obvious. We believe there is sufficient evidence to conclude that the most likely channel is the educational loss induced by the war. If this were the case, our IV estimates could be interpreted as estimates of the average earnings loss experienced by those workers who received less education just because of the war.

There are, however, reasons suggesting the existence of relevant channels of war effects on earnings that have less to do with education. For example, the reduced size and educational attainment of the war cohorts could have increased returns to schooling in all cohorts and, in particular, in the war cohort itself, thereby biasing our estimates. However, results for Austria show that controlling for these cohort effects does not change the results much. Another potential confounding factor is represented by the fact that the war cohort might have suffered not only an educational loss but also psychological disorders or malnutrition. We find it hard to believe, however, that these effects may persist in earnings observed 40 years after a war. Instead, we find it very likely that the educational choices made because of World War II might have had long-lasting effects. At least a first-order component of the observed earnings losses must be due to the distortion of educational choices that took place during the war. An additional potential problem of our preferred interpretation of the earnings loss is that the quality of education might have been lower during World War II, reducing the earnings of students trained in that period. But in this case we would still be capturing a dimension of the educational effect of the war, albeit a different one.

We therefore conclude that our estimates do capture the loss of earnings for individuals who received less education just because of the constraints imposed by World War II. These estimates can be used to infer the long-run educational cost of impediments to educational attainment similar to the ones imposed by a war. A possible relevant example is represented by the constraints faced by students whose fathers are unemployed, in jail, or missing for other reasons, or by students living in areas hit by earthquakes. Extrapolating from the evidence presented in this article, it seems possible to say that actions aimed at increasing the educational attainment of these individuals may save them from suffering substantial and long-lasting earning losses.

Appendix A

The Data

The data sources are as follows.

Austria: The Mikrozensus 1981, 1983, and 1985 surveys provide a 1% sample of the Austrian population. For the calculation of the education residuals, we included two dummy variables (1949, 1952) to capture increases in the minimal school-leaving age. Cohort size and mean years of education were calculated from the Austrian census 1981 (total population).

Germany: Socioeconomic panel, waves 1–3 (1984–86); information on

Table A1
Descriptive Statistics for the Variables Used in the Analysis

	Austria	Germany	Sweden	Switzerland
Female	.53	.52	.52	.52
Born in 1920–29	.33	.28	.27	.28
Born 1930–39	.33	.35	.30	.32
Born 1940–49	.34	.36	.42	.40
Years of education	9.05	11.07	10.16	10.96
	(1.77)	(2.23)	(3.66)	(3.31)
Log of hourly earnings	4.06	3.00	1.41	1.59
	(.44)	(.56)	(.20)	(.75)
Living in big city at age 14		.24		
Father served in World War II		.03		
Father dead in 1940–45		.12		
Father blue-collar worker		.38		
Father has high school degree		.08		
Born 1930–39 and father died 1940–45		.05		
Born 1930–39 and father without high school degree		.33		
Born 1930–39 and father in war		.02		
Father in war and without high school degree		.03		
Mean years of education of war cohort	8.65			
	(.75)			
Mean years of education of nonwar cohort	9.23			
	(.57)			
Relative size of war cohort	.23			
	.08			
Sample size	72,128	3,572	3,364	1,236

NOTE.—For each country, the table reports the means of the variables used in the analysis. For nonbinary variables, the standard deviations are also reported in parentheses. The log of hourly earnings is available only for the employed workers, and, therefore, the sample sizes are smaller: 24,423; 1,337; 2,808; and 837, respectively, for the four countries. These are also the relevant sample sizes for all the tables in which the earnings information is used.

parental background and childhood events was taken from the bioparenfile of the German Socio-Economic Panel.

Switzerland: Einkommens- und Vermoegensstichprobe survey (1982).

Sweden: Swedish Survey of Household Market and Nonmarket Activities—HUS project (1984) and Swedish Level of Living Survey (1981).

Foreign citizens, self-employed persons, and persons with missing educational information were excluded from the samples. Table A1 reports, separately for each country, the means and standard deviations of the variables used in the analysis.

Appendix B

The Andrews Test for a Break with Unknown Break Point

Consider a stationary outcome y_t. Let β_t be the parameters of the model that explains the outcome.

$$H_0 : \beta_t = \beta_0 \qquad \forall\, t$$

$$H_A(\pi) : \beta_t = \begin{cases} \beta_1 & t = 1, 2, \ldots, \pi T \\ \beta_2 & t = \pi T + 1, \ldots, T. \end{cases} \tag{B1}$$

The test statistic is constructed as follows.

1. Calculate the restricted log likelihood under H_0:

$$l_R(\beta_0).$$

2. Calculate the log likelihood under the hypothesis of a break at the earliest possible break point, for example, when $\pi = \pi_{\min}$:

$$l_{\pi_{\min}}(\beta_1, \beta_2).$$

3. Calculate the corresponding likelihood ratio test statistic:

$$\lambda_{\pi_{\min}} = -2[l_R(\beta_0) - l_{\pi_{\min}}(\beta_1, \beta_2)].$$

4. Repeat for each possible break point and calculate the test statistic for each $\pi \in (\pi_{\min}, \pi_{\max})$.
5. Compute

$$\lambda_{\pi^*} = \sup_{\pi \in (\pi_{\min}, \pi_{\max})} \lambda_\pi.$$

6. Compare with critical values.

Note that since it is not possible to search for a break from the very beginning of the sample, or until the very end, the trimming parameters $\pi_{\min}$ and $\pi_{\max}$ specify how far into the sample one starts looking for a break and how early one stops. Andrews (1993) tabulates critical values for this test statistic. Note also that instead of imposing when a structural break occurs, the procedure allows one to determine the most likely period in which it happens.

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