The Long Run Educational Impact of Iran-Iraq War

Mohammad Vesal

m.vesal@sharif.edu

August 21, 2017

Sharif University of Technology

Abstract

Children exposed to wars and other catastrophic events could suffer long lasting effects if their human capital accumulation is disrupted. In this paper, I use Iran Population Census 2006 to estimate the long run educational attainment impact of Iran-Iraq War (1980-1988). By comparing cohorts exposed to war at various points in life in war provinces to other cohorts, I find the probability of finishing high school is reduced by 8.8 percentage points for early life exposure to war. The impact of later life war exposure, i.e. during primary school, is smaller and becomes insignificant in some cases.

Keywords: Iran-Iraq War, Children, Educational attainment

JEL Classification: I20, O10, O15

1 Introduction

Wars, natural disasters, and other catastrophic events could have long lasting effects on individual well-being. Young individuals who are still in the process of human capital accumulation are particularly vulnerable to negative shocks. Destruction of schools, interruption of classes, loss of teachers, loss of family members, and loss of household income are a few mechanisms that could reduce educational attainment of young individuals. Exposure to catastrophes could also have an adverse health effect that negatively affects educational outcomes.

This paper looks at the educational attainment of Iranian children exposed to Iran-Iraq War, 18 years after the war ended. Iran-Iraq War was the second longest war during the twentieth century. It started on 22 September 1980 with large scale Iraqi invasion of Iranian territory and resulted in a peak displacement of more than 1.6 million individuals across five provinces neighboring Iraq by June 1982 (around one fifth of the population living in these provinces). While there is a vast literature on the analysis of motivations, operations, and strategic implications of the war, there is little work on the economic impact of this war. I provide the first reduced form estimates of the long run impact of Iran-Iraq War on educational attainment of children[1].

I use the 2 percent public record sample of Iran Population Census 2006 and compare high school graduation rates for children exposed to war to those not exposed. Date of birth and place of residence jointly determine whether a child was exposed to war. Therefore, I employ a difference-in-differences (DD) estimation strategy and compare war time cohorts across war and non-war provinces to pre-war cohorts. I distinguish

[1]My search of the Farsi and English literature has returned no study of educational impact of the war. Mofid (1990) and a few other Farsi publications provide aggregate estimates of the economic cost of the war. A handful of articles studied the impact of exposure to chemical warfare during Iran-Iraq War on health outcomes (e.g. Ahmadi et al. (2010), Ahmadi et al. (2009), Kadivar and Adams (1991), and Khateri et al. (2003)). Mahvash (2011) studies impact of the war on divorce patterns.

between early childhood and primary school exposure to war. Hence, I define three treatment cohorts based on age at the onset of war: a) individuals aged between -6 and -2 years old are only exposed to war in the years before going to primary school, b) individuals aged between -1 and 5 years old spent at least one pre-primary year and one primary school year during war, and c) individuals aged between 6 and 10 years old spent only primary school during war. The large literature on the importance of early childhood events suggests that early years exposure to war has a negative impact on physical and psychological development of very young children[2]. On the other hand the large scale displacement of individuals could have interrupted schooling and led to negative effects for school cohorts. In this study I would be able to provide a comparison of early childhood and school time effects which might be useful in formulating mitigation policies for similar catastrophic events.

The DD estimates show that exposure during early years of life, generate the greatest negative effect on educational attainment. The probability of high school graduation is reduced by 8.8 percentage points (significant at 1 percent) for the early years cohorts, while there is a small insignificant effect on primary school cohorts. The impact on the double treated cohorts is 4.1 percentage points (significant at 5 percent) reduction in probability of finishing high school. The early childhood effect is robust to several alternative specifications.

To interpret estimates as causal, I need to rule out several potential confounding factors. First, the 2006 Census does not provide data on wartime residence and birth place of all individuals. I only observe the "birth place" for individuals living at their birth place at

[2]For example, Almond and Mazumder (2005) and Almond (2006) study the 1918 influenza pandemic in the US, Almond, Edlund, and Palme (2009) investigate the effects of Chernobyl's radioactive radiations, Almond et al. (2010) consider Chinese famines, and Almond, Mazumder, and van Ewijk (forthcoming) and Almond and Mazumder (2011) study the impact of fasting during pregnancy on children. All these studies detect significant large impacts of the early childhood event on adult human capital and labor market outcomes. For review of the literature see Almond and Currie (2011a) and Almond and Currie (2011b).

the time of census. I define these individuals as "non-migrants" and restrict the sample to be able to assign treatment status correctly. While 65 percent of individuals are non-migrants, non-migrants in war provinces may not be comparable to their counterparts from non-war provinces. War induced many individuals, who would not have migrated otherwise, to migrate out of war provinces. If well-endowed individuals are more likely to migrate and permanently settle out of war areas, the sample restriction would imply a downward bias for the war impact estimates.

To partly address concerns from the above sample selection, I take two approaches. First, province-level migration figures show that bulk of migration happens within provinces (intra-province). Furthermore, war provinces had higher than usual in-migration rates after the war. Therefore, in a robustness check, I define treatment status based on place of residence in 2006 for all individuals. This has little impact on the estimated war effects. Second, there is not a discernible difference between the fraction of non-migrant individuals within each birth cohort across war and non-war provinces. In other words, the same share of individuals from each cohort is included in the sample across war and non-war provinces.

The DD identification requires that in the absence of Iran-Iraq War the educational gap between war and non-war provinces stays the same for treated and control cohorts (parallel trends). Therefore simultaneous events that have a differential impact on cohorts in war provinces pose a challenge to causal inference. I discuss four such events. The two prominent ones are a baby boom episode and ethnic rebellions after the 1979 revolution. Iran had a dramatic increase in population growth during 1976 - 1986. The baby boom, however, resulted in similar birth increases in war and non-war provinces. Furthermore, adding controls for the number of schools and students at the primary level, does not change the significance of early cohorts impact. The second simultaneous event is a series of ethnic rebellions that started right after the revolution. Short lived

rebellions happened in Khuzestan, Azerbaijan, and Sistan but Kordestan uprising was the most prominent and continued until 1982. When I exclude Kordestan from the sample, the war effect changes very little. Furthermore, the war impact seems to be increasing over time which is in contrast to an expected impact of the rebellions that finished by the end of 1982.

This paper speaks to the vast literature on the impact of early childhood circumstances on adult outcomes and confirms conclusions in the literature that very young children are more vulnerable and could suffer long lasting effects from catastrophic events. On the specific subject of conflict, there are several papers that estimate impact of conflict on educational attainment of children using DD methodology. Using within country variation Shemyakina (2011) estimates that Tajikistan civil war had a significant impact on enrollment of girls and their rate of finishing mandatory schooling but she does not find any impact on boys. Many studies use a similar identification strategy and find significant negative impacts of conflict on educational attainment: Leon (2012) for Peru, Minoiu and Shemyakina (2014) for Cote d'Ivoire, Pivovarova and Swee (2015) and Valente (2014) for Nepal, Kecmanovic (2013) for Croatia, Verwimp and Van Bavel (2014) for Burundi, and Justino, Leone, and Salardi (2014) for Timor Leste, Akresh and Walque (2008) for Rwandan Genocide, Blattman and Annan (2010) for child soldiering in Uganda, Merrouche (2006) for Cambodia, Singh and Shemyakina (2016) for the impact of Punjab terrorism, and Chamarbagwala and Moran (2010) and Chamarbagwala

and Moran (2011) for Guatemala[3][4][5].

The rest of the paper is organized as follows. The next section gives a brief overview of Iran's education system and Iran-Iraq War. Section 3 and 4 describe the data and the identification strategy. In section 5, I present regression results. Section 6 tries to rule out alternative explanations of the estimated effects. The last section concludes.

2 Context

2.1 Education in Iran

In Iran, children start grade 1 of primary school at age 6 and with no grade repetition will graduate from grade 12 at age 18. At the final grades of primary, lower secondary and high school students sit through centrally administered exams to obtain the relevant degree. I use education codes available in the census data to determine educational attainment of individuals. I assume an individual has attained high school diploma if he has 11 or more years of schooling[6]. Figure 1 gives an overview of the expansion of

[3]Some papers use cross-country difference-in-difference identification. For example, Ichino and Winter-Ebmer (2004) compare Austria and Germany to countries that were not involved in WWII. They find that school age children exposed to WWII attained lower education relative to non-war cohorts. They also find significant earning losses 40 years after the war that could be attributed to lower educational attainment of these cohorts.

[4]There are also a few studies that look at other dimensions of human capital like health. As an example Akbulut-Yuksel (2010) finds significant impact of allied bombing on children educational attainment, health and adult labor market outcomes in Germany during WWII. She attributes the educational impact to the physical destruction of schools an teacher absence and the health impact to malnutrition during WWII.

[5]From a macroeconomic perspective catastrophic events could shift the equilibrium of the economy and leave local economies in a poverty trap. Empirical literature, however, was generally unable to provide support for this theoretical possibility. For example, Davis and Weinstein (2002) and Davis and Weinstein (2008) find no evidence of multiple equilibria in the context of allied forces bombing of Japanese cities. Japanese cities converge to their pre-bombing population trends in the long run. Miguel and Roland (2011) are unable to uncover local poverty traps for heavily destroyed areas after the Vietnam War. Bosker et al. (2007), however, seem to find some evidence of multiplicity for German cities subject to WWII destruction.

[6]In most pre-1992 years, high school diploma corresponds to finishing 12th grade. Post-1992, diploma was awarded after successfully finishing 11th grade.

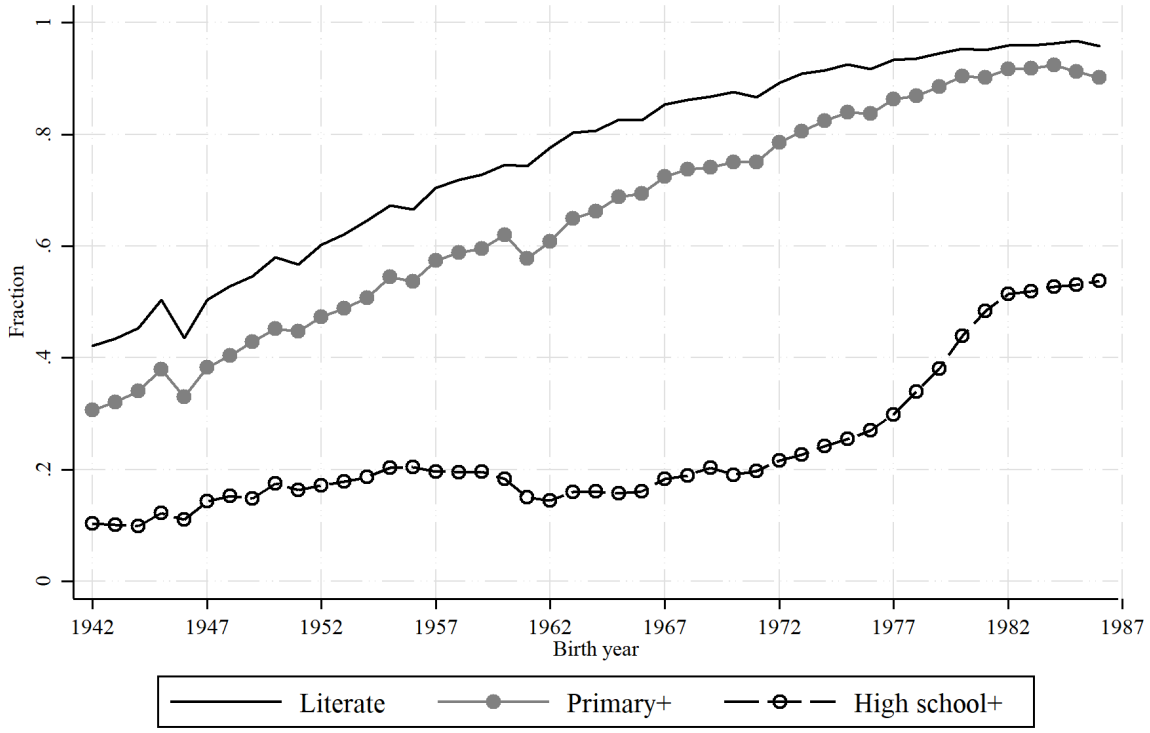


Figure 1: Expansion of education in Iran

Notes: Figure shows fraction of individuals with the specified degree in each birth cohort. Solid line shows fraction of literature individuals. Gray line with solid markers shows fraction of individuals with primary or higher degree. Dashed line with hollow markers show fraction of individuals with high school degree or higher. The figure is constructed from the 2006 census data restricting attention to individuals aged between -6 and 38 years old at the onset of war (September 1980).

modern education in Iran. For 1942 birth cohorts around 40 percent of individuals are literate but after 45 years, literacy rates are above 95 percent. Fraction of individuals with at least primary school attainment has risen sharply as well. Fraction of individuals with a high school degree has started to rise during 1970s and stands at about 54 percent for the youngest cohort. The increase in educational attainment was a result of school construction efforts and other education campaigns that started in 1960s and continued post the 1979 revolution.

2.2 Iran-Iraq War

Iran Iraq relationship was very contentious right from Iraq's independence in 1932. The major source of dispute was over the control of the bordering river, Arvand-Rud. However, except for a few skirmishes the relationship was by and large peaceful. The main agreement during this period was the Algiers Agreement in 1975 that set the frontier along the thalweg in Arvand-Rud allowing Iran to freely use the river's navigational routes. The 1979 Islamic revolution in Iran and the subsequent instability, however, encouraged Iraq to denounce the Algiers Agreement and to engage in an unprecedented large scale war lasting for about 8 years and claiming 213,255 lives on the Iranian side[7]. The war officially started on 22 September 1980, one day before the beginning of school year. Iraq started an ambitious ground invasion of Iranian territory along the 650 miles border. Until November 1980 Iraq captured vast swathes of Iranian territory including ten important cities and came close to a few major cities[8]. The advancement of Iraqi forces soon came to a halt and after some unsuccessful offensives during 1981, Iran was able to recover most of the occupied territory (including some major cities) until June 1982. From this time until the signing of UN's 598 resolution and the subsequent cease fire on 20 August 1988, there was virtually little territorial exchange and the war continued with battles along the border as well as air strikes on major cities. From the beginning till the end of the war all bordering villages and cities were battle fronts subject to constant shelling, aerial, and ground attacks. I define five provinces bordering Iraq as war provinces. These are also officially declared as war hit provinces and include Khuzestan, Illam, Kermanshah, Kordestan, and West Azerbaijan.

[7]Many books and articles are written on the background of the conflict and the development of the war during its 8 years. See Bakhash (2004), Cordesman (1987), Souresrafil (1989), Karsh (2002) and Hiro (1989) for detailed chronologies of war events and Cordesman and Wagner (1990) and Potter and Sick (2004) for in-depth analysis of war events.

[8]The captured cities are Khorramshahr, Susangerd, Bostan, Mehran, Dehloran, Ghasreshirin, Howeize, Naftshahr, Sumar, and Musian. The cities subject to continuous shelling are Abadan, Ahwaz, Andimeshk, Dezful, Shush, Islamabad, and Gilangharb.

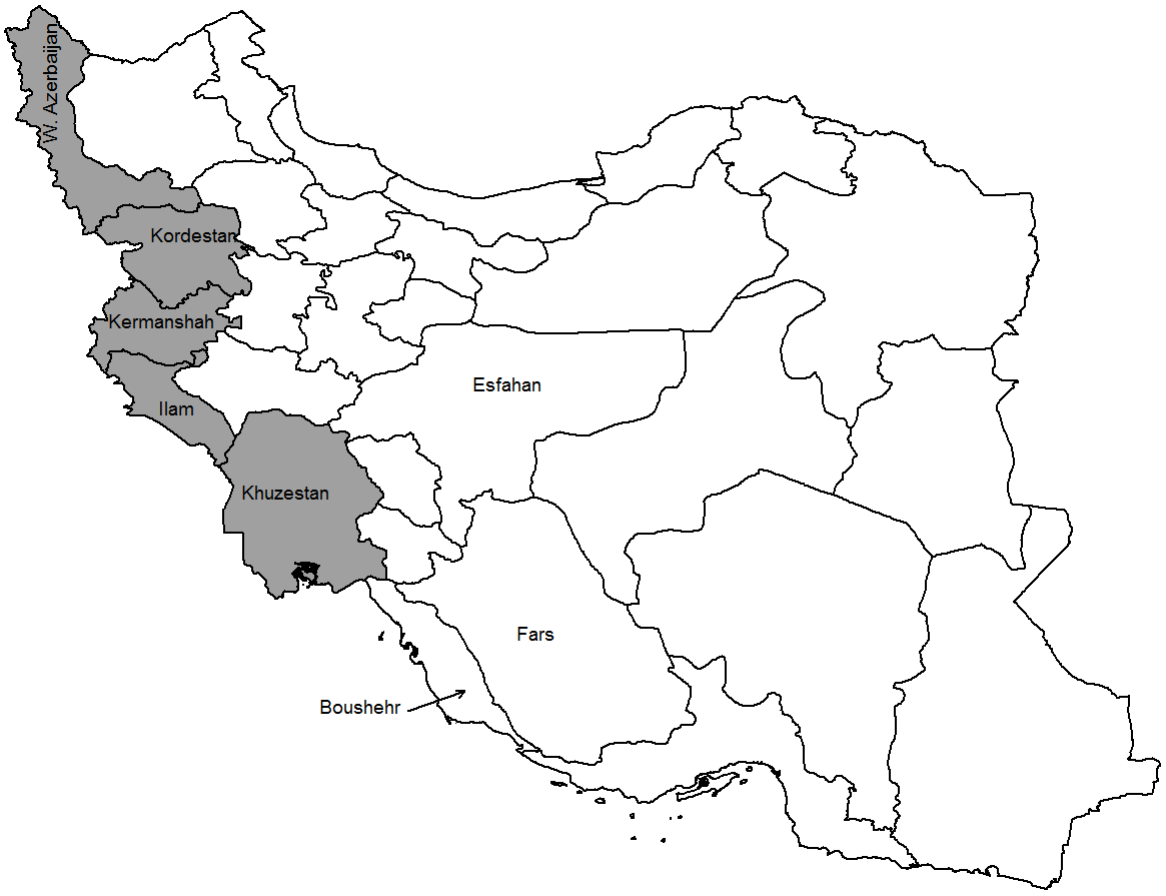


Figure 2: War hit provinces

Notes: Figure shows a map of Iran provinces. The grayed areas are the five provinces officially declared as war hit.

3 Data

The variables for my analysis are coming from a 2 percent sample of individual records of 2006 Iran Population Census from the Statistical Center of Iran (SCI)[9]. 2006 census administered an extended questionnaire to about 20 percent of randomly selected households. Current data is a 10 percent extraction of this sub-sample. The sampling unit is a household and it is stratified at district by urban location. It provides data on current residence, date of birth, migration during the past 10 years, educational attainment, employment status and other characteristics. The sample within each stratum is random but SCI provides individual probability weights (i.e. inverse of sampling probability) that is used in all of the analysis in this paper.

The main variable used for educational attainment is a dummy that shows whether the individual has finished high school. I focus on high school graduation because primary graduation rates are quite high among young cohorts (figure 1) and show little difference between war and non-war provinces[10].

Census records data only on current residence and whether the individual is living in his birth place. Therefore, I could only identify birth place of those living in their birth places in 2006[11]. I define these individuals as non-migrants[12] and estimate effects for this group. In section 6.1 I present supportive evidence to rule out sample selection as an alternative explanation of estimated effects.

I further restrict the sample to individuals aged between -6 to 38 years old at the

[9]This is freely available from Statistical Centre of Iran in Farsi and from IPUMS in English.

[10]Educational variables are derived from a single coded variable in the original dataset. I could also use years of education but the mapping from the coded variable to years of education is less clear and is subject to greater error.

[11]Since the war has ended 18 years before the Census I am unable to use migration questions (which relate to past 10 years) to identify war time residence of all individuals.

[12]Technically some of these individuals could be return migrants, i.e. those who have returned to their birth places after a temporary leave.

onset of war in September 1980 (i.e. 20 to 64 years old in 2006). The youngest cohort would start primary school in 1986 and is expected to finish high school by 2004[13]. On the other hand, very old cohorts have a small sample size and very low high school completion rates. Therefore, I restrict to cohorts aged 64 or less. The school year begins on 23 September each year and ends in June next year. Therefore, the age conditions outlined above are based on age as of 22 September. This is also the way I define birth cohorts throughout the paper. For example, all individuals born between 23 September 1941 and 22 September 1942 are assigned to the 1942 birth cohort and are 38 years old at the onset of war.

Table 1 shows summary statistics for the variables used in this study. Panel A presents summary statistics for the whole sample of 20 to 64 years old individuals while panel B restricts to non-migrants. About 65 percent of individuals in panel A are non-migrants, and appear in panel B while the rest (migrants) are shown in panel C. Migrants seem to be on average less educated and older. While 85 percent of non-migrants are literate about 82 percent of migrants are literate. Similarly 31 percent of non-migrants have finished high school while 26 percent of migrants did so. Migrants are mostly living in urban areas and are more likely to be married.

4 Empirical Strategy

I employ a difference-in-differences (DD) strategy to estimate the educational attainment impact of war. I compare the difference between average high school completion rates for cohorts exposed to war to those not exposed, across war and non-war provinces. I distinguish between two types of war exposure: before starting primary school and

[13]Even with two years of grade repetition the youngest cohort should have come out of high school by 2006.

Table 1: Summary Statistics

Variable	A: Whole sample			B: Non-migrants			C: Migrants		
	Obs.	Mean	S.D.	Obs.	Mean	S.D.	Obs.	Mean	S.D.
Literate	741,017	0.84	0.37	481,410	0.85	0.36	259,607	0.82	0.39
High school	741,017	0.29	0.45	481,410	0.31	0.46	259,607	0.26	0.44
Age	741,017	35.66	11.73	481,410	34.48	11.70	259,607	37.51	11.54
Female	741,017	0.50	0.50	481,410	0.49	0.50	259,607	0.53	0.50
Family size	741,017	4.53	1.94	481,410	4.60	1.99	259,607	4.42	1.86
Urban	741,017	0.71	0.45	481,410	0.65	0.48	259,607	0.81	0.39
Married	732333	0.77	0.42	474,520	0.72	0.45	255,079	0.86	0.35
Single	732,322	0.21	0.41	474,513	0.27	0.44	257,809	0.12	0.32
Widow	732,322	0.03	0.16	474,513	0.02	0.15	257,809	0.03	0.17
Divorced	732,322	0.01	0.09	474,513	0.01	0.10	257,809	0.01	0.09
Ind. is in birth place	738,258	0.65	0.48	-	-	-	-	-	-

Notes: Table shows actual number of observations, mean and standard deviation of main variables. Panel A is for the whole sample of individuals aged between 20 and 64 in Census 2006. Panel B consists of all individuals in panel A who are currently living in their birth place. Panel C restricts to individuals who are not living in their birth place. Panel B is the main sample used in the paper.

during primary school. In the sample, there are individuals who have exposure to one or both of these measures. Therefore, I define three dummy variables. Early_c is a dummy variable that equals to one if the individual is aged between -6 and -2 years old at the onset of war in 23 September 1980. Primary_c is equal to one if the individual is aged between 6 and 10 years old at the onset of war. These individuals spent at least one year of their primary school during the war. Finally Double_c captures individuals aged between -1 and 5 years old at the onset of war. These individuals spent at least one year of early childhood and one year of primary school during the war. Equation (1) shows the basic regression specification that implements the DD methodology with three treatment groups.

$$\begin{aligned} y_{icsd} &= \alpha + \beta \text{War\_prov}_s + \delta_E \text{Early}_c + \delta_S \text{Primary}_c + \delta_D \text{Double}_c \\ &\quad + (\gamma_E \text{Early}_c + \gamma_P \text{Primary}_c + \gamma_D \text{Double}_c) \times \text{War\_prov}_s + \epsilon_{icsd} \end{aligned} \tag{1}$$

where y_{icsd} is a dummy that shows whether individual i in birth cohort c living in province s and district d has finished high school, War_prov_s is equal to 1 for five war hit provinces, and α is a constant. Coefficients of interest are γ_E, γ_P, γ_D which respectively show the impact of war exposure during early childhood, during primary school, and during both. I cluster standard errors at district level to allow for correlated shocks for all cohorts within a given district[14]. I also estimate an extended specification where I control for district and cohort fixed effects as well as individual characteristics.

$$\begin{aligned} y_{icsd} =& \alpha + \beta_d + \delta_c + (\gamma_E \text{Early}_c + \gamma_P \text{Primary}_c + \gamma_D \text{Double}_c) \times \text{War\_prov}_s \\ &+ \Psi X_{icsd} + \epsilon_{icsd} \end{aligned} \tag{2}$$

where β_d and δ_c represent district and cohort dummies, and X_{icsd} is a set of individual or province level controls[15]. In a more stringent specification I allow for province specific linear trends in high school completion rates as well. The identification assumption is that in the absence of war, the difference between high school graduation rates across war and non-war provinces would have been the same for control and treated cohorts. In other words, I identify the war impact from changes in the size of war versus non-war educational gap. Therefore any other factor that affects younger cohorts in war provinces differently could pose a challenge to causal interpretation. DD is, however, robust to fix differences between provinces and country-wide cohort specific variation. I elaborate on potential concerns in section 6.

[14]Since there are 30 provinces in the sample, clustered estimators for standard errors at province level are not reliable.

[15]The set of controls included are indicators for urban households, gender of individual, family size, dummies for marital status and their interactions with War_prov dummy. It is not possible to include parents educational attainment as I do not observe that for individuals living apart from their parents.

5 Results

5.1 Basic results

I start by presenting average high school completion rates for treatment and control cohorts in table 2. The completion rate for the control group, individuals aged 18 to 38 years old at the onset of war[16], is reported in row I. Columns (1) and (2) show high school graduation rates respectively for war and non-war provinces. High school completion rates for early childhood cohorts, double treated cohorts, and primary cohorts are reported in row II, III, and IV respectively. Treated cohorts have on average higher high school graduation rates compared to row I. Also war provinces have lower high school completion rates relative to non-war provinces. The bold figures in column (3) are DD estimates. Early childhood exposure to war on average reduced high school completion rates by 7.2 percentage points in war provinces (significant at 1 percent). The impact on cohorts who spent at least one year of their childhood and one year of primary school is smaller and equal 3.5 percentage points. Finally there does not seem to be an effect on those who spent at least one year of primary during the war.

In rows V and VI of table 2, I compare two control cohorts as a placebo test. I compare individuals aged between 18 and 29 years old to those aged 38 and 30 years old at the onset of war across provinces. The DD estimate for high school graduation shows the war non-war gap has widened for younger cohorts by 1.1 percentage points. This effect is however, much smaller than DD estimates for treated cohorts and is insignificant at conventional levels.

[16] I would not expect the war to have an impact on these individuals because they are expected to be out of high school when the war started.

Table 2: Average rate of finishing high school

	High school graduation		
	Province		
	war	Non-war	difference
	(1)	(2)	(3)
I. Control: aged$\in [38, 18]$ in 1980	0.122	0.166	-0.044
	(0.015)	(0.035)	(0.038)
II. Early: aged$\in [-6, -2]$ in 1980	0.438	0.554	-0.115
	(0.016)	(0.032)	(0.036)
Difference (II - I)	0.316	0.388	**-0.072**
	(0.012)	(0.007)	(0.013)
III. Double: aged$\in [-1, 5]$ in 1980	0.305	0.383	-0.078
	(0.014)	(0.030)	(0.033)
Difference (III - I)	0.182	0.217	**-0.035**
	(0.011)	(0.007)	(0.012)
IV. Primary: aged$\in [6, 10]$ in 1980	0.177	0.218	-0.042
	(0.008)	(0.027)	(0.028)
Difference (IV - I)	0.054	0.052	**0.002**
	(0.010)	(0.008)	(0.013)
V. Placebo: aged$\in [30, 38]$ in 1980	0.083	0.119	-0.036
	(0.014)	(0.032)	(0.035)
VI. Placebo: aged$\in [29, 18]$	0.137	0.184	-0.047
	(0.015)	(0.035)	(0.038)
Difference (VI - V)	0.054	0.065	**-0.011**
	(0.006)	(0.005)	(0.008)

Notes: Columns (1) and (2) show average rates of finishing high school for cohorts born in war and non-war provinces respectively. Column (3) reports the difference between column (1) and (2). The row labeled "Difference" takes the difference between the treated cohort and the control cohort (row I). The bold figures in column (3) shows DD estimate of the war impact based on the cohorts compared. Standard errors are clustered at district level (335 clusters) and reported in parenthesis below coefficients. The sample restricts to individuals living in their birth place (Panel B of table 1.

5.2 Regression results

Table 3 shows estimation results for various specifications using high school completion dummy as the dependent variable. Here I only report γ_E, γ_D, and γ_P (coefficients of the interaction terms). Column (1) reports estimates from the basic specification with no controls (equation (1)). This should correspond to the bold figures in table 2 with a slight change since I included cohorts aged between 11 and 18 years old in the current regression but not in table 2[17].

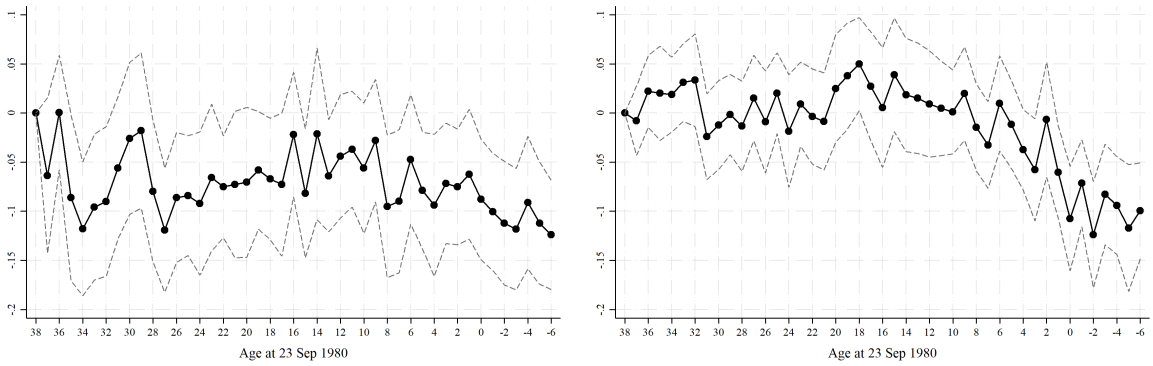
All three coefficients of interest show slight changes as I add province and cohort fixed effects, district fixed effects, controls, and province specific linear trends respectively in column (2) to (5). Column (4) corresponds to specification (2) and shows that cohorts that spent at least one year of their early life during the war are on average 8 percentage points less likely to finish high school. Similarly for cohorts that spent at least one year of early life and primary school during the war the probability of finishing high school is reduced by 3.5 percentage points. Both these effects are significant at 1 percent level. There is, however, no impact on cohorts that spent only primary school during war.

In the last column of table 3, I collapse the data to province-cohort observations and control for province and cohort fixed effects. The magnitude of the effects are now larger and all are significant. The results, however, all become insignificant if I control for province specific linear trends in the collapsed data (not reported).

Table 4 splits the sample based on gender and place of residence. It turns out that the war impact on females is almost double that of males (columns (1) and (2)). Furthermore the magnitude of the effect seems to be larger for females in rural areas.

To get a better idea of the war impact on different cohorts, I extend the regression in

[17] With more cohorts I would have statistical power to identify coefficients even in presence of province specific trends.



(a) Male (b) Female

Figure 3: Coefficients estimates for interactions of cohort by war province

Notes: Figure plots coefficient estimates and 95 percent confidence intervals for the full set of birth cohort by War_prov interactions as in equation (3). Dependent variable is whether the individual has finished high school. Individuals aged 38 years old at the onset of war are used as the reference group. Sample used for regressions is non-migrant individuals aged between [-6,38] years old at the onset of war. Regressions use sampling weights and standard errors are clustered at district level.

(2) and include a whole set of cohort by war province interaction terms as follows:

$$y_{icsd} = \alpha + \beta_d + \delta_c + \sum_{k=-6}^{37} (\text{War\_prov}_s \times \text{d}_{ik})\gamma_k + \Psi X_{icsd} + \epsilon_{icsd} \tag{3}$$

where d_{ik} is a set of cohort dummies[18], and other variables are as in (2). γ_k captures the average difference between individuals in cohort k living in war and non-war provinces relative to the cohort of individuals aged 38 years at the onset of war. γ_k is expected to be zero for cohorts who finished schooling before the war and should become negative for younger cohorts. Figure 3 shows the estimated γ_ks and their 95 percent confidence intervals. Figure 3a reveals that the war effect on males is noisier and of a similar magnitude to coefficient estimates for control cohorts. Figure 3b reveals a clear negative impact on cohorts aged 1 years old or younger at the onset of war. These figures shed light on the validity of parallel trends assumption. For the female sample the coefficient estimates for almost all cohorts older than 1 years old at the onset of war is insignificant and small. A similar but less robust pattern is observed for the male sample.

[18]Individuals aged 38 at the onset war are taken as the reference group and hence omitted.

Table 3: Main regression results

Dep. Var. High school	(1)	(2)	(3)	(4)	(5)	(6)
Double×War_prov	-0.040***	-0.040***	-0.038***	-0.035***	-0.041**	-0.074***
	(0.011)	(0.011)	(0.010)	(0.010)	(0.019)	(0.020)
Early×War_prov	-0.077***	-0.078***	-0.078***	-0.080***	-0.088***	-0.121***
	(0.012)	(0.012)	(0.012)	(0.011)	(0.027)	(0.045)
Primary×War_prov	-0.003	-0.003	-0.001	-0.003	-0.006	-0.034***
	(0.009)	(0.010)	(0.010)	(0.011)	(0.008)	(0.013)
Obs.	456,977	456,977	456,977	450,387	450,387	1,350
R^2	0.107	0.155	0.180	0.232	0.234	0.931
Controls	N	N	N	Y	Y	Y
Prov. FE	N	Y	N	N	N	Y
District FE	N	N	Y	Y	Y	N
Cohort FE	N	Y	Y	Y	Y	Y
Prov. Lin. trends	N	N	N	N	Y	N

Notes: Table shows coefficient estimates and standard errors from 6 OLS regressions. Dependent variable is a dummy showing whether the individual has finished high school. Early, Double, and Primary are three indicators capturing cohorts aged [-6,-2], [-1,5], and [6,10] years old at the onset of war (September 1980). I report only the three coefficients of interest, equation (1) show the specification for column (1). Equation (2) show full specifications for columns (2) to (5). Control variables included in columns (4), (5) are dummies for urban, gender, and marital status, and family size, and their interactions with War_prov. Column (2) includes province and cohort fixed effects. Columns (3) to (5) include district and cohort fixed effects. Column (5) controls for province specific linear trends. Column (6) runs the estimation on the collapsed sample (cohort-province observations) and controls for province and cohort fixed effects plus the control variables used in column (4) averaged at province level. In all cases standard errors are adjusted for district clusters (around 335 clusters). *, **, and *** respectively show significance at 10, 5, and 1 percent levels. All regressions use sampling weights. Sample restricts to individuals living in their birth place and aged [-6,38] years old at the onset of war.

Table 4: Heterogeneity of effects

	All		Rural		Urban	
	Male	Female	Male	Female	Male	Female
Dep. Var. High school	(1)	(2)	(3)	(4)	(5)	(6)
Double×War_prov	-0.0255	-0.0559***	-0.0111	-0.0387***	-0.0409	-0.0695***
	(0.0200)	(0.0203)	(0.0102)	(0.0110)	(0.0288)	(0.0254)
Early×War_prov	-0.0646*	-0.1029***	-0.0439**	-0.0968***	-0.0737*	-0.1059***
	(0.0330)	(0.0268)	(0.0170)	(0.0189)	(0.0434)	(0.0325)
School×War_prov	-0.0041	-0.0105	-0.0058	0.0005	-0.0123	-0.0223
	(0.0109)	(0.0126)	(0.0097)	(0.0059)	(0.0145)	(0.0192)
Obs.	232,124	218,263	109,189	100,125	122,935	118,138
R^2	0.1742	0.3212	0.1329	0.2164	0.1045	0.2771

Notes: Table shows coefficient estimates and standard errors from 6 OLS regressions that use the same specification but different samples. Dependent variable is a dummy showing whether the individual has finished high school. Early, Double, and Primary are three indicators capturing cohorts aged [-6,-2], [-1,5], and [6,10] years old at the onset of war (September 1980). I report only the three coefficients of interest. All columns include cohort and district fixed effects, province specific linear trends, and dummies for urban, gender, and marital status, and family size, and their interactions with War_prov. In all cases standard errors are adjusted for district clusters (around 335 clusters). *, **, and *** respectively show significance at 10, 5, and 1 percent levels. All regressions use sampling weights. Sample restricts to individuals living in their birth place and aged [-6,38] years old at the onset of war. In columns (1) and (2) I respectively look at the male and female sub-samples. In columns (3) and (4) I look at the male and female sub-samples in rural areas. Columns (5) and (6) looks at male and female sub-samples in urban areas.

6 Alternative Explanations

Before interpreting the estimated impacts as causal, I would need to address several concerns. In the following subsections, I first present some evidence that help alleviate concerns about sample selection. Then, I try to rule out several post revolution events including a baby boom episode and ethnic rebellion as alternative stories that might explain the results. Table 5 shows several robustness checks that will be discussed in the following subsections. Column (1) reports the benchmark estimation results for ease of comparison (column (5) of table 3).

6.1 Migration effects

Since I only observe the birth place of individuals if they lived in their birth places in 2006, I could only define the treatment status for non-migrants. Therefore, the

Table 5: Robustness regressions

	Main	incl. migrants	Excl. Khuzes-tan	Excl. Kordestan	Excl. Centers	Input controls
Dep. Var. High school	(1)	(2)	(3)	(4)	(5)	(6)
Both	-0.041**	-0.035**	-0.014	-0.044**	-0.029***	-0.012
×War_prov	(0.019)	(0.014)	(0.018)	(0.020)	(0.009)	(0.017)
Early	-0.088***	-0.082***	-0.067**	-0.081***	-0.069***	-0.039**
×War_prov	(0.027)	(0.020)	(0.029)	(0.028)	(0.0122)	(0.018)
School	-0.006	-0.010	0.009	-0.011	-0.002	0.005
×War_prov	(0.008)	(0.008)	(0.007)	(0.008)	(0.007)	(0.011)
Obs.	450,387	693,601	425,012	441,684	351,100	344,738
R^2	0.234	0.205	0.237	0.234	0.222	0.221

Notes: Table shows results of 6 regressions using high school completion as the dependent variable. Different columns use different samples but all columns include cohort and district fixed effects, province specific linear trends, and the set of controls specified in table 3. Column (1) is the same as column (5) in table 3. Column (2) includes migrants and non-migrants in the regression, and defines treatment status based on current place of residence. Column (3) excludes Khuzestan province. Column (4) excludes Kordestan province. Column (5) excludes all districts that contain the provincial capital cities. Column (6) merges census data with province-cohort data on number of students and schools and includes log of these variables in the regression (educational inputs). Because of data unavailability the sample here includes only individuals aged [-6,20] years old at the onset of war. All regressions use sampling weights. In all cases standard errors are adjusted for district clusters. *, **, and *** respectively show significance at 10, 5, and 1 percent levels.

estimated war effects are based on the comparison of non-migrants across cohorts and provinces. To the extent that educational attainment of migrants is different from that of non-migrants, the estimated war impact is biased[19]. I take two approaches to partly respond to this threat. First, I define treatment status based on current residence and therefore assign all individuals living in war provinces as treated. Estimation results show little change in all coefficients relative to the benchmark specification that used only the sample of non-migrants (table 5 column (2)).

Below I try to establish two facts: a) most of migration in Iran is intra-province migration, and b) there is some evidence that war migrants returned to their home province. The alternative treatment assignment based on current residence might be better justified if the majority of reshuffling is within the same province and if many individuals returned to their home province after the war.

A survey of war migrants shows more than 1.6 million individuals (around one fifth of population) were displaced by June 1982[20]. Based on this survey Khuzestan accounts for almost 77 percent of all migrants. Furthermore, 48 percent of migrants were settled in their pre-war provinces. Excluding Khuzestan from the sample reduces the early cohort coefficient slightly and makes the double treated cohort coefficient insignificant but the the overall pattern of results remains the same (Column (3) of table 5).

To further shed light on facts a) and b), table 6 collects province-level migration data from 1986, 1996, and 2006 population censuses. Panel A and B report inter- and intra-province migration numbers respectively. Panel A, columns (1) to (3), show the number of out-migrants, while columns (4) to (5) show the number of in-migrants. Share of out-migration from war provinces in fairly stable over the three rounds of census but

[19]Forced migration itself is a mechanism for the impact of war on educational attainment. Interruption of schooling due to forced migration could result in school dropout. The bias discussed above is due to exclusion of war migrants who settled in locations other than their birth place.

[20]"Findings of the imposed war migrants survey", November 1982, Statistical Center of Iran. Results of similar surveys in later years indicates that 1.6 million is the maximum number of displaced people.

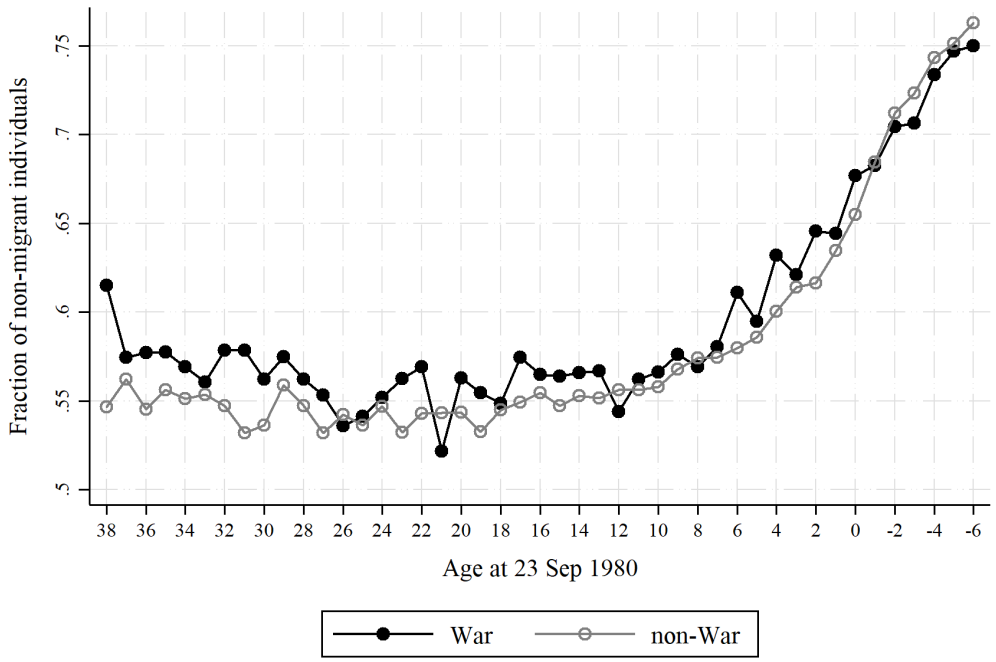


Figure 4: Fraction of non-migrant individuals in each cohort

Notes: Figure shows fraction of individuals who are living in their birth places in 2006 Census for various birth cohorts in war and non-war provinces. Sample used here is the full sample of individuals aged between -6 to 38 years old at the onset of war (23 September 1980).

share of in-migration peaks at 18 percent in 1996 while it is around 10 percent in the 1986 and 2006. This suggest an unusual influx of people into war provinces. Looking at panel B, the share of war provinces from all intra-province migration is particularly high in 1986 and 1996 (39 and 24 percent respectively). But it falls to 16 percent in 2006 census. This is suggestive of the fact that during and after the war, within province movements were intensified in war provinces.

The second way I tackle the migration problem is to see whether migration probability is affected by the treatment. Figure 4 plots the average fraction of non-migrants for each cohort in war and non-war provinces. Interestingly, war and non-war provinces have fairly similar fraction of non-migrants across cohorts. Running a regression confirms that the probability of migration is unaffected by the treatment (results not shown).

Table 6: Provincial migration patterns during and after the war

		Out-migration			In-migration		
		1986	1996	2006	1986	1996	2006
		(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Inter-province migration							
I. War	No.	513,016	545,875	805,867	242,072	583,617	496,316
	share	18%	17%	17%	9%	18%	10%
II. Non-war	No.	2,287,312	2,615,016	4,076,264	2,558,256	2,577,274	4,385,815
	share	82%	83%	83%	91%	82%	90%
III. Total	No.	2,800,328	3,160,891	4,882,131	2,800,328	3,160,891	4,882,131
	% inter	54%	39%	40%	54%	39%	40%
Panel B: Intra-province migration							
VI. War	No.	929,742	1,208,780	1,160,849	-	-	-
	share	39%	24%	16%	-	-	-
V. Non-war	No.	1,472,440	3,821,651	6,096,893	-	-	-
	share	61%	76%	84%	-	-	-
VI. Total	No.	2,402,182	5,030,431	7,257,742	-	-	-
	% Intra	46%	61%	60%	-	-	-

Notes: Table shows aggregate migration figures from three rounds of census (1986, 1996, 2006). Each census asks about migration in the 10 years preceding census date. Each row labeled as "No." shows number of migrants while rows labeled as "share" show the share out total migrants. For example in row I, No. shows the number of migrants from/to war provinces in each round of census and share shows percent of migrants from/to war provinces from total number of migrants in that census round. Panel A reports inter-province migrants while panel B looks at Intra-province migrants. In panel A and B the last row shows percentage of inter and intra-province migrants from all migrants in each round of census.

6.2 Post revolution events

Four post revolution events warrant some discussion. First, between 1976 and 1986 Iran had a baby boom with an average yearly population growth rate of 3.9 percent. If the baby boom had a differential impact on war and non-war provinces it could challenge the estimated war impact. To shed light on the evolution of the baby boom, figure 5 shows average number of registered births across war and non-war provinces over time. While non-war provinces have on average higher number of births and the number of births sharply rises after 1979, the difference between war and non-war provinces is stable. To control for the impact of baby boom on educational inputs, I include log of number of schools and students for each province-cohort group in the benchmark regression. This reduces the impact of war on early cohorts to 3.9 percentage points (still significant) but makes the double treated coefficient insignificant and small (column (6), table 5). This reduction is, however, mostly due to a smaller sample because the two measures are unavailable for cohorts born prior to 1960 (aged 21 or more at the onset of war).

The second range of events that could potentially bias war estimates are ethnic rebellions in West Azerbaijan, Kordestan, and Khuzestan right after the revolution. Many of these rebellions were small scale and lasted for a few months but the Kordestan uprising lasted until the first half of war. In table 5, column (4), I exclude Kordestan and results do not change significantly. Similarly removing Khuzestan (column (3)) and West Azerbaijan (not shown) does not have a significant effect on results.

Apart from the baby boom and ethnic rebellions, two other events happened at the same time as the war. Right after the 1979 revolution, some factions of the revolutionary groups started to oppose the policies undertaken by the mainstream forces. Soon the opposition moved underground and embarked on assassinations and terrorist bombings in a few major cities between 1979 and 1982. Several observations make it less likely

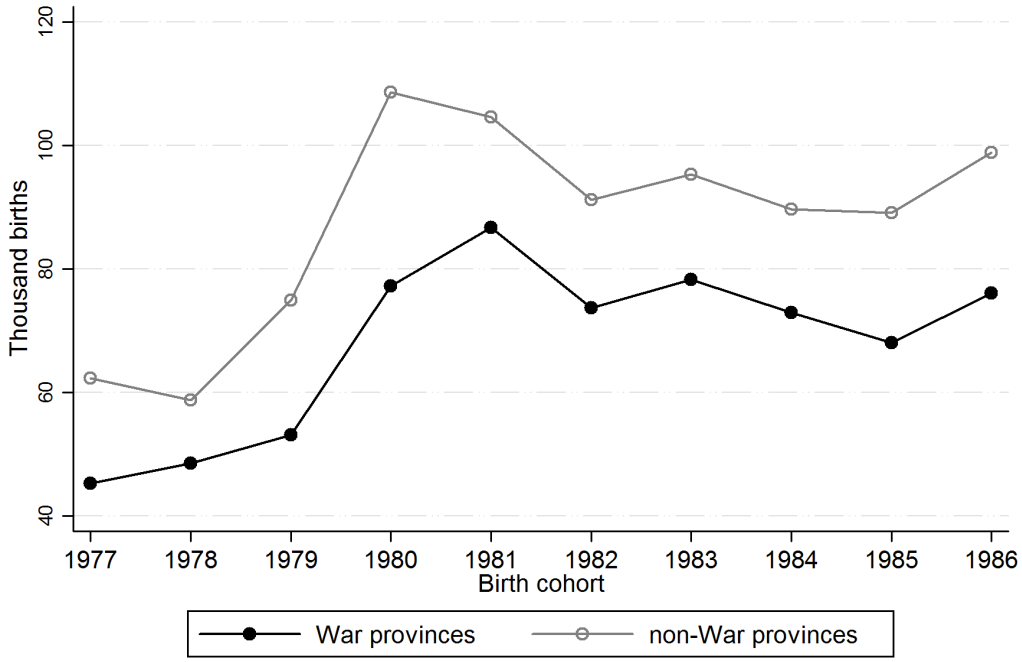


Figure 5: Average provincial registered births

Notes: Figure plots average number of births in war and non-war provinces. Source of this data is Statistical Yearbooks from SCI over various years. Registered births are different from actual births during a calendar year because some birth events are registered with delay.

that the terrorist activities are responsible for the estimated effects. First, most of terrorist activities took place in major cities (often Tehran and other province capitals). However, when I exclude all province capital districts, the estimated war impact changes slightly (table 5, column (5)). The second observation that alleviates concerns is the fact that the treatment effect seems to be stronger for younger cohorts (figure 3). This is despite the fact that little terrorist activities happened after 1982.

The last event that requires some explanation is the Cultural Revolution which closed all universities between 1980 and 1982. The stated objective was to bring the tutoring in line with Islamic thought. This event could reduce incentives for finishing high school as the prospect of entering university was unclear. However, it is not entirely obvious that the Cultural Revolution had a heterogeneous impact on war provinces. Furthermore, the strongest impact of the war is on cohorts who started primary or are born during the war. These cohorts are quite far from university education and the universities were expected to open soon.

7 Conclusions

In this paper I estimated the reduced form impact of Iran-Iraq War on educational attainment of children. DD estimates suggest probability of finishing high school is reduced by 8.8 percentage points for cohorts exposed to war in early life, whereas cohorts that spent some years of their schooling during the war did not receive a significant effect. The results of my analysis show very young and unborn children are more susceptible to adverse shocks. This suggests that spending more resources for families with very young children or pregnant women is a reasonable remedial policy for war affected areas.

Acknowledgments

I would like to thank Tim Besley, Roohollah Honarvar and seminar participants at LSE Development PhD seminar for comments. All remaining errors are mine.

References

Ahmadi, K., M. Reshadatjo, N. Sepehrvand, P. Ahmadi, and H. Yaribeygi. 2010. "Evaluation of vicarious PTSD among children of Sardasht chemical warfare survivors 20 years after Iran-Iraq war." *Journal of Applied Sciences* 10 (23):3111–6.

Ahmadi, Khoda Bakhsh, M. Reshadatjou, Gholam Reza Karami, and J. Anisi. 2009. "Vicarious PTSD In Sardasht Chemical Warfare Victims' wives." *Journal of Behavioral Sciences* 3 (3):195–199.

Akbulut-Yuksel, Mevlude. 2010. "Children of War: The Long-Run Effects of Large-Scale Physical Destruction and Warfare on Children." *Working Paper* .

Akresh, Richard and Damien de Walque. 2008. "Armed Conflict and Schooling: Evidence from the 1994 Rwandan Genocide." *Policy Research Working Paper 4606, The World Bank* .

Almond, Douglas. 2006. "Is the 1918 Influenza Pandemic Over? Long-Term Effects of In Utero Influenza Exposure in the Post-1940 U.S. Population." *Journal of Political Economy* 114 (4):672–712.

Almond, Douglas and Janet Currie. 2011a. "Chapter 15: Human Capital Development before Age Five." In *Handbook of Labor Economics*, vol. Volume 4, Part B, edited by Ashenfelter Orley and Card David. Elsevier, 1315–1486.

———. 2011b. "Killing Me Softly: The Fetal Origins Hypothesis." *Journal of Economic Perspectives* 25 (3):153–172.

Almond, Douglas, Lena Edlund, Hongbin Li, and Junsen Zhang. 2010. "Long-Term Effects of Early-Life Development: Evidence from the 1959 to 1961 China Famine." In *The Economic Consequences of Demographic Change in East Asia*, edited by Takatoshi Ito and Andrew K. Rose. NBER–East Asia Seminar on Economics, vol. 19. Chicago and London: University of Chicago Press, 321–345.

Almond, Douglas, Lena Edlund, and Marten Palme. 2009. "Chernobyl's Subclinical Legacy: Prenatal Exposure to Radioactive Fallout and School Outcomes in Sweden." *Quarterly Journal of Economics* 124 (4):1729–1772.

Almond, Douglas and Bhashkar Mazumder. 2005. "The 1918 Influenza Pandemic and Subsequent Health Outcomes: An Analysis of SIPP Data." *American Economic Review* 95 (2):258–262.

———. 2011. "Health Capital and the Prenatal Environment: The Effect of Ramadan Observance during Pregnancy." *American Economic Journal: Applied Economics* 3 (4):56–85.

Almond, Douglas, Bhashkar Mazumder, and Reyn van Ewijk. forthcoming. "Fasting during pregnancy and children's academic performance." *Economic Journal* .

Bakhash, Shaul. 2004. "Chapter 1: The Troubled Relationship: Iran and Iraq, 1930-80." In *Iran, Iraq, and the Legacies of War*, edited by Lawrence G. Potter and Gary G. Sick. Palgrave Macmillan.

Blattman, Christopher and Jeannie Annan. 2010. "The Consequences of Child Soldiering." *Review of Economics and Statistics* 92 (4):882–898.

Bosker, Maarten, Steven Brakman, Harry Garretsen, and Marc Schramm. 2007. "Looking for multiple equilibria when geography matters: German city growth and the WWII shock." *Journal of Urban Economics* 61 (1):152–169.

Chamarbagwala, Rubiana and Hilcias E. Moran. 2010. "The Legacy of War: Post-War Schooling Inequality in Guatemala." .

———. 2011. "The human capital consequences of civil war: Evidence from Guatemala." *Journal of Development Economics* 94 (1):41–61.

Cordesman, Anthony H. 1987. *The Iran-Iraq war and Western security 1984-87: strategic implications and policy options*. Jane's.

Cordesman, Anthony H. and Abraham Wagner. 1990. *The Lessons Of Modern War, Vol. 2: The Iran-Iraq War*. Boulder, Colo. : London: Westview Press, 1ST edition ed.

Davis, Donald R. and David E. Weinstein. 2002. "Bones, Bombs, and Break Points: The Geography of Economic Activity." *American Economic Review* .

———. 2008. "A Search For Multiple Equilibria In Urban Industrial Structure." *Journal of Regional Science* 48 (1):29–65.

Hiro, Dilip. 1989. *The Longest War: The Iran-Iraq Military Conflict*. Psychology Press.

Ichino, Andrea and Rudolf Winter-Ebmer. 2004. "The Long-Run Educational Cost of World War II." *Journal of Labor Economics* 22 (1):57–87.

Justino, Patricia, Marinella Leone, and Paola Salardi. 2014. "Short- and Long-Term Impact of Violence on Education: The Case of Timor Leste." *The World Bank Economic Review* 28 (2):320–353.

Kadivar, Hooshang and Stephen C. Adams. 1991. "Treatment of chemical and biological warfare injuries: insights derived from the 1984 Iraqi attack on Majnoon Island." *Military medicine* 156 (4):171–177.

Karsh, Efraim. 2002. *The Iran-Iraq War, 1980-1988*. Osprey Publishing.

Kecmanovic, Milica. 2013. "The Short-run Effects of the Croatian War on Education, Employment, and Earnings." *Journal of Conflict Resolution* 57 (6):991–1010.

Khateri, Shahriar, Mostafa Ghanei, Saeed Keshavarz, Mohammad Soroush, and David Haines. 2003. "Incidence of lung, eye, and skin lesions as late complications in 34,000 Iranians with wartime exposure to mustard agent." *Journal of occupational and environmental medicine* 45 (11):1136–1143.

Leon, Gianmarco. 2012. "Civil conflict and human capital accumulation the long-term effects of political violence in peru." *Journal of Human Resources* 47 (4):991–1022.

Mahvash, Janmardy. 2011. "Study of divorce in Iran provinces from 1977 to 1998: Emphasis on the role of Iran-Iraq war." *International Journal of Sociology and Anthropology* 3 (4):132–138.

Merrouche, Ouarda. 2006. "The Human Capital Cost of Landmine Contamination in Cambodia." *HiCN Working Paper 25* .

Miguel, Edward and Gerard Roland. 2011. "The long-run impact of bombing Vietnam." *Journal of Development Economics* 96 (1):1–15.

Minoiu, Camelia and Olga N. Shemyakina. 2014. "Armed Conflict, household victimization, and child health in Cote d'Ivoire." *Journal of Development Economics* 108:237–255.

Mofid, Kamran. 1990. *The economic consequences of the Gulf War*. Routledge, Taylor & Francis Group.

Pivovarova, Margarita and Eik Leong Swee. 2015. "Quantifying the Microeconomic Effects of War Using Panel Data: Evidence From Nepal." *World Development* 66:308–321.

Potter, Lawrence G. and Gary G. Sick. 2004. *Iran, Iraq, and the Legacies of War*. Palgrave Macmillan.

Shemyakina, Olga. 2011. "The effect of armed conflict on accumulation of schooling: Results from Tajikistan." *Journal of Development Economics* 95 (2):186–200.

Singh, Prakarsh and Olga N Shemyakina. 2016. "Gender-differential effects of terrorism on education: The case of the 1981–1993 Punjab insurgency." *Economics of Education Review* 54:185–210.

Souresrafil, Behrouz. 1989. *The Iran-Iraq war*. Guinan Lithographic Co.

Valente, Christine. 2014. "Education and Civil Conflict in Nepal." *The World Bank Economic Review* 28 (2):354–383.

Verwimp, Philip and Jan Van Bavel. 2014. "Schooling, Violent Conflict, and Gender in Burundi." *The World Bank Economic Review* 28 (2):384–411.