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An Example of Local Average Treatment Effect Estimation

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An Example of Local Average Treatment Effect Estimation

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Abstract

An important component of the long run cost of a war is the loss of human capital suffered by children in schooling age who receive less education because of the war. This paper shows that in the European countries involved in WWII, children who were ten years old during the conflict were significantly less likely to proceed into higher education. On the contrary, we find no effect for individuals in the same cohorts living in countries not involved in the war. Using data for Austria, Germany, Sweden and Switzerland, we estimate the cost of the war in terms of earning losses suffered by those individuals who did not reach higher education because of the conflict and we compute the implied loss of GDP for their countries. In order to identify this cost, we interpret our Instrumental Variable estimates as measures of the Local Average Treatment Effects (Angrist and Imbens, 1994) of education connected to our war instruments. Inasmuch as WWII has caused an increase of liquidity constraints for families with children in schooling age (e.g. the absence of the father), our estimates may be considered as measures of the long lasting income losses that could be avoided in peaceful times by exogenously increasing the educational attainment of children in families subject to constraints similar to those caused by the war (e.g. families with single mothers).

Keywords: Returns to Schooling, Wage Determination, Endogeneity, Self Selection
JEL Classification: J31, I21, J24, C25

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1 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 in schooling 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 in primary schooling age. For these cohorts, in particular during wars that hit severely the civilian population, the 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 liquidity constraints and prevent an otherwise feasible transition into higher education even when the war is over.

In Section 2 of this paper we provide evidence on these effects for some countries involved in World War II (WWII hereafter). In particular we show that in countries in which the civilian population has been more severely affected by the war, children who were ten years old during the conflict were also significantly less likely to reach higher educational degrees later on. The comparison group is of course made of those children who had that same age in other peaceful years. By contrast, we also show that in other countries in which the population was less severely affected by WWII, the same cohort's educational attainment is unchanged. We discuss whether other reasons, different from the war, might explain these facts and we conclude that the disruption of the education process caused by the military events is the most likely explanation of the observed evidence.

Having established that these educational effects of WWII exist, the following question is to evaluate their relevance. One way to do this is to measure the average earnings loss suffered by those children who, just because of the war, did not reach the educational degree that they would have preferred and had to stop before. The total amount of these losses in a given year indicates how much higher GDP could have been if the war had not had the observed educational effect.

The interpretation of Instrumental Variables suggested recently by Angrist and Imbens (1994) [1] allows us precisely to identify and estimate non–parametrically the effect that we would like to measure. Within a treatment–outcome framework in which participation to treatment is subject to self–selection, they provide the conditions that an instrument has to satisfy in order to identify (from non–experimental data) the average treatment effect for those who are induced to change participation status by a change in the instrument. This is what they define as the Local Average Treatment Effect (LATE).

In our paper the outcome is represented by labor earnings while the treatment consists in refraining from higher education. Participation into treatment is obviously subject to self–selection for a variety of well known reasons analyzed in the existing literature[2]. In Section 3 we discuss how the interpretation of IV proposed by Angrist and Imbens (1994) applies to our treatment-outcome framework. We also discuss the conditions under which the natural experiment represented by WWII provides the in-

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

[2] From the pioneering article of Griliches (1977) to the recent survey of Card (1994). See also the less recent survey by Willis (1986) that covers more exhaustively the existing literature up to 1986.

struments for the identification of the earnings losses suffered by those who would have reached higher educational degrees in normal conditions but had to drop out of school because of the war. This is a Local Average Treatment Effect because it is the average variation in the outcome for those who changed treatment status because of a change in the instrument. Note, that different LATE estimators arise in the case of different instruments. In sections 4 and 5 we apply this procedure to data for Germany and Austria. We also extend the analysis to Sweden and Switzerland (two countries which were not involved in the war) in order to enlarge and improve the quality of our control sample.

Under the conditions that Angrist and Imbens (1994) require for the identification and estimation of LATEs using IV, this is the only average return to schooling that we can identify with our instruments and our samples. However, this is precisely the average return in which we are interested given that our goal is to measure the educational cost of WWII. Heckman (1997, p. 454) suggests that such local average treatment effects may be problematic to interpret because "when parameters are defined to be instrument dependent, and they are defined for unobserved subsets of the population (those who would have changed state if their Z [i.e. the instrument, our addendum] were changed while their unobservables were held fixed), it is no longer clear what interesting policy question they answer".

However, Card (1994) gives very convincing theoretical reasons to support the idea that estimates of returns to schooling should be sensitive to the instruments used in the empirical analysis: he shows that the OLS downward bias (with respect to IV) estimated by several authors for the US may be explained by the choice of instrument[3] in the presence of heterogeneity of marginal costs and benefits of education in the population. Our paper follows up on the analysis of Card (1994) in showing a different context in which the Local Average Treatment Effect answers a well–posed economic history problem.

We believe that our results may also have a policy interest beyond economic history. It would clearly be too much to hope that our estimates may convince governments, at the margin, to stop the wars in which their countries are involved or not to start new ones. But in the concluding Section 6 we discuss in which other sense the Latin proverb "Historia magistra vitae" may apply to our results. Inasmuch as the observed decrease in educational attainment during WWII can be explained by a significant increase of liquidity constraints for families with children in schooling age, our estimates may be interpreted as a measure of the long–run educational cost of a similar increase of liquidity constraints in peaceful times.

One could say that we could have simply estimated the overall effect of the war on earnings if the goal were just to estimate the income loss due to the war. This estimate would have been enough to extrapolate the cost of similar increases in liquidity constraints in peaceful times. However, the structural estimation of the earnings losses due to the educational effect of the war is more interesting for policy purposes. While

[3]In particular, quarter of birth in Angrist and Krueger (1991a), lottery number $*$ year of birth in Angrist (1990) presence of sisters in Butcher and Case (1994), tuition in 2–yr and 4–yr colleges in state and distance to nearest college in Kane and Rouse (1995), nearby college in county of residence in Card (1993). See also Kalwij (1996) for an analysis for the Netherlands.

removing the cause of the liquidity constraint (e.g. the loss of a father) may be unfeasible, removing its educational effects may be feasible. Our estimates can be interpreted as measures of the long lasting income losses that could be avoided by exogenously increasing the educational attainment of children in families subject to constraints similar to those caused by a war.

2 The Effect of WWII on Educational Attainment

If one were to search for educational effects of wars, the first place to look would be veterans. Most veterans, are exposed directly to combat activity and they may be physically or psychologically wounded while being still in their schooling age. Nonetheless many military jobs may provide skills that are also transferable to the civilian labor market.[4] The problem is further complicated by the fact that in some cases veterans are entitled to preferential treatment in education after conscription [5] or have been able to gain educational or training degrees already during their duty.

To avoid problems with veterans, we concentrate on persons who were 10–15 years old during the war. At this age, they faced no risk of conscription. At the same time, age 10 was — and still is at least in Germany and Austria on which we will focus more closely in the econometric analysis of section 4 — a crucial age 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; most of them can be circumscribed as liquidity constraints. Financial means for schools are in general lowered, transportation becomes more difficult, etc. Moreover, if the father serves actively in the war, the family situation is certainly unfavorable with respect to schooling. Apart from these financial constraints, the children might also act as a substitute bread winner and start working earlier.

In order to establish whether these kinds of educational effects exist also for WWII, we apply a uniform specification to the data described in Ganzeboom et al. (1992). This dataset contains comparable information on education attainment for cross–sections of parents and children in different countries. For each of these countries we estimate a Probit regression of the probability of not reaching a higher educational degree ($LOED_i$) on age, a dummy for the cohort born 1930–35 and a dummy for higher education of the father ($LOEDF_i$). [6]

$$Pr\left(LOED_i\right) = F(a_1 + a_2 BORN30\text{–}35_i + a_3 AGE_i + a_4 LOEDF_i) \qquad (1)$$

where F is the cumulative normal distribution. Table 1 presents only the coefficients for the war cohort dummy. To facilitate interpretation of the Probit estimates, the

[4]See Angrist and Krueger (1994) for an assessment of earnings effects of U.S. WWII veterans.

[5]For example, the U.S. GI Bill of Rights in 1944 entitled WWII veterans to subsidised education. In Germany and Austria no such programs existed.

[6]Higher education is defined as more than 8 years of schooling in all countries.

probability change for a change in the dummy variable is reported. In specification I the information on father's education is omitted. Countries in the upper panel have been actively involved in the war; in the lower panel results for some control countries — not participating in the war — are presented.

Results are remarkably significant for European countries who suffered most from the war. In Italy, Germany and Austria, the probability of dropping out of school is five to eight percentage points higher for war cohorts. Very similar results are obtained for the Netherlands and Hungary. Finland has similar point estimates, but because of the very small sample size they are not significantly estimated. The impact of the war on educational attainment is even bigger in the UK and Northern Ireland with a 10–17 percentage points increase. Finding a bigger impact in the UK than in continental European countries may appear peculiar. It can be explained, though, by the greater low–education rates in the latter. [7] In addition, it should be remembered that the UK experienced severe bombings during the war. Also in the US the effect of the war has the same sign although smaller in size and significant only in one specification. This should not be surprising, however, given that in this country the civilian population was less severely affected by the war and the age of 10 was less relevant for educational decisions.

At least in European countries, the cohort born between 1930 and 1935 seems to have significantly lower educational attainment than other cohorts. Can this effect be attributed to the war with certainty? Of course not. We can provide only circumstantial evidence in favor of this interpretation and the observed effect could also be caused by other cohort phenomena. For example, the cohort 1930–35 could be different from other cohorts because its members are born immediately after the Great Depression, i.e. in a period of very high unemployment world–wide. To investigate this possibility we include in Table 1 the available evidence for some other countries who have not participated in the war: Ireland, Thailand, Switzerland, Sweden, India and Brazil. In all these countries educational attainment in the cohort born between 1930 and 1935 is not lower than in other cohorts. The case of Ireland provides a particularly interesting piece of evidence in favour of our interpretation. We have very similar surveys of Northern Ireland and of the Republic of Ireland: both are from 1973, they have the same sample size and the same mean higher education rate. Yet, in Northern Ireland there is a sizeable impact of the war, while in the Republic of Ireland there is none.

¿From these observations we conclude that, in fact, liquidity constraints due to the war are of first order importance for an explanation of the observed drop in educational attainment. Yet, cohort effects might act as a "catch–all" variable. Therefore later, in the econometric analysis of section 4, in addition to drawing also control samples from non-war countries, we also include variables aimed at capturing other cohort effects and we use a more direct proxy of liquidity constraints represented by an indicator of whether the father of the potential student was actively involved in the war as a soldier. Before going into the empirical analysis, however, in the next section we discuss under which hypothesis the natural experiment represented by the war allows us to identify and estimate the Local Average Treatment Effect in which we are interested.

[7] See the mean of the left hand side variable in the table. For German–speaking countries, like Austria and Germany, these lower rates are mainly caused by the prevalence of vocational training of apprentices in firms.

3 The Identification and Estimation of Local Average Treatment Effects

Consider a sample of workers denoted by i and observed in period t who may have two possible levels of education: low and high. Let D_i be an observed binary indicator such that $D_i = 1$ if worker i has low education. Y_i indicates instead the (log of) labor income of worker i. Using a treatment–outcome interpretation, the treatment is in our case a reduction of education while labor income is the outcome.[8] The schooling decision is taken by these workers in a period s_i different for each worker but occurring before t. This decision is not random in the sense that each worker chooses his level of education on the basis of his (partially unobservable) idiosyncratic gain.[9] Therefore, using again the treatment–outcome terminology, participation into treatment is subject to self–selection.

We also observe a binary indicator Z_i that takes value 1 for those workers for whom WWII represented an additional constraint at the moment of choosing whether to continue towards higher education. Within a treatment–outcome framework, Z_i indicates the assignment to treatment. While in some controlled experiments in epidemiology assignment to treatment is random and compliance with assignment is enforced, in the case of natural experiments like the one considered in this paper assignment is not random and compliance is not perfect.[10] Leaving for later a discussion of random assignment, in our case compliance is certainly imperfect because the war constraint did not necessarily prevent the choice of higher education; vice–versa, among those who were not constrained by the war the choice of lower education has always been possible. Therefore, there is no one to one correspondence between assignment ($Z_i = 1$) and treatment ($D_i = 1$).

Yet there is evidence (see Section 2) that some workers were induced to change treatment status by the war (i.e. complied with the assignment) in the sense that because of the constraint imposed by the war they chose a lower education level. Our goal is to estimate the average loss of labor income suffered by these workers and this is precisely what Angrist and Imbens (1994) call a Local Average Treatment Effect.

In order to define this effect formally, note that labor income depends in general on education (the treatment) and on the existence of a war constraint (the assignment) and we denote this function as $Y_i(D_i, Z_i)$.[11] Let $D_i(Z_i)$ indicate the participation decision

[8]When a treatment–outcome framework is applied to the interpretation of returns to schooling, the treatment is usually defined as an increase of the amount of education received by an individual. But the natural experiment that we consider in this paper produces a decrease of education, of which we would like to measure the outcome in terms of income. For this reason it is more convenient to adopt the non–conventional definition of treatment described in the text.

[9]We restrict the empirical analysis to male workers in order to reduce possible biases generated by labor force participation decisions.

[10]Other natural experiments like the quarter of birth used by Angrist and Krueger (1991) or the distance to college used in Card (1993) are also characterized by non–random assignment and imperfect compliance.

[11]To simplify the notation, in this section we omit the explicit consideration of other potential determinants of labor income that will instead be considered in the empirical analysis. It should, however, be immediately stressed that given our goal the only covariates that will be considered are exogenous or predetermined with respect to the time s_i in which the educational decision is taken: for example, age and family background variables.

that worker i would take in the two possible situations defined by the war indicator Z_i. In this framework it is useful to imagine that for each worker the full sets of possible outcomes $[Y_i(0,0),\ Y_i(1,0),\ Y_i(0,1),\ Y_i(1,1)]$, of possible treatments $[D_i = 0, D_i = 1]$ and of possible assignments $[Z_i = 0, Z_i = 1]$ exist even if only one item for each set is actually observed. Given these sets of events, Angrist Imbens and Rubin (1996) propose the following useful classification of the population on the basis of the values $D_i(Z_i)$.

1. *Compliers*: These are workers for whom $D_i(0) = 0$ and $D_i(1) = 1$, i.e. workers who would choose the higher education level in the absence of the war constraint and the lower education level if constrained by the war.
2. *Never takers*: These are workers for whom $D_i(0) = 0$ and $D_i(1) = 0$, i.e. workers who would always reach a higher education level independently of the war and therefore who would never accept the reduction of education implied by the treatment.
3. *Always takers*: These are workers for whom $D_i(0) = 1$ and $D_i(1) = 1$, i.e. workers who would always stop at a lower education level independently of the war, and therefore who would take the treatment even if not assigned to it.
4. *Defiers*: These are workers for whom $D_i(0) = 1$ and $D_i(1) = 0$, i.e. workers who would stop at a lower education level in the absence of the war constraint, but would switch to a higher education level if constrained by the war.

Obviously, the categories 2, 3 and 4 include workers who do not comply with the assignment mechanism defined by the war. Among the *non–compliers*, defiers are those who do the opposite of the assignment.

Using the notation described above, the Local Average Treatment Effect due to the war constraint can be written formally as:

$$\Delta_Z = E\{Y_i(1,1) - Y_i(0,0)\} \tag{2}$$

Inasmuch as labor income is lower on average for the less educated, Δ_Z is negative. Furthermore, note that for each worker i who changes treatment because of the assignment, we observe either $Y_i(1,1)$ or $Y_i(0,0)$ but never both, because each worker has either a high or a low education level and was either constrained or not constrained by the war. Therefore, the argument of the expectation in equation 2 is never observed and we cannot estimate Δ_Z using its sample counterpart because the latter does not exist. In order to identify Δ_Z we have to rely on comparisons between different individuals.

Angrist and Imbens (1994) and Angrist, Imbens and Rubin (1996) discuss the assumptions that have to be satisfied in order to identify and estimate Local Average Treatment Effects using Instrumental Variables techniques. With specific reference to our natural experiment, these assumptions can be summarized as follows.

Assumption 1 (*Stable Unit Treatment Value Assumption*): potential incomes and education levels of each worker i are unrelated to the incomes, education levels and war status of other workers; thanks to this assumption we have been able to write, in the definitions given above: $Y_i(\{\mathbf{D}, \mathbf{Z}) = Y_i(D_i, Z_i)$ and $D_i(\mathbf{Z}) = D_i(Z_i)$ where $\mathbf{D}$ and $\mathbf{Z}$ represent the full vectors of assignments and treatments in the population.

Assumption 2 (*Random Assignment*): individuals have the same probability of being constrained by the war, i.e. $Pr\{Z_i = 1\} = Pr\{Z_j = 1\}\ \forall i$.

Assumption 3 (*Non-zero Causal Effect of Assignment on Treatment*): the probability of low education is higher for those who are constrained by the war, i.e. Z_i is such that $Pr\{D_i(1) = 1\} = E\{D_i \mid Z_i = 1\} > Pr\{D_i(0) = 1\} = E\{D_i \mid Z_i = 0\}$.

Assumption 4 (*Exclusion Restrictions*): the triple $Y_i(0, Z_i)$, $Y_i(1, Z_i)$, $D_i(Z_i)$ is jointly independent from Z_i; therefore the war affects incomes only through education and we can, from now on, write $Y_i(D_i, 0) = Y_i(D_i, 1) = Y_i(D_i)$.

Assumption 5 (*Monotonicity*): no worker reaches higher education if constrained by the war and stops at low education in the absence of the war constraint, i.e. $D_i(1) \geq D_i(0)$ for all i with strict inequality for at least some i.

Under these assumptions, Angrist, Imbens and Rubin show that the Local Average Treatment Effect of D on Y due to Z, i.e. the income loss suffered by workers who received less education because of the war, can be expressed as the following function of the moments of the joint distribution of (Y, D, Z):

$$\Delta_Z \equiv E\{Y_i(1) - Y_i(0) \mid D_i(1) = 1, D_i(0) = 0\} = \frac{E\{Y_i \mid Z_i = 1\} - E\{Y_i \mid Z_i = 0\}}{Pr\{D_i(1) = 1\} - Pr\{D_i(0) = 1\}}. \quad (3)$$

Substitution of the corresponding sample averages in the righ hand side of equation 3 gives an IV estimate of Δ_Z. Note that if labor income increases with education, $\Delta_Z < 0$ because $Y_i(1) - Y_i(0) < 0$ and the war causes a loss of income through its effect on education.

Most of the assumptions on which this result is based cannot be tested but this setup forces the researcher to focus closely on the plausibility of each of them within the specific estimation problem under consideration. In this section we characterize in general terms the nature of these assumptions within the setup of the natural experiment provided by WWII. In Section 4 we will discuss them with specific references to the actual indicators of war constraint that we will use as instruments.

Assumption 1 seems plausible but it can certainly be disputed in the case of our natural experiment. For example, the fact that a cohort in the population receives less education because of the war, may affect incomes of later cohorts inasmuch as there are complementarities between workers in different cohorts. Furthermore, the fact that fewer workers within a cohort reach higher education may increase the labor earnings of those cohort members who nevertheless hold higher degrees.

Since the year of birth of an individual is random one could say that in our case Assumption 2 (random assignment) is satisfied in the sense that the cohort of individuals most likely to be constrained by the war has been randomly chosen by nature. Yet, also this statement can be disputed for the following reason. Suppose that the war, instead of constraining a certain cohort of individuals, affects the educational decision of workers whose last name begins with the letters K and X. Furthermore, suppose that such names are predominantly of foreign origin and therefore the corresponding individuals are less likely to go to higher education because of language difficulties. In this case, assignment (the war constraint) would not be random with respect to a characteristic (foreign versus national origin) that cannot be ignored. Not only the war constraint but also the foreign nationality would make these workers more likely to stop at a low education level. Going back to our case, one can imagine several non–ignorable factors independent of the war but characterizing the cohort of individuals most likely to be constrained by the war. For example, these individuals are likely to be born during or right after the great depression and this may be relevant for the schooling decision independently of the war constraint. With specific reference to our indicators of war constraint, we will nevertheless assume, in the next section, that, controlling for the age of each worker, assignment deviations from randomness may be considered *ignorable* (Angrist, Imbens and Rubin, 1996). Note that *ignorability* together with Assumption (1) allow for a consistent estimation of the causal effects of Z_i on Y_i and of Z_i on D_i. These causal effects can be easily shown to be, respectively, equal to the numerator and the denominator of the estimand in equation 3.[12]

Assumption 3 is analogous to the usual condition, in standard IV estimation, requiring the instrument to be correlated with the endogenous variable. This is the only testable assumption and evidence on its validity for the natural experiment offered by WWII has already been discussed in Section 2. Further evidence for the specific instruments that we will use in the econometric analysis of the Austrian and German case, will be discussed in Section 4. At least on this issue we feel confident in claiming that WWII has indeed constrained certain groups of individuals reducing their probability of reaching higher educational degrees.

Assumptions 1, 2 and 3, however, are not enough to ensure that our instruments can identify the treatment effect in which we are interested. A crucial further assumption is Assumption 4 that plays the same role of exclusion restrictions in regression analysis. What is required in our case is that the war 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 other effect on the workers whose education decision would be the same independently of the war. On the other hand, for those workers whose education decision would be changed by the war, this should be the only channel of effects on earnings. To clarify this point it is useful to consider the four sub–populations in the classification described above.

Beginning with the *always–takers*, the exclusion restriction requires that for each of these individuals (who have low education in all cases) labor earnings are unaffected by the war instruments, i.e. $Y_i(1, Z_i)$ should be independent of Z. This would not be true if, for example, forty years after the war, labor incomes *within* the group of low education workers were depending on whether these workers were in primary school during the war or not. Similarly, for the sub–population of the *never–takers* the requirement is that forty years after the war labor incomes *within* the group of highly educated people should not depend on the war. For both these groups, the exclusion restriction implies that the war has no effect whatsoever because it does not change the schooling decisions

[12]See Angrist, Imbens and Rubin (1996).

and it does not affect labor incomes through any other channel.[13] In the two remaining sub–populations (*compliers* and *defiers*), the war must have an effect on future labor earnings, but the exclusion restriction requires that this effect should be due only to the change of educational level imposed by the war. The possibility of malnutrition of children growing up during the war could be a cause of failure of the exclusion restriction assumption inasmuch as it represents a potential channel through which the war directly influences future labor incomes, independently of schooling. But it seems implausible to imagine earnings consequences of malnutrition still in effect some forty years after the war.

In standard structural models in which the participation to treatment is modeled using a constant parameters equation for the relation between Z and D (the first–stage regression), the above assumptions are enough to ensure identification.[14] However, the assumption of a constant parameter in the relation between Z and D is hiding an additional crucial identification condition that is instead made explicit by the Angrist and Imbens framework on which we base our analysis. This is the Monotonicity Assumption 5 that essentially rules out the existence of *defiers*, ensuring that $Pr\{D_i(1) = 0; D_i(0) = 1\} = Pr\{D_i(1) - D_i(0) = -1\} = 0$. This assumption says that no worker who prefers a low educational degree in the absence of the war constraint may be induced by the war to go into higher education. Vice–versa, some workers who would prefer a high educational degree in the absence of the war may be induced by the war constraint to stop at a lower educational degree. The Local Average Treatment Effect is precisely the treatment effect for these workers. As noted by Angrist and Imbens (1994), the LATE is analogous to a regression coefficient estimated in linear models with fixed effects. In these models the data are only informative about the impact of binary regressors on individuals for whom the value of the regressor changes during the period of observation. The problem is that in the presence of both *compliers* and *defiers* there are, potentially, two types of switchers in opposite directions. Therefore, the effect of treatment for those who go from high to low education (*compliers*) could be completely cancelled out or even reversed by the effect of treatment on those who go from low to high education (*defiers*). In order to identify the effect for *compliers* it is necessary to rule out the possibility of *defiers*. Note also that this assumption is clearly not testable, given that only one type of assignment and treatment is observed for each worker. Yet, it seems a very plausible assumption in our case and it is comforted by the evidence that our war instruments significantly increase the probability of low education.

Given that most of these assumptions cannot be tested but only checked for plausibility, it is advisable to consider what would happen if they were violated. Angrist, Imbens and Rubin (1996) compute the biases generated by the violation of either the exclusion restriction or the monotonicity assumption, each one considered separately.[15] Violation of the exclusion restrictions for *non–compliers* produces a bias that is equal to the average net effect of the war on these workers (i.e. an effect that goes through channels different than schooling), times the odds of being a *non–complier*. It seems plausible to assume that even if the first of these components is non–zero, it should

[13] In section 4 we provide some collateral evidence on the validity of these assumptions.

[14] For example the standard "Heckman Selection" setup in which a Probit model determines participation to treatment.

[15] We refer to their paper for the formal characterization of these biases. Here we focus only on the interpretation of their results for our estimation problem.

not be large. Furthermore, inasmuch as the war can be considered as causing a major increase of liquidity constraints, the odds of being a *non–complier* may be expected to be low. The bias due to violations of the exclusion restrictions for *compliers* can be written as the sum of the bias for *non–compliers* plus a term that captures the direct effect of assignment on outcomes for *compliers*. Note that this later source of bias would be present even in the case of perfect compliance.

The size of these biases is inversely proportional to the correlation between the war instruments and the education dummy, because the higher this correlation, the lower are the odds of *non–compliance*. [16] The sign, instead, depends on the sign of the effect of the war on labor earnings through channels different from education. For example, if malnutrition due to growing up during the war reduces the income of workers, our interpretation of the estimated effect would exaggerate the educational effect of the war. Yet, as we said, we find implausible that the experience of the war at a young age could have long–term effects on earnings through channels *other* than education.

Assuming that exclusion restrictions are satisfied, violations of monotonicity produce a bias that has two components. The first one increases with the proportion of *defiers* and decreases with the correlation between the war instruments and education. [17] The second component is the difference between the average causal effect of education on labor earnings for *defiers* and *compliers*. We have no intuition on whether in our case this difference should be positive or negative (not to mention large or small). Therefore, we cannot say if from the point of view of monotonicity our estimated effects should be considered as a lower or upper bound of the true effect. We do believe, though, that the proportion of *defiers* is small because it seems implausible that children who would have chosen a lower education level in the absence of the war constraint, reach a higher education level if constrained by the war.[18] We are, therefore, less worried by the sign of this bias.

Before turning to the presentation of our results, it should be noted that the estimate based on equation 3 is known in the literature as the "Wald Estimate".[19] This estimation method essentially consists in adjusting the overall earnings differential due to the war for the effect that the war had on education levels. It can be easily checked that if both the instrument Z and the treatment D are binary, the estimate based on equation 3 is numerically identical to the Two–Stage Least–Squares – Instrumental Variables (TSLS-IV) estimate obtained using Z as an instrument for D. Formally:

$$\Delta_Z = \frac{E\{Y_i \mid Z_i = 1\} - E\{Y_i \mid Z_i = 0\}}{Pr\{D_i(1) = 1\} - Pr\{D_1(0) = 1\}} = \frac{COV\{Y;Z\}}{COV\{D;Z\}}. \quad (4)$$

In the case of multiple binary instruments, Angrist and Imbens (1995) show that

[16]More generally the results concerning "weak instruments" described for example in Bound et al. (1995) and in Staiger and Stock (1997) can be extended to this intrepretation of IV, with the caveat that biases are defined with respect to the LATE.

[17]See again Bound et al. (1995) and Staiger and Stock (1997).

[18]Already at 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 (up to 1941) to work as a self–employed farmer. Therefore, defiers can practically be ruled out.

[19]See Angrist (1990) who applied it to the estimation of the wage effect of veteran status during the Vietnam war.

standard TSLS estimation using the full set of binary instrument gives an estimate that is a weighted average of the Local Average Treatment Effects due to each instrument. The weights of this average are proportional to the correlation between assignment and treatment, i.e., in our case, to the correlation between each war instrument and education. The correct standard errors, in this case, turn out to be those given by the formulae of Huber (1967) or White (1982).

Finally, it should be stressed that our estimates of the Local Average Treatment Effect of WWII refer only to the group of *compliers*. Therefore, they cannot be easily interpreted as estimates of average returns to schooling for individuals who are not in this group. As convincingly argued by Angrist, Imbens and Rubin (1996) our (and similar types of) data can only be informative on the workers who would change their education decision according to the presence or the absence of the war constraint (i.e. the *compliers*). Indeed, this is the only group for which members are observed in both treatment statuses in the sense that the members constrained by the war have low education while the others have high education. The workers whose education decision would never be affected by the war constraint (the *always–takers* and the *never–takers*) are always observed in only one treatment status (either low or high education). Therefore, the data cannot be informative on the average return for these two groups.

These considerations imply that if one were interested in estimating the average return to schooling for all the treated, the only way to obtain these estimates would be to assume that both *compliers* and *always–takers* have the same return to schooling. Only in this case the estimable return of the former group could be considered equal to the non–estimable return of the latter group. Even more restrictive would be the assumption needed for an estimate of the average return in the population. In this case, the estimate would have to be the same for all the four groups in the classification described above. [20]

As we argued in the introduction, we agree with Heckman (1996 and 1997) in saying that the average effect of treatment on the treated is often a more interesting parameter than the average effect of treatment in the population at large. However, we disagree with him when he claims that the LATE is not interesting because it refers to a population that is never observed. We believe that our paper presents one case in which the LATE is instead well defined and provides interesting information from both the points of view of economic history and economic policy.

4 Estimates of the LATE of education due to WWII

In this section we first apply the framework described above to data from Germany and Austria in order to estimate the long run educational cost of WWII in these two countries. We later extend the analysis to Sweden and Switzerland (two countries which were not involved in the war) in order to enlarge and improve the quality of our control

[20] See Björklund and Moffitt(1987) and Heckman and Robb (1985) for an early recogntion of similar distinctions in the presence of heterogeneity of treatment effects.

sample. [21]

4.1 Germany

For Germany we use the 1986 wave of the Socio–Economic Panel because this is the wave in which persons were asked about educational attainment, war experience and social status of their parents. In order to estimate the earnings loss due to the effect of the war on education, we use a parsimomious specification of the earnings function. We regress the logarithmic hourly wage ($lnW_i = Y_i$) on a dummy for lower education ($LOED_i$)[22] , on age and on other variables which are predetermined at age 10 of the student, like the father's education and social status. [23] Usual additional determinants of wages, like work experience, tenure, sectors and occupations remain unconsidered in our framework, because they are partly determined by educational attainment. We are interested in the overall effect of educational choices independently of the channel — e.g. a different occupational choice — through which it takes place. Therefore, our approach attributes all these — secondary — returns directly to schooling as such.

An important further consideration is the labor market situation at the time of entry into gainful employment. Welch (1979) showed that the "Baby Boom Cohorts" in the U.S. suffer in terms of lower wage rates, an effect which declines over the life cycle but does not vanish all together. Cohort size at entry in the labor market is also found important for the U.K. (Wright, 1991), but here the negative effect does not persist as the cohort ages.[24] Unfortunately, no consistent data on the size of entry cohorts are available for these years in Germany. Instead, we use the general unemployment rate in the year the student turned 14 ($URATE_i$) as our cohort size indicator.[25] This indicator has also the advantage that economic disruptions after the war as well as population movements, like the inflow of refugees, Germans from eastern territories and other immigrants are picked up. Given the above considerations, the estimated earnings function is:

$$\begin{aligned} lnW_i &= \beta_1 + \beta_2 LOED_i + \beta_3 AGE_i + \beta_4 AGE_i^2 + \beta_5 URATE_i + \beta_6 LOEDF_i + \\ &\quad + \beta_7 BLUEF_i + \beta_8 SELFF_i + \varepsilon_i \end{aligned} \tag{5}$$

Descriptive statistics on the variables used for the IV estimation of equation 5 are reported in table 2. Remember that our schooling variable is a dummy that takes value 1 for individuals who do not reach higher educational degrees and age 10 is the crucial

[21] We do not consider other nations because among the potentially relevant countries these are the ones for which it was easier to get suitable data on old cohorts. For example, we considered the possibility of adding Italy to our empirical analysis, but reliable and suitable earnings data for this country are available only for cross sections observed in the '90s. Because of retirement, recent data are useless for the purpose of this study.

[22] The degrees that we classify as higher education for Germany are: *Abitur, Fachhochschule* and *Universitaet* that correspond, in terms of US educational curricula, to going beyond a highschol degree.

[23] Father's education is measured by a dummy ($LOEDF$) defined in the same way as $LOED$ for the children. Father's social status is captured by a dummy for blue collar status ($BLUEF$) and a dummy for self employment status ($SELFF$).

[24] In equilibrium, these reduced labor market opportunities should also have repercussions on educational choice. Flinn (1993) considers this question and finds the effects small so that we can disregard them.

[25] These data come from Mitchell (1975).

age for this educational decision. Therefore, a dummy for the cohort born between 1930 and 1935, whose members reach age 10 during the war, serves as our first instrument (or assignment mechanism) for the educational choice. As shown in table 2, 8% of the sample is born before 1930 while the size of the assignment group (i.e. those born between 1930 and 1935) is 11%. Aside from cohort considerations, the fact that the father of the student served actively in the military service is a more direct measure of the existence of potential liquidity constraints imposed by the war . This is our second instrument.[26] Note again, that different LATE estimators are associated with these two instruments.

Results for the wage regressions are in Table 3; the first column presents an OLS estimation, whereas columns 2-4 present different LATE estimators. In the OLS regression, workers who have lower educational degrees earn 25.1% less than other workers. [27] Earnings losses are more than twice as high in the IV–LATE estimation. For workers who dropped out of education because they belonged to the cohort born in the period 1930–35 (column 2) these losses are equal to 44.6%. For those who dropped out because their father was in the war the loss amounts to 51.7% (column 3). As expected (Angrist and Imbens, 1995), combining the two instruments (column 4) gives a weighted average of the two LATE estimators based on single instruments.

Although the point estimate in column 2, based only on the cohort dummy, is very similar to the other LATE estimates, it is not significant. Two reasons might be responsible for this phenomenon. First, the father–in–war instrument is more important as a determinant of the educational status. Adding the instrument to the other exogenous variables in the first stage regression increases the R^2 by 0.007 in the case the father–in–war dummy and only by 0.001 in the case of cohort 1930–35 dummy. Second, even after controlling for a (quadratic) age profile, the effect of the war on the 1930-35 cohort would be better captured if we could eliminate younger individuals and make our treatment and control groups more similar. The ideal control group should include only those born immediately before and after the cohort 1930-35, in order to reduce the effect of confounding cohort characteristics. The available German data set is, however, too small to do so. If we limit the analysis to the elderly, what we gain from looking at a more informative treatment-control comparison goes lost in terms of sample size. As a result, when we use only workers born before 1946, the estimated cohort effect (that we do not report to save space) is very similar but not more significant.

Following Staiger and Stock (1997), the reciprocal of the F-test on the excluded instruments in the first stage 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, this fraction is approximately 29.8 %; using only the father–in–war instrument, the bias is only 6.3%, respectively . When they are used together, the small-sample IV bias is approximately 10.5 % of the OLS bias. This analysis shows that having the father–in–war is probably a much better indicator for the educational losses of the war, compared to the simple cohort instrument.

[26]For a stricter interpretation of liquidity effects, the time the father served in the military should be considered, which is unavailable. Moreover, the time span could also extend to periods after the war, because soldiers — and only soldiers — can be and have been kept for prolonged periods prisoners of war afterwards.

[27]Here (and in the rest of the paper) the effect of dummies in percentage terms has been obtained using the transformation $e^{\beta} - 1$ of the estimated coefficient β.

The other variables in equation 5 have generally the expected influence for Germany. The age–earnings profile is concave. Cohort size effects — as captured by the unemployment rate at age 14 — are slightly negative. In line with the existing literature [28] the educational background of the father has no direct effect on wages once the educational attainment of the child is controlled for. Perhaps more surprising is the absence of significant direct effects of parental occupational attainment, as measured by the self–employment and blue–collar dummies for the father; but these are very crude measures of occupational status and exploring parental effects more closely is outside the scope of this paper.

4.2 Austria

For Austria we use data from the Austrian Microcensus 1983. In this dataset we do not have information on the status of parents during the war and, therefore, our analysis can be based only on the cohort instrument. But thanks to the larger number of observations than in Germany, it is possible to restrict the analysis to men born before 1946, with the advantage of comparing more similar birth cohorts. As shown in table 2, where the descriptive statistics for the Austrian sample are reported, the size of the assignment group (those born between 1930 and 1935) is now higher than in Germany (29%), while 23% of the sample is born before 1935.

For these men, a wage regression similar to equation 5 is run, with the exclusion of family background variables that are not available for Austria. Furthermore, age is introduced only linearly because of the rather flat age–earnings profile of elderly workers. The dummy for lower education is instead defined similarly to the German case and takes value 1 for individuals that have less than a high-school degree (*Matura*). Again the age of 10 is crucial for the educational decisions that determine the value of this dummy.

The results in Table 3 confirm the findings for Germany. In the OLS regression workers without higher education command 40.4% lower wages than other workers. In the IV-LATE regression, the loss is estimated to be higher and equal to 61.2%. The cohort instrument gives more precise estimates here because of the larger sample size and the sharper comparison between the more similar cohorts of older workers. As a result, not only the statistical significance but also the size of both the OLS and LATE effects are estimated to be larger in Austria than in Germany.

4.3 Collateral evidence on exclusion restrictions

As we discussed in section 3, most of the assumptions that are necessary for the identification and estimation of Local Average Treatment Effects are untestable. We mentioned, however, that one of the theoretical requirements imposed by the exclusion restrictions is that the war must not have any effect for *always takers* and for *never takers* (i.e., respectively, those who would never reach higher education and those who would always do so independently of the war). Furthermore, *compliers* should be affected by the war only through the educational channel.

[28]See, for example, Treiman and Yip (1989) and Ichino at al. (1996)

Strictly speaking these assumptions are untestable because we cannot identify these groups in the population and because for individuals in each of them only one realization of the triple Assignment–Treatment–Outcome is observed. We nevertheless believe that some collateral corroborating evidence on the above implications of exclusion restrictions can be obtained by looking at wage regressions estimated separately for the two groups of low and high education individuals. In other words, assuming no endogenity of the treatment, we now check for direct effects of the assignment on the outcome, conditioning on the treatment.

Consider the group of individuals with higher education. [29] This group includes all the *never–takers* [30] and the *compliers* assigned to high education because they were not constrained by the war. Therefore, the war assignment mechanism should have no effects on the earnings of these individuals. If, for example, the *never takers* born in 1930-35 or those who had the father in war were earning lower wages than the other highly educated individuals because they were malnurished in their youth, the war instruments should have a negative and significant effect in a wage regression estimated only on the highly educated. Furthermore, if the war effect for *compliers* were due to additional channels beyond education, the war instruments should again have a negative and significant effect. Similarly, for the group of individuals with lower education. This group includes all the *always–takers* and the *compliers* assigned by the war to low education. Again, all these individuals should have the same earnings independently of the war.

Table 5 shows that in both Germany and Austria the war instruments have no significant effects in wage regressions estimated separately for the groups of high and low educated individuals. The signs are negative for Germany but positive among highly educated Austrians. The sizes are fairly negligible with respect to the dimension of the LATE parameters estimated in Tables 3 and 4. Furthermore, for the case of Germany in which two instruments are available, a more traditional over–identification test is possible and, as reported in Table 3, the over–identifying restrictions cannot be rejected.

4.4 Pooled-countries evidence

As we already mentioned above, the assignment mechanism based on the cohort dummy is not fully satisfactory because the earnings of individuals born between 1930 and 1935 may have been influenced by several other factors that have nothing to do with the loss of education due to the war: for example, the fact that they were born during the Great Depression or that they entered the labor market immediately after the war. These events might have had effects on earnings that should not be confounded with the effects due to the educational losses caused by the war.

In order to control for these confounding factors, we pool together the German and Austrian datasets and three similar datasets from Switzerland and Sweden. [31] These

[29] In the argument that follows we are assuming the absence of *defiers*, an assumption on which, as argued in section 3, we feel fairly confident.

[30] The reader should remember that in this paper the treatment is defined as refraining from higher education.

[31] The sources of these datasets are: Switzerland (Einkommens– und Vermögensstichprobe), Sweden I (1984 Swedish Survey of Household Market and Non-market activities - HUS Project), Sweden II (1981

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 WWII.[32] 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 1935, but this cohort is likely to have shared with the analogous Austrian and German cohort most of the other counfounding factors. Therefore, by adding samples from these two countries, the quality of our control group improves considerably, because now it includes not only individuals born in different cohorts of the same country, but also individuals born in the same cohort of different countries. Given the sample size that we reach within this pooled-countries dataset, we can further increase the comparability of the treatment and the control group by restricting the analyisis to individuals born before 1946. Descriptive statistics for all these samples are reported in table 2. The estimated equation is:

$$\begin{aligned} lnW_{ik} &= \gamma_1 + \gamma_2 LOED_i + \gamma_{3k}(AGE_i * COUNTRY_k) + \gamma_{4k} COUNTRY_k + \\ &\quad \gamma_5 BORN30\text{–}35_i + \varepsilon_i \end{aligned} \tag{6}$$

where i denotes individuals and k denotes countries [33]; $AGE_i * COUNTRY_k$ captures country-specific age effects; $COUNTRY_k$ is a set of dummies that controls for countries' fixed effects; $BORN30\text{–}35_i$ is a dummy for the cohort born between 1930 and 1935. This dummy should control for cohort-specific influences on earnings, which are unrelated to the war.

In the corresponding IV-LATE estimation the assignment to treatment is defined as the intersection of the following two events: "being born in Austria or Germany" and "being born between 1930 and 1935". The product of the two dummies denoting these conditions is, therefore, the instrument for $LOED$. Note that this specification has the conventional difference-in-difference form, where country effects and cohort effects are eliminated. The LATE estimator should therefore give us the earnings loss of individuals who stopped at lower education because they were born between 1930 and 1935 *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.

The results are reported in Table 6. The OLS coefficient is in-between the corresponding Austrian and German estimates with an earnings loss of 35% for those with low education. Again, earnings losses are instead more than twice as large in the LATE estimation. Those who dropped out of school because of the war in Austria or Germany lost 77% of potential earnings. The effect in this pooled-countries regression is even higher than the one measured for Austria alone. Moreover, the simple cohort effect for those born between 1930 and 1935 is never significant. These pooled-countries results corroborate the evidence presented separately, in the previous sections for Germany and Austria: our LATE estimator based on cohort information is indeed capturing the impact of the war and not just general cohort effects.

Swedish Level of Living Survey).

[32] There are very few other countries not involved in WWII and, with the exception of Spain, Portugal and Ireland which are certainly less similar to Germany and Austria (the former also had the Civil War), none of them is European (i.e. they are even more dissimilar).

[33] To be more precise, given that for Sweden we have two independent data sources, k denotes the five cross-sections: Austrian, German, Swiss and two Swedish.

5 The Long-run Educational Cost of WWII in Austria and Germany

On the basis of the LATE parameters estimated for Germany and Austria we are now able to calculate three different possible measures of the educational cost of WWII. The first measure, that we indicate with $COST1$, is the LATE itself in percentage terms: it measures the income loss due to the war for a random person in the sub–population of those who changed educational attainment just because of the war. This loss is expressed as a percentage of the income that such a person would have obtained if he had reached higher education. Therefore:

$$COST1 = e^{\Delta_Z} - 1 \tag{7}$$

This measure is the crucial one if we want to interpret the results of this paper in a structural way. In other words, if we want to infer from our estimates what would be the individual earnings loss attributable to a constrained educational decision when the latter is due to an increase of liquidity constraints similar to the one produced by WWII.

The second measure, $COST2$, calculates the average impact of the war on the earnings of an individual in the assignment group. Depending on the specific instrument, this is the group of individuals born between 1930 and 1935 or the group of individuals having a father in the war. Note that this measure is nothing else than the numerator of the LATE in percentage terms: [34]

$$\begin{aligned} COST2 &= e^{E\{Y_i|Z_i=1\}-E\{Y_i|Z_i=0\}} - 1 \\ &= COST1 \;\; e^{[E\{D_i|Z_i=1\}-E\{D_i|Z_i=0\}]} - 1. \end{aligned} \tag{8}$$

It therefore measures the effect of the war instruments on the earnings of the assignment group expressed again as a percentage of the average income of the highly educated. This overall 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 assignment group 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 went lost, in the year of the survey, because of the distortion of educational decisions induced by our war instruments:

$$COST3 = \frac{(COST2\;\, Y_H)Pr(Z_i = 1)}{Y} \tag{9}$$

where Y_H is the average income of the highly educated and Y is the average income in the population. Of course, a more detailed calculation could in principle aggregate the earnings losses in the years from 1946 to the survey's year. This exercise is possible, but it 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 WWII for Germany and Austria. Beginning with Germany the computation of each measure is performed separately for

[34]This is sometimes referred as the *intention-to-treat* effect of Z on Y. See Angrist Imbens and Rubin(1996).

each of the two instruments used. All the three cost measures are estimated to be larger for those who had a father in war than for those who were born between 1930 and 1935. In terms of $COST1$ those who dropped out of school because of the father in war lost 51.7% of their potential income while the loss for those who made the same decision because of the cohort effect was 44.6%. Even larger is the differential impact of the two instruments when we look at the average effect on the two assignment groups. Having a father in the war implies on average a percentage income loss of more than 15% while the average cohort effect is as low as 2.49%. These results suggests that the first assignment group suffered on average more binding liquidity constraints than the second: a larger fraction of individuals in the first assignment group decided to comply with assignment and to refrain from higher education; in addition, those who did it reduced their years of schooling by a larger amount.

Despite the large differences concerning $COST2$ in the two assignment groups, looking at $COST3$ the percentage losses of GDP for the two instruments are more similar. This is evidently explained by the fact that the total number of individuals affected by the father–in–war instrument is smaller then the size of the 1930-35 cohort. Both estimates are anyway sizable ranging between 0.42% and 0.36%. The educational cost of the war in terms of GDP appears substantial even after 40 years.

Table 7 shows also that the three measures are generally higher in Austria than in Germany. For Austria we can measure only the cohort effect. This amounts to a loss of 61.2% of potential income for those who refrained from higher education. The average percentage loss for the entire 1930-35 cohort is instead equal to 3.21% This measure is higher than the cohort effect in Germany but lower than the father–in–war effect, that definitely must capture the most binding liquidity constraint. The percentage loss in terms of Austrian GDP is nevertheless larger than any of the correspondent GDP losses for Germany, being equal to 0.67%.

The higher Austrian GDP loss may be due to the larger dimension of the assignment group in the Austrian sample, or to other idiosyncratic differences of the effects of wars in the two countries on which we have nothing to say. But it could also be due to the fact that, thanks to the larger sample size, for Austria we have been able to estimate the returns to education using elderly workers only. In other words, it could be due to the better degree of comparability of the treatment and control groups that gives higher LATE estimates for Austria and for the pooled-countries sample analysed in section 4.4. Given our parsimonious specification of the wage function, all earnings-enhancing lifetime events, like job experience, tenure, and the choice of industry or occupation, which are different for low education and high education workers, are implicitly attributed to the choice of education. Since the raw earnings difference is larger for elderly workers, the war effect running through the educational choices will also appear to be larger. Another way to say it is that by focusing on the elderly, we are measuring the cost of the educational constraint imposed by the war at a moment of the careers of individual workers in which the instantaneous difference in earnings is larger.

Since focusing on the elderly implies using the most correct control group from the point of view of this paper, the estimates for Austria could be considered as a better approximation of the educational cost of the war, while the estimates for Germany would be a sort of lower bound.

6 Conclusions

Apart from all other — human, financial and emotional — costs, World War II led to a significant drop in educational attainment in Europe, with effects that are still noticeable in the '80s. Because of the educational effects of the war that we have been able to identify in our data, German GDP has been at least 0.36% lower in 1986; the loss for Austrian GDP in 1983 is even larger, being equal to 0.67%.

For various reasons, we believe that these effects should be considered as a lower bound of the overall educational effect of WWII, at least for Germany. Indeed for this country the estimates are not based just on the elderly and therefore are likely to understate the effects that we can identify, i.e. the impact on those who changed education because they had a father in war or because they were 10 years old during the war. In addition, and this is true for Austria as well, the war might also have reduced the educational attainment and earnings of those born in other cohorts, of active soldiers, 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, etc. All these effects should go in the same direction: we are not only underestimating the educational effects that we can identify but we are leaving out of the picture other educational effects that we cannot identify.

There are, however, two kinds of reasons suggesting that instead our estimates could be an upper bound of the true effect. The first possibility is that the war may have affected the earnings of individuals in our assignment groups through channels that have nothing to do with education. For example, these individuals might have suffered because of malnurishment during the war. Nevertheless, we find it hard to believe that these effects may persist in earnings observed forty years after a war. We find instead very likely that the educational choices made because of the war might have 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. A second possible reason is that the quality of education might have been lower during WWII, 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.

It could also be argued that our estimates based on cohort effects overstate the educational effect of the war because several counfounding factors may have reduced the earnings of individuals born between 1930 and 1935 without having anything to do with the educational effect of the war. Examples of these factors are the fact that these individuals were born during the Great Depression or entered the labor market immediately after the war. However our pooled-countries analysis shows that the loss of earnings for individuals in the German and Austrian 1930-35 cohort persists even compared to individuals in the analogous Swedish and Swiss cohort, who shared similar confounding factors but not the educational effect of the war. 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 WWII

In the light of the recent literature on returns to schooling, our paper is linked to the analysis of Card (1994) and Angrist and Imbens (1994) inasmuch as it shows that the choice of instrument matters for the estimation of returns to schooling. It should be noted, however that Harmon and Walker (1997), using UK data, find that their estimates are not particularly sensitive to the choice of instrument. Therefore, they suggest that the theoretical issues raised by Card (1994) may not be so relevant in practical terms. If one were willing to trust their conclusion, our estimates could also be considered an approximation to the average return to schooling in the population. We have no reason to doubt their results, but we agree with Heckman (1985, 1997) who argues that estimating the average effect of treatment for a random person in the population may be of limited policy interest if the treatment is education and the outcome is labor earnings.[35] More interesting should be the identification and estimation of the average effect of treatment among all those who freely decide to be treated. But Angrist, Imbens and Rubin (1996) show that the average effect of treatment on the treated can only be deduced from the IV estimation of the LATE *with additional assumptions*. Within the context of a particular study and given a particular instrument, data can only be informative on the effect of treatment for those who change treatment status because of the instrument (i.e. the *compliers*). Conclusions on the average effect of treatment for the treated can only be obtained by extrapolation from this effect. What is often implicitly and not carefully assumed in the interpretation of IV results is that the average treatment effect is equal to the LATE. Given the goal of our paper, i.e. the estimation of the long–run educational cost of WWII, we do not need and do not want to make the additional assumptions needed for this extrapolation: the assumptions required by Angrist and Imbens (1994) are exactly what we need.

The fact that our estimates are informative only for the group of *compliers* with respect to the assignment mechanism, is also important to understand why our OLS estimates are smaller than the IV–LATE estimate. As described by Card (1994), this finding is fairly general in the recent empirical literature on returns to scholing and should be interpreted considering the discount rates of the specific group of individuals whose educational choice is modified by the instrument chosen for the estimation (i.e. the *compliers*). While a formal model is outside the scope of this paper, a possible justification of the difference between OLS and IV-LATE estimates in our case is offered by the following consideration. Individuals invest in schooling until their marginal return to schooling is equated with their marginal discount rate. Rich individuals are likely to be *never takers* in our framework because their discount rate is low enough to allow them to go to school independently of the war. The *always–taker* are likely to be poor individuals with marginal returns that are so low that they always refrain from higher education. *Compliers* are instead the individuals with higher marginal returns among poor families. These individuals go to higher education when they face the normal amount of liquidity constraints due to their poor status. But if the war increases these liquidity constraints, they have to drop out of school and stop at lower educational degrees. As a result the average return to schooling for these individuals may be larger than the average return in the population.

[35] To put it in his words (1997, p.443), "[P]icking a millionaire at random to participate in a training program for low skilled workers, or making an idiot into a Ph.D. may be intriguing thought experiments but are usually neither policy relevant not feasible. They are not policy relevant because interest centers on the effects of programs on intended participants — not on persons for whom the program was never intended. It is not feasible random–assignment strategy because millionaires would never agree to participate in such a training program even if they were offered to do so, and few idiots would be able to attain the PhD in most fields."

We would like to conclude by suggesting in which sense we believe that our paper might have a policy relevance beyond economic history. If our research had been moved by purely historical accounting interests, it would have been sufficient to calculate the direct effect of the war instruments on wages (i.e. our second and third measures of the cost of the war). But our analysis allows us to understand under which assumptions those measures can be interpreted as costs due to the distortion of educational decisions. From this point of view, the second and third cost measures should be interpreted as reduced form estimates, while the first measure (the LATE parameter) should be interpreted as a structural effect. On the basis of the Angrist-Imbens-Rubin interpretation of IV, we are able to identify the structural channel through which the war causes the observed long run effects even 40 years after its end. This channel is the distortion of the educational decisions (presumably of the poorer families) due to the increase of liquidity constraints imposed by the war.

The policy contribution of our paper consists, therefore, in the estimate of a structural parameter that could be used to infer the long run educational cost of similar increases of liquidity constraints. One example will suffice. If German workers who had their father in the war lose 52% of their potential income because of the lower education that they received, students whose father is unemployed, in jail or missing for other reasons may face similar losses. While replacing the missing father may be unfeasible, actions aimed at increasing the educational attainment of these individuals may save them from suffering substantial and long lasting earnings losses.

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Table 1: The Impact of WWII on Educational Attainment

Country	Year of survey	I: Born 1930–35	II: Born 1930–35	# observations	Mean of LHS
Italy	1985	0.048 (0.016)	—	3551	0.763
Germany	1986	0.086 (0.022)	0.076 (0.023)	2131	0.808
Austria	1983	0.058 (0.009)	—	8062	0.879
The Netherlands	1977	0.089 (0.040)	0.108 (0.042)	1210	0.440
Hungary	1982	0.072 (0.023)	0.055 (0.024)	4707	0.504
Finland	1975	0.072 (0.067)	0.079 (0.067)	360	0.639
UK	1972	0.174 (0.018)	0.174 (0.018)	6503	0.491
Northern Ireland	1973	0.102 (0.035)	0.114 (0.036)	1725	0.489
US	1973	0.016 (0.007)	0.007 (0.006)	24935	0.180
Ireland	1973	0.006 (0.035)	0.013 (0.036)	1661	0.460
Thailand	1970	0.032 (0.027)	0.029 (0.024)	989	0.884
Switzerland	1982	0.004 (0.033)	—	895	0.144
Sweden I	1984	0.005 (0.059)	—	651	0.330
Sweden II	1981	0.054 (0.037)	—	2503	0.399
India	1971	0.006 (0.019)	0.012 (0.013)	1641	0.899
Brazil	1982	-0.025 (0.015)	-0.019 (0.014)	8497	0.840

All data come from Ganzeboom et al. (1992), except for Austria (Microcensus 1983), Germany (Socio–Economic Panel 1986), Italy (Indagine sulla Mobilita' Sociale 1985), Switzerland (Einkommens– und Vermögensstichprobe), Sweden I (1984 Swedish Survey of Household Market and Non-market activities - HUS Project), Sweden II (1981 Swedish Level of Living Survey). Probit estimates of the following model:

$$Pr\,(LOED_i) = F(a_1 + a_2 BORN30\text{–}35_i + a_3 AGE_i + a_4 LOEDF_i)$$

where lower education is defined as less than 9 years of schooling in all countries. The father's higher educational degree is included only in specification II. The reported coefficients express for each country the change in the probability of dropping out of education for individuals born between 1930 and 1935. Standard errors in paranthesis. — = not available.

Table 2: Descriptive statistics

	Austria	Germany	Switzerland	Sweden I	Sweden II
Low education (0,1)	0.88	0.81	0.42	0.43	0.80
Age	48.1 (6.04)	39.5 (11.4)	49.05 (7.81)	49.71 (7.9)	48.61 (8.68)
Unemployment rate at age 14	4.95 (4.12)	3.30 (2.87)	—	—	—
Father has low education	—	0.92	—	—	—
Father is a blue-collar	—	0.45	—	—	—
Father is self employed	—	0.13	—	—	—
log(wage)	4.27 (0.33)	2.87 (0.55)	3.25 (0.40)	5.42 (0.34)	3.75 (0.32)
Born before 1930	0.23	0.08	0.36	0.30	0.38
Born in 1930 – 1935	0.29	0.11	0.20	0.20	0.18
Born after 1935	0.48	0.81	0.44	0.50	0.44
Father in war	—	0.02	—	—	—
# Observations	4134	1894	534	372	890

Note: Standard deviations in parentheses. Wages are in local currencies as of the time of the interview. Descriptive statistics for Germany are for all the individuals used in Table 3. (Table 6 uses only those born before 1946).

Table 3: The individual earning loss due to the war in Germany

	OLS	LATE–IV: Instrument: Cohort 1930–35	LATE–IV: Instrument: Father in war	LATE–IV: Instruments: Cohort 1930–35, Father in war
Low education (0,1)	-0.289	-0.590	-0.727	-0.708
	(0.031)	(0.844)	(0.278)	(0.279)
Age (years)	0.082	0.077	0.075	0.075
	(0.009)	(0.017)	(0.010)	(0.010)
Age^2	-0.001	-0.001	-0.001	-0.001
	(0.000)	(0.000)	(0.000)	(0.000)
Unemployment rate at age 14 (%)	-0.010	-0.007	-0.005	-0.005
	(0.004)	(0.011)	(0.005)	(0.005)
Father has low education (0,1)	-0.038	0.099	0.162	0.153
	(0.040)	(0.381)	(0.136)	(0.137)
Father is a blue–collar worker (0,1)	-0.004	0.030	0.045	0.042
	(0.029)	(0.097)	(0.039)	(0.039)
Father is self–employed (0,1)	-0.038	-0.034	-0.032	-0.032
	(0.040)	(0.045)	(0.043)	(0.043)
Constant	1.325	1.652	1.900	1.780
	(0.190)	(0.933)	(0.376)	(0.357)
$\bar{R}^2$	0.185	0.146	0.103	0.110
# Observations	1894	1894	1894	1894
Partial R^2 for instrument(s) in 1^{st} stage	—	0.001	0.007	0.008
F-Test on instrument(s) in 1^{st} stage	—	3.35	15.77	9.51
Over–identification test, $\chi^2(DF)$	—	—	—	0.022 (1)

Standard errors in parentheses (Huber–White corrected in column 4), data come from the German Socio–Economic Panel, wave 1986. The dependent variable is the log of hourly wages. The dummy for lower education takes value 1 for individuals who have not reached the following educational degrees: *Abitur, Fachhochschule* and *Universitaet.* In terms of US educational curricula, not reaching these degrees corresponds to not reaching a highschool diploma.

Table 4: The individual earning loss due to the war in Austria

	OLS	LATE–IV: Instrument: Cohort born 1930–35
Low education (0,1)	-0.518	-0.947
	(0.015)	(0.343)
Age (years)	0.001	0.001
	(0.000)	(0.001)
Unemployment rate at age 14 (%)	0.003	0.001
	(0.001)	(0.013)
Constant	4.658	5.057
	(0.040)	(0.322)
$\bar{R}^2$	0.242	0.081
# Observations	4134	4134
Partial R^2 for instrument in 1^{st} stage	—	0.0019
F-test for instrument in 1^{st} stage	—	7.94

Standard errors in parentheses. Data come from the Austrian Microcensus, 1983. Only individuals born before 1946 are included. The dependent variable is the log of hourly wages. The dummy for lower education takes value 1 for indivduals who have not reached degrees higher than the *Matura*. In terms of US educational curricula, not reaching these degrees corresponds to not reaching a highschool diploma.

Table 5: Collateral Evidence on Exclusion Restrictions: Germany, Austria

	Germany	Germany	Austria	Austria
	Only low education individuals	Only high education individuals	Only low education individuals	Only high education individuals
Cohort born 1930–35 (0,1)	-0.0125 (0.078)	-0.026 (0.046)	-0.017 (0.013)	0.014 (0.044)
Father in war (0,1)	0.110 (0.500)	-0.025 (0.089)	—	—

Estimates of the effects of the war instruments in wage regressions estimated separately for the groups of individuals with high and low education. Standard errors in parentheses. The data, the specifications and the definitions of high and low education are as in Tables 3 and 4.

Table 6: Pooled countries evidence, including Sweden and Switzerland

	OLS	LATE–IV: Instrument: Cohort 1930–35 in Germany, Austria
Low education (0,1)	-0.436 (0.011)	-1.501 (0.655)
Country (Base: Germany)		
Austria	1.254 (0.107)	1.288 (0.216)
Switzerland	0.090 (0.136)	-1.166 0.791)
Sweden I	2.011 (0.148)	1.084 (0.643)
Sweden II	0.737 (0.118)	0.212 (0.406)
Cohort 1930–35	-0.005 (0.010)	0.037 (0.032)
Constant	3.351 (0.100)	4.274 (0.605)
Country-specific age terms	Yes	Yes
$\bar{R}^2$	0.766	0.449
# Observations	6811	6811
Partial R^2 for instrument in 1^{st} stage OLS regression	—	0.0006
F-test for instrument in 1^{st} stage	—	5.70

Standard errors in parentheses. # of Observations: Austria 4134, Germany 892, Switzerland 523, Sweden I 372, Sweden II 890. The data and the specifications are as in Tables 1, 3 and 4. Only individuals born before 1946 are included. The dependent variable is the log of hourly wages. The dummy for lower education takes value 1 for individuals who have not reached degrees higher than the *Matura* or *Abitur*. In terms of US educational curricula, not reaching these degrees corresponds to not reaching a highschool diploma.

Table 7: Three measures of the educational cost of WWII

Instrument	$COST1$	$COST2$	$COST3$
A: Germany			
Father in war	51.7	15.55	0.42
Cohort born 1930–35	44.6	2.49	0.36
B: Austria			
Cohort born 1930–35	61.2	3.21	0.67

$COST1$ is the average percentage income loss for the individuals who refrain from higher education because of the correspondent war instrument. $COST2$ is the average percentage income loss for all the individuals for which the correspondent war instrument takes value 1. $COST3$ is the percentage loss of GDP attributable to the educational effect of the correspondent war instrument in the year of the survey. The formal definitions of these variables are given in section 5. All calculations are based on the data and the estimates described in tables 3 and 4.