H i C N Households in Conflict Network

Conflict displacement and labor market outcomes in post-war Bosnia & Herzegovina

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Abstract: The 1992/95 war in Bosnia and Herzegovina (BiH) drove about 1.3 Million people into displacement (UNHCR). This study uses a longitudinal data source to document the nature of individual selection into conflict-induced displacement and the effects of displacement on labor market outcomes for Bosnians in post-war BiH. To account for endogeneity in the displacement status, I exploit the fact that the level of violence in the pre-war residence likely affected the displacement decision for Bosnians and yet is not associated to economic performance. I find evidence of positive selection into displacement, i.e. more "able" individuals in terms of labor market outcomes are more likely to be displaced, and that displaced Bosnian men and women are less likely to be in work relative to stayers. Interestingly, whereas worklessness translates into higher unemployment for men, it decreases the women's participation with no effect on unemployment once selection is accounted for. The informality of the labor market in BiH and the destruction of networks are not only the most plausible candidates to explain the negative effect of displacement on labor market performance, but also help rationalise the lack of an effect on participation for displaced men.

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1   Introduction

One direct consequence of armed conflicts is population displacement. Those parts of the population who are uprooted from their original place of residence are likely to experience particularly stark socio-economic vulnerability during and after the period of conflict (CIET International, 1997). The unique characteristics of the 1992/95 Bosnia and Herzegovina (BiH) conflict and the devastating consequences on the Bosnians[1] in particular present an analytical opportunity with both academic and policy relevance. First, the path of the war was determined by the non-economic desire of the Serbs to spatially separate the ethnic groups. Second, the Bosnian civilian population suffered the brunt of the war as 64,036 of the casualties were Bosnian (or about 3 percent of the pre-war Bosnian population) of whom only 52 percent were soldiers, compared to 24,905 for the Serbs of whom 84 percent were soldiers (Ball *et al.*, 2007). Finally, displacement was widespread –approximately 1.3 million people were displaced during the conflict and 1 million resettled in BiH after the conflict.

This study documents the nature of selection into displacement and the labor market effects of conflict displacement in the context of the 1992/95 BiH war. I use longitudinal post-conflict household survey data containing rich information on labor market outcomes, migration status and other individual characteristics to estimate the effect of displacement on a number of labor market outcomes of the displaced Bosnians relative to their stayer counterparts.[2] I account for potential endogeneity between displacement and individual labor market outcomes by using a method of instrumental variable (IV) estimation.

The results of this study contribute to two strands of literature: individual selection into conflict-induced migration and the cost of conflict at the microeconomic level. While individual selection into economic migration is well researched[3], the literature offers no empirical evidence on the nature of the selection into conflict displacement, most likely due to the scarcity of adequate sources of identification.[4] This study contributes to filling this gap by documenting the nature of the selection into displacement in the context of civil conflict.

A growing number of studies have emerged that offer a measure of the cost of conflict at the microeconomic level.[5] For instance, Blattman (2006) and Shemyakina (2006) find that civil conflicts in Uganda

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[1] By Bosnians, I refer to muslim Bosnians, as opposed to Serb and Croat Bosnians to whom I refer to as Serbs and Croats respectively.

[2] This study therefore does not assess either the extent of ethnic discrimination in BiH or the process of cultural assimilation by any of the main ethnic groups represented on the territory. For a contribution on the topic of cultural assimilation of foreign immigrants and language in the US context, see Lazear (1999).

[3] For instance, Chiswick (1999) outlines why we should expect economic migrants to be positively selected. Borjas (1987) documents that immigrants to the US can be negatively selected when the earning distribution is more unequal in the home country than in the host country. De Coulon and Piracha (2005) show that Albanian immigrants returning to Albania are negatively selected relative to the stayers.

[4] Interestingly, however, Chiswick (1999) notes that (positive) selection is likely to be less prominent in the case of non-economic migrants such as "refugees".

[5] A large body of literature also establishes the link between civil conflicts and a country's socioeconomic performance

and Tajikistan significantly reduced school attendance and grade completion.[6] Kondylis (2007a) assesses the cost of conflict at the household level with regard to agricultural productivity following the conflicts in Rwanda. In addition, a number of microeconomic studies suggest a causal link between poverty, the lack of economic prospects in particular, and the likelihood of joining a militia (Deininger, 2003; Bigombe et al., 2000; Verwimp, 2005). By providing an analysis of post-conflict economic performance, this study also contributes to this strand of literature on conflict prevention.

This is the first study to analyze the nature of selection into displacement during a conflict and to assess the effect of conflict-induced displacement on the labor market outcomes of the displaced. The effect of displacement is measured on a number of individual labor market outcomes, including indicators of work, unemployment, inactivity, wages and hours worked, controlling for individual characteristics, displacement status and municipality of destination fixed effects.[7] The labor market outcomes of the displaced are compared to those of stayers. Displacement status is defined using the date of the last migration: a person who reports having migrated during the period of the conflict or soon after –while resettlement and returns were still occurring on a large scale– is considered a displaced person. Since a displaced person's decision to resettle in her municipality of origin as opposed to moving to a new municipality of destination constitutes an outcome variable, the effect of displacement measured in this study is averaged over those who returned to their municipality of origin and those who did not.

Typically, displacement as such is contaminated by problems of self-selection that might lead to endogeneity issues when used as a regressor on individual labor market outcomes. Therefore, using an OLS or a random-effect error component regression framework to estimate the effect of displacement would likely produce inconsistent estimates. To account for this potential source of endogeneity, I exploit spatial variations in the level of violence against Bosnians during the 1992/95 war to provide IV estimates of the cost of displacement on labor market outcomes.

As local violence is likely to serve as a good predictor for the displacement of Bosnians, it can be used as an instrumental variable to purge the estimates of the impact of displacement on labor market outcomes of potential inconsistency and shed some light on the selection process. The pattern of the Serb invasions in the 1992/95 BiH war was not governed by an economic purpose but by the will to create an ethnically homogenous and contiguous Serb territory (UN, 1994; UNHCR, 1998; Burg and Shoup,

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at a macroeconomic level using cross-country comparisons. Miguel et al. (2004) find a positive causal relation between economic under-performance and the likelihood of civil strife in Africa. Collier (2003) finds supportive evidence of a 'conflict trap', whereby low aggregate levels of physical as well as human capital correlate to the likelihood of conflict resurgence.

[6]Blattman's (2006) study uses arguably random abduction of children by the militia to measure the long-term costs of child soldiering for boys in Northern Uganda. Shemyakina (2006) finds significant and negative effects on girls though, interestingly, her results suggest no effect for boys.

[7]Municipalities are the fourth and last level of political division, below entities, districts, and cantons. There were 115 muncipalities in 1991 BiH, and some were divided after the war to form 137 municipalities. In 1991, the median population in the municipalities was 31577, and the mean was 44439, with a maximum of 195,139 and a minimum of 4,162.

1999; Nation, 2003; Toal and Dahlman, 2004). There is, therefore, no reason *a priori* to expect pre-war economic performance and the levels of violence endured in a given municipality to be systematically associated. A particularly attractive feature of the data used is that the municipality of residence before the war, or "municipality of origin", is recorded for all adults. Individuals can, therefore, be matched to their municipality of origin and can be assigned the corresponding level of violence using population loss data also available at the municipality level (RDCS, 2007). This allows me to exploit variations in the level of violence across municipalities of origin for identification. Since the assumption of zero correlation between economic performance and the level of violence at a local level is central to this analysis, I perform a number of robustness and falsification exercises to assess its accuracy. All results corroborate the notion that this is a reasonable assumption.

An advantage of including both those who returned to their municipality of origin and those who moved to a new municipality of destination in the sample of displaced is that I can include municipality of destination fixed effects in the model even when IV is used. Non-random sorting of individuals into their municipality of origin poses a potential threat to the consistency of the regression estimates. Similarly, failing to account for unobserved variations in economic performance across municipality is likely to result in biased estimates of the effect of displacement on labor market outcomes. Hence, in the absence of a credible instrument for the assignment of individuals to various municipalities of destination, conditioning on municipality of destination fixed-effects will capture the potential non-random location of individuals within BiH.

My results consistently corroborate the idea that displacement negatively impacted the labor market outcomes of Bosnian men and women particularly in terms of access to employment. Interestingly, the effect of lower employment on participation in the labor market varies across genders: whereas for men it entirely translates into higher unemployment, it implies lower participation for women once selection is accounted for. The results obtained when displacement is instrumented substantially exceed the one-step estimates, suggesting that Bosnian men and women positively selected into displacement, *i.e.* that the more "able" were more likely to migrate. I also test for assimilation of displaced men and women into the labor market over the 2001/04 period. Although year-by-year point estimates for men suggest that assimilation is taking place, I cannot reject that the effect of displacement is invariant across waves of the panel. In all gender groups, I find no additional effect of education on labor market outcomes for the displaced. I further run a number of robustness checks and find that my results cannot be attributed to disruption in schooling or to displacement along the new 1995 border. The informality of the labor market in BiH and the destruction of networks are plausible candidates to explain the high cost of displacement in terms of labor market outcomes and also help rationalise the lack of an effect on participation for

displaced men. However, differences in selection suggest that the experience of war was highly contrasted along gender lines and that sociological and cultural factors may also play a significant role.

The next subsection introduces some background on the war and the post-war reconstruction in BiH. In the second section I present the data and some descriptive evidence on the main variables of interest. The empirical strategy is detailed in the third section. The econometric results are presented in the fourth section and discussed in the fifth section. A sixth section briefly concludes.

1.1   Background on Bosnia and Herzegovina

BiH is a former territory of ex-Yugoslavia that became independent with the fall of the communist regime in 1992. The population of BiH in 1992 was 4.4 million people and was composed of 40 percent Bosnians, 37 percent Serbs, and 17 percent Croats. Most Bosnians are Muslim, whereas the Serbs are essentially Orthodox Christian and the Croats Catholic. Although I do not, to this date, have access to comprehensive pre-conflict socioeconomic household data, there is no evidence that pre-1992 economic prosperity differed systematically across ethnic lines.

Shortly after independence in 1992 a war broke out among the Serbs, the Bosnians and the Croats. The Bosnian-Croat war lasted from 1993 to 1994. The Bosnian-Serb and Croat-Serb conflicts lasted longer, but in 1994, NATO used its air force to bomb Serb strongholds and, in December 1995, the Dayton Peace Agreement ended four years of ethnic conflict in BiH. The agreement initiated the partitioning of the territory into 2 distinct entities: the Bosnian and Croat-led Federation of Bosnia Herzegovina (FBH) and the Serb-led Republika Srpska (RS). Each entity makes up for roughly a half of the total territory with the new borders essentially established on the 1995 front lines (Burg and Shoup, 1999; Toal and Dahlman, 2004).

The conflict and partitioning created widespread displacement. Reports by the International Criminal Tribunal for the former Yugoslavia (ICTY) estimate that 102,000 people were declared missing or dead[8], and the UNHCR reports that 1.3 million people were displaced either internally or abroad. Between 1996 and 2004, over 1 million of the displaced resettled in BiH with two peaks in 1996 and 2001.[9] The displaced were free to resettle anywhere in the territory; however, most displaced Bosnians were found to resettle in the FBH while most Serbs chose the RS.[10]

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[8] Most estimates did not previously take into account possible overlapping of the casualties data and had, therefore, a tendency to overestimate the number of deaths. The municipality level population losses data used in this study is in line with the ICTY estimate.

[9] As documented by Angrist and Kugler (2003), the Balkan wars resulted in an important wave of immigration to western Europe. This is of concern, as the displaced who resettled in BiH might consist of a selected sample. As a check, I compare the education attainment of the displaced in our sample to that reported by Angrist and Kugler. I find that 31 percent of our sample of men is in the equivalent of their low education group (ISCED levels 0-2) against 39 in their sample. In later results, I show that education has no additional effect on displaced people's labor market outcome relative to its effect on stayers'. Hence it is not clear that this selection creates any bias.

[10] As the displaced were not forced back into their pre-war residence, the issue arises of whether the decision to return to

Although the conflict caused massive infrastructure destruction, large amounts of aid were put into the reconstruction effort. Of the 1,295,000 housing units in the country (of which 80% were privately owned houses), 500,000 were subject to destruction either partial or total (UNHCR). The amount of damage and destruction as a fraction of the total stock of housing units by entity is as follows: 50% damaged and 6% of destroyed in the FBH against 24% damaged and 5% destroyed in the RS. In order to help the country rebuild and cope with the displaced returnees crisis, the Priority Reconstruction Program was put into place in 1995/99 by the UNHCR, the World Bank, and the European Union. The programme mainly aimed to provide help and/or materials to re-habilitate housing units. There is no evidence that any eligibility criterion was used and allocation was solely based on availability of material and aid. The UN found an occupancy rate of re-built houses of about 77 percent overall, including those who returned to abandoned re-built houses and not their own. It promoted the rebuilding of approximately 10-15 percent of houses per affected village in order to encourage non-programme households to return or relocate in areas which suffered badly from destruction, hence attenuating population density in relatively less blighted areas. The average expenditure on re-habilitating a house was between US$1,000 and US$3,000 according to the extent of the damage. An estimate of the total cost of damage to houses is put at US$ 4 billion (UNHCR), and the Priority Reconstruction Program channelled about US$ 693 million toward re-construction. This programme likely created some transitory job-rich economic growth. However, as its intensity decreased over time and stopped in 1999, well before the first wave of the panel was collected, this should not affect the estimates of the impact of displacement on time use.[11]

2 Data

This study exploits two main types of data sources: first, longitudinal household survey data, the 2001/04 Living in BiH (LBiH) panel; second, municipality level data on war casualties published by the Research and Documentation Centre in Sarajevo (RDCS, 2007) and the 1991 BiH population census.

The 2001/04 LBiH panel randomly sampled households in BiH and recorded information on demographics, labor activities, migration, education, and other characteristics on all individuals aged 15 and above in the selected households. The sampling scheme of the panel was stratified as follows. First, the

their original residence or not consists of an outcome variable *per se*. In 1999 the Commission for Real Property Claims of Displaced Persons and Refugees of the UNHCR carried out a survey of the return intentions among displaced persons and refugees still internally displaced and asked those willing to return to their homes (76 percent in the FBH and 34 percent in the RS) their main reason for wanting to return to their pre-war residence. The main self-reported reason was that "this was their home" (59 percent), whereas the second most quoted factor was that "their current housing situation was unacceptable" (proportion not reported). I further discuss the implications this has on this study's empirical methodology later as I describe the measure of displacement used in this study.

[11] All figures given above on the destruction during the war and the extent of the reconstruction programme were issued by the UNHCR (1998). For a thorough description of the pattern of returns to pre-war residence in post-war BiH, see Toal and Dahlman (2004).

137 municipalities of BiH were split into 6 strata (using a created Master Sample): by entity, and type (rural, urban, and "mixed"). Municipalities were independently sampled from each of the 6 strata. 25 municipalities were then selected with probability proportional to their estimated population size within each stratum (11 in the RS, 14 in the FBH).[12] From these municipalities, 5400 households were interviewed between September and November in 2001, and half were then re-interviewed each year from 2002 to 2004 to form a panel. In the end, the panel samples about 4800 individuals from all ethnic backgrounds, between 70 to 442 individuals in each municipality, aged 18-64 in each wave. A unique identifier allows us to follow individuals from the cross-section to the panel. The attrition across the waves of the panel is modest, on the order of 4% *per* wave, as households were followed when they moved geographically.[13] The panel is unbalanced, with individuals moving in and out of households, although households and individuals changing municipality across waves are followed. Since ethnicity was only ascertained from 2002 onward, I drop those individuals who only appear in 2001 and the sample used in this study is therefore only composed of those households followed into the panel.

The municipality-level RDCS population losses data (which is in line with the ICTY total estimate of the casualties) and the 1991 census are used to build the proportion of the pre-war population that went missing in each municipality as a result of the conflict. The number of casualties are recorded by municipality of suffering as opposed to municipality of origin. This offers a measure of the severity of the conflict at the local level, which is used to provide an exclusion restriction in subsequent instrumental variable estimation. The variations in the level of casualties across municipalities are mapped in Figure 2.

2.1 A Measure of Displacement

The UNHCR estimates that the 1997 to 2001 period saw large numbers of displaced people (internally displaced and refugees alike) resettling in the territory of BiH. Figure 3 illustrates the patterns of migrations for Bosnians by year of arrival in the municipality of destination as recorded in the 2001/04 LBiH panel. It is clear that migrations intensified in 1992-2001. I define individual displacement status using information on the last migration recorded at the individual level for all individuals aged 15 and above in the LBiH panel data. The year the individual left her municipality of origin is not observed in the data; however, the date the individual moved in her current residence ("year moved into current place of

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[12]The population of a municipality is subject to large variations across municipalities: it ranges from approximately 5000 to 200,000 individuals. The municipalities included in the sample are the following: Visoko, Gradačac, Kakanj, Zavidovići, Vogošća, Breza, Posušje, Grude, Novi Grad, Modriča, Srbac, Šamac, Višegrad, Srpska Ilidža, Kneževo, Čajniče, Travnik, Novi Grad, Tuzla, Novo Sarajevo, Prijedor, Banja Luka, Zvornik, Centar, and Zenica. Figure 1 shows a map of BiH and its municipalites.

[13]As individuals are followed when they change residence across waves of the panel, the number of sampled municipalities increases from the original 25 in the 2001 LSMS to 44 in 2004 (21 in the RS and 33 in the FBH).

residence") is recorded and I use it to define whether the migration occurred in the context of the 1992-95 war. Consistent with the evidence discussed above, any resettlement that occurred between 1992 and 2001 is categorized as conflict-induced displacement. Robustness checks on the definition of war-related displacement by narrowing the definition down to the 1992-96 period indicate that parameter constancy cannot be rejected across definitions (Kondylis, 2007b). Further checks on this definition are presented in the Appendix.

A remarkable feature of the LBiH panel is that the municipality of residence before the war (or municipality of origin) is recorded for all individuals. This allows me to attribute the level of local violence experienced in the pre-war municipality of residence (municipality of origin) to each individual regardless of her displacement status, using the casualties data from the RDCS as a proxy for local violence.[14]

There are four main limitations in measuring the effect of displacement using the LBiH. First, 'refugees' (displaced persons who went into exile abroad during the time of the conflict) and 'internally displaced' (persons who resettled in camps within BiH during the conflict) cannot be separately identified. Indeed, no information was recorded on the place of residence in the period between the time a displaced person left her municipality of origin and the time she resettled in her municipality of destination. Second, the definition of displacement only includes those who declared that they were living in BiH just before the war.[15] Third, only the date of the last resettlement is recorded. Consequently, the duration of the initial displacement is not observed in the data and the period spent in camps cannot be used as a source of variation in the treatment. Finally, the definition of displacement used in this study does not take into account the individual decision to return to the municipality of origin or to resettle in a new municipality of destination. Indeed, the decision to return or not to the same municipality constitutes an outcome variable in itself. Therefore, consistent estimates of the effect of displacement cannot be obtained on the selected sample of those displaced who returned to the municipality of origin. This study compares displaced persons' labor market outcomes to stayers' averaging across the displaced who moved to a new municipality and those who returned to their municipality of origin.[16]

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[14] Although I do not condition the displacement status on self-reported outcomes, I further test the relevance of the definition of displacement using self-reported individual information from the panel: the main reason for the last migration and the individual "status" in her current residence. More details on those checks are provided in the data appendix.

[15] Only 66 people (1% of our total sample) declared living abroad when the war started. Those individuals are likely to be quite different to those living in BiH before the war and therefore not included in the treatment group.

[16] Conditioning the effect of displacement on the decision of displaced individuals to return to their residence of origin or to settle in a new location would most likely yield inconsistent estimates. Indeed, this decision is an outcome in and of itself. As a check, I reduce the sample to individuals who have settled in a new municipality (the sample of returnees being too small to provide sufficient statistical power). I find that parameter constancy can never be rejected across samples (Results not reported). However, since the direction of the induced selection bias is unobvious, that does not provide a clean check of whether or not the effect of displacement is attributable to the decision to return to the same municipality or to a new one.

2.2 Descriptive Evidence

2001 BiH was populated by about 3.9 million people, 48.3% Bosnian, 34% Serb, 15.4% Croat, and 2.3% from other ethnic backgrounds. The gross national income was very low, about US$1,290 *per capita*.[17] The economy is highly informal. The World Bank estimates that the share of informal employment was about 37% in 2001 and 42% in 2004 (respectively 33 and 36 percent in the FBH and 41 and 49 in the RS). 40% of those working in the informal sector are employed in agriculture, 19% in construction, 9% in manufacturing and 9% in trade (Bank, 2005a). The public sector represents a large share of total employment although this share is decreasing over time (from 52% in 2001 to 42% in 2004) due to the privatization of state-owned enterprises. Despite the rise in private sector employment between 2001 and 2004 (from 34 to 40%), the World Bank concludes that most of the job creation over the 2001-2004 period originated from the informal sector. The high level of informality on the labor market in BiH is likely to impact on the labor market outcomes of the displaced upon their resettlement within BiH. However, the only measure of informality one could extract from the LBiH data would only be partial.[18]

Tables 3 and 4 describe labor market status by gender, age group, ethnicity and region as reported in the LBiH panel. In both tables, the reported sample means are averaged over all four waves of the panel. The analysis is restricted to men and women aged 18-64. Labor market outcomes for men by entity is reported in the lower part of Table 3.

Looking at disparities in labor market outcomes across displaced and stayers using simple descriptive analysis gives a sense of the labor market cost of displacement (lower panels, Tables 3 and 4). Bosnian men (Table 3) are found to be significantly less likely to be in work by 14 percentage points relative to stayers. Nevertheless, these unconditional sample means indicate that this worklessness does not translate into higher inactivity but into higher unemployment for the displaced relative to the stayers. The descriptive evidence on displaced women's occupational status is very similar to that for men (Table 4), as displaced women are less likely to be in work by about 7 percentage points relative to stayers and more likely, by a similar amount, to be in unemployment.

Finally, Table 5 reports descriptive statistics of educational attainment by gender, age, and displacement status. Differences in education across displacement status are mostly insignificant and, where significant, small suggesting that, if anything, displaced individuals are less educated than their stayer counterparts. Education is controlled for in all subsequent regressions.

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[17] World Development Indicators, World Bank (2000).

[18] Indeed, the labor part of the individual questionnaire does not allow for an evaluation of the informality of the work occupied. The only information record of informality surveyed was the "non-agricultural owned businesses" part of the cross-sectional 2001 LSMS and only refers to those households who actually own a business. In addition, the informality of the non-owner employees is not available.

3 Empirical strategy

This section outlines the identification strategy employed to measure the effect of displacement on labor market outcomes and offers some tests and discussion on the main identifying assumptions.

3.1 Individual Heterogeneity and Displacement

Measuring the effect of conflict-induced displacement on labor market outcomes at the individual level presents the challenge that displacement status is likely correlated, through self-selection into displacement, to the unobserved ability component. The main regression model estimated in this study is

$$y_{it} = \beta_1' X_{it} + D_i \gamma_1 + \eta_i + \omega_{it}, \tag{1}$$

where y_{it} is the outcome of interest (labor market outcomes) for individual i in year t, X_{it} a vector of exogenous individual characteristics, D_i is a dummy for individual (time invariant) displacement status, and η_i is the unobserved individual heterogeneity. I assume that displacement status is a function of personal characteristics Z_i, the incidence of conflict in the location of origin C_o, economic conditions in the municipality of origin relative to "the rest of the world" p_o, a component of unobserved individual heterogeneity ν_i and a random shock u. Subscripts i and o respectively designate individual specific characteristics and municipality of origin characteristics. I denote the discrete choice function associated to displacement status as

$$D_i = F(Z_i, C_o, p_o, \nu_i, u) = \mathbb{1}\{E[U_D(Z_i, C_o, p_o, \nu_i, u) - U_S(Z_i, C_o, p_o, \nu_i, u)] > 0\}. \tag{2}$$

Individuals base their displacement decision on the expected net variation in utility (noting as $U_D(\cdot)$ and $U_S(\cdot)$ the respective utility functions of a displaced and of a stayer). The comparative static on $F(\cdot)$ in the various parameters is *a priori* ambiguous except in relative local economic performance p. Intuitively, $\partial(U_D - U_S)/\partial p < 0$ since good local economic conditions are likely to act as a "pull" factor, all else equal.

It seems plausible that unobserved parameters p and ν partly determine post-conflict labor market outcomes and, therefore, estimating (1) using OLS will likely produce inconsistent estimates of the effect of displacement. This source of endogeneity cannot be addressed by exploiting the longitudinal nature of the LBiH dataset for two main reasons. First, displacement is, by design, a time invariant variable and therefore its impact on labor market outcomes cannot be computed using fixed-effect estimates. Second, using the random component model would require that the component of unobserved heterogeneity η_i be uncorrelated to all parameters in (1). As this assumption is likely to be violated, using random-effect

might produce biased estimates of the effect of displacement. The displacement decision as estimated in a linear probability model is

$$D_i = \beta_2' Z_i + C_o \delta + p_o \varphi + \nu_i + u. \tag{3}$$

Suppose that a is the coefficient obtained when estimating a linear regression of ν_i on η_i (such that $\nu_i = \eta_i a + \epsilon$), the bias in the unconditional OLS estimate of γ_1 in (1) can be written as

$$p\lim \gamma_{1OLS} = \gamma_1 + a(\sigma^2_{\mu_i}/\sigma^2_{D_i}).$$

Assuming that displacement has a negative effect on work and that higher ability is positively correlated to a higher propensity to be displaced ($a > 0$), then using simple OLS to estimate (1) results, in absolute terms, in a downward biased estimate of the effect of displacement. Conversely, if ability is negatively correlated to the propensity to choose displacement, then the OLS estimates produce (in absolute terms) an upwardly biased estimate of the negative effect of displacement on work.

3.2 Displacement and Violence across Municipalities

A way to address the problem of endogeneity when estimating the effect of displacement on labor market outcomes is to use instrumental variable estimation. From (2), an obvious candidate to serve as instrumental variable is the severity of the conflict in the location of origin C_o. Casualties are used in this study as they are likely to constitute a good proxy for the level of violence against Bosnians and are attractively available in the RDCS dataset at the disaggregate level of the municipality.

For C_o to be a valid exclusion restriction, it needs to be exogenous to individual labor market outcomes. In the next sub-sections, I discuss and offer robustness checks on two main sources of concern: first, that pre-war local economic performance might have affected local conflict incidence; and second, that casualties might be correlated to post-war individual heterogeneity which would affect labor market performance.

3.2.1 Level of casualties and pre-war economic performance

Assume that the level of casualties is a linear function of local economic performance p_o plus an error term ($C_o = cp_o + \epsilon$) and that economic performance in the municipality of origin is (positively) correlated to post-war labor market outcomes through the unobserved heterogeneity component η_i: ($\eta_i = bp_o + u$, where $b > 0$ and u is an error term). The probability limit of the IV estimator of the unconditional effect of displacement using C_o as an instrument for displacement is

$$p\lim \gamma_{1IV} = \gamma_1[1 + b/(\varphi + \delta c)].$$

The sign of the bias is *a priori* ambiguous and depends on the values taken by φ and c. Notice that the first stage equation is now

$$D_i = \beta'_2 Z_i + C_o(\delta + \varphi/c) + u.$$

Assuming that, in line with the idea that a higher level of violence resulted in higher displacement, $(\delta + \varphi/c) > 0$, then, if $c > 0$ and violence is positively correlated to pre-war local economic performance, the IV estimates of the effect of displacement on work will be upward biased. Conversely, if $c < 0$ and violence is negatively correlated to economic performance, the IV will produce a downward biased estimate of the true coefficient γ_1.

Historical evidence suggests, however, that casualties were not determined by economic performance at the local level, as the primary aims of Serb attacks was territorial separation and ethnic cleansing of their acquired territories. The evolution of the front lines between 1992-95 clearly validates the notion that the violence followed a geo-strategic course from the Serbian border to the West of the country (Burg and Shoup, 1999). Similarly, a UN report on the pattern of Serb attacks confirms this idea (UN, 1994, as cited in Bulutgil, 2004), :

> The method by which the campaign of "ethnic cleansing" was carried out ensured that, comparatively, the most brutal and inhumane treatment of those detained occurred within the geographic arc following the Sava and the Drina Rivers of the former Yugoslavia. [...] For it is within this region of Bosnia Herzegovina that the Serbs required absolute control in order to establish a separate nation with contiguous borders and an uncompromised geographic link with Serbia and Montenegro. That control required the subjugation, if not the disappearance, of the non-Serb populations of the area.

Bulutgil's (2004) attempt to formally document this claim using data on ethnicity from the 1991 census and other sources of data on strategic position (distance to Serbian border, elevation, the geography of Serb attacks and areas of control) corroborates this notion.

The economic part of the 1991 Census is not available as of now and thus cannot be used to verify the assumption of no link between local pre-war economic performance and war casualties. Using pre-war educational attainment and ethnicity as proxies for local economic performance, I carry out two robustness checks on the validity of this assumption.

Local Educational Attainment and Casualties   I now test whether pre-war educational attainment can predict casualties. Since educational attainment is likely a good proxy for local prosperity, this provides a more direct check on the assumption that pre-war local economic performance does not predict war casualties.

To this effect, I regress the level of violence in the pre-war municipality of residence, as proxied by the proportion of casualties from the RDCS (*Casualties*), on educational attainment at the individual level for those aged 27 and above in 2001. I also use a set of dummies to indicate the local severity of the conflict: namely, the distribution of the proportion of casualties over the total population in the municipality of origin is split into 5 groups (0-20th, 20-40th, 40-60th, 60-80th, and 80-100th percentiles as compared to national levels of casualties).[19] I use two dummies for educational attainment: one for medium education (complete or incomplete high school, *Educ_Medium*) and one for high education (above complete high school, *Educ_High*) measured relative to low education (primary school completed and below). Some other individual characteristics (X) are also included.[20] In formulas,

$$Casualties = \alpha + Educ\_Medium\beta_1 + Educ\_High\beta_2 + \gamma'X + \epsilon.$$

The results reported in Table 9 reinforce the historically documented idea that the intensity of the conflict was determined more by geo-strategic motives rather than economic motives.[21] The coefficients on the two education dummies are significant in most specifications (cols. 4-6). For each specification, the coefficients on medium and high education bear the same sign, which tends to suggest that those specifications are picking up genuine regional variations in wealth. However, there is no linear effect (col. 1) and the signs vary across the range of the distribution of violence (cols. 2-6), indicating that the relationship between wealth and conflict incidence is not monotonic across ranges of the distribution of violence.

Ethnicity and Casualties Finally, I check whether pre-war ethnicity explains a large amount of the cross-municipality variations in war casualties. This is of concern insofar as ethnicity might have determined individual economic status through ethnic discrimination. I use ethnicity data from the 1991 BiH Census to estimate the effect of the proportion of Bosnians on casualties at the municipality level. I divide the total number of casualties by the number of Bosnians in order to correct for the fact that Bosnians were more likely to be killed than Serbs. I refer to this measure as the number of casualties per Bosnian. Figure 4 shows a municipality-level scatter plot of casualties *per* Bosnian and pre-war proportion of Bosnians as well as the results from a linear regression of this index of casualties on proportion of Bosnians at the municipality level. The estimated regression coefficient is 0.084 and

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[19] For instance, the "Violence 0-20 percentile" dummy takes value 1 if the individual's pre-war municipality experienced a level of casualties in the first 20 percentiles of the distribution and zero otherwise.

[20] The inclusion of controls is somewhat problematic since the observable characteristics are from the 2001/04 panel only and might differ from those held by individuals at the onset of the war. However, the inclusion of those characteristics do not affect the results. The controls are similar to those included in later regression analysis, to the exception of war disability.

[21] I also run the same set of regressions splitting the sample along gender lines and find that the coefficients are not affected (results not reported for brevity).

is significant at conventional levels. However, the associated R-squared is low at 0.135, suggesting that ethnicity only explains a small portion of cross-municipality variations in casualties. Moreover, the scatter plot corroborates this notion, exhibiting no distinct trend. Indeed, for a given proportion of Bosnians, the graph shows large variations in the experienced levels of casualties, suggesting no systematic relationship between war casualties and the pre-war ethnic make-up of a municipality. This reinforces the notion that war casualties are not systematically correlated to pre-war economic performance and therefore not associated to post-war labor market outcomes.

3.2.2   Individual Heterogeneity and Casualties

Another source of concern is that casualties in the municipality of origin might be correlated to post-war individual heterogeneity and, therefore, to labor market outcomes. Should higher level of casualties have caused individuals to be more likely to hold characteristics that worsened their labor market performance, then using IV-2SLS to estimate the effect of displacement on work would cause the coefficients to be downward biased.

One important concern is that higher casualties might be correlated to poorer health status, both among stayers and displaced. Since physical health is well document in the LBiH panel, I perform two robustness checks, using war disability and chronic disease status.

Disability status and year of suffering were recorded at wave 2 of the LBiH panel. As a check, I regress a binary variable indicating having suffered from a disability during the 1992/95 period on casualties in the municipality of origin.[22] The results are reported in Panel A of Table 6. Columns (1)-(3) present the results for men, and columns (4)-(6) the results for women. The sample is restricted to wave 2 of the LBiH panel. Columns (1) and (4) shows results from the unconditional specification, while coefficients conditional on individual characteristics and municipality of destination fixed-effects are reported in columns (2) and (5), and (3) and (6) respectively. The standard errors are clustered at the level of the municipality of origin. The results suggest no significant effect of the magnitude of the violence in the municipality of origin on the probability to have suffered from a war-time disability.

As a second and somewhat complementary check, I explore the relationship between suffering from a chronic disease and casualties in the municipality of origin.[23] Current chronic disease status was surveyed in the health section of the LBiH questionnaire. I regress a dummy for declaring suffering

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[22]The type of disability was also self-reported. One of the categories the respondents could choose was "war wounded". I find that over 50 percent of the individuals who became disabled during the war report having been war wounded.

[23]It is important to note that, to the extent that chronic disease status is likely to be correlated to economic welfare over the post-war period, checking for correlation between chronic disease and casualties does not provide as powerful a test as when war disability is used. I present it in more as a complement to war disability, since not all physical damages from the war resulted in disabilities. Similarly, mental health (recorded in the survey) is also likely to be correlated to casualties. However, current mental health status is even more likely to be correlated to current economic status than physical health and hence is even more questionable than using physical health.

from a chronic disease on casualties in the municipality of origin and the same controls as used above, including municipality of destination fixed-effects in some regressions. The results are presented in Panel B of Table 6 using the same layout as above.[24] The estimates corroborate the idea that casualties in the municipality of origin is not systematically correlated to physical health status, therefore reinforcing the notion that using casualties as an IV provides consistent estimates.

These results are consistent with the notion that there is no straight link between casualties and physical health. I control for war-time disability in all subsequent labor market specifications. However, since there is no record of health in 1995, and as current health status (physical or, to an even greater extent, mental alike) is likely endogenous to current labor market performance, I do not control for current chronic disease status.

3.3 First-Stage Estimates

In this subsection I present the first-stage estimates, regressing a dummy for displacement status on the level of casualties in the municipality of origin. In formulas,

$$D_i = \beta'_2 X + Casualties_o \delta + \epsilon, \tag{4}$$

where D_i is a binary variable indicating individual displacement status, $Casualties$ is the proportion of casualties in the municipality of origin from the RDCS data, and X some observed individual characteristics.[25] Table 7 presents the results of the first stage estimation.[26]

The instrument is highly significant in all specifications for both men and women. Adding further individual controls in columns (2) and (5) does not affect the point estimates and standard errors of the coefficient on casualties. This corroborates the idea that the effect of casualties on the individual displacement decision was not influenced by observable characteristics. Moreover, the unconditional specification carries adjusted R-squared and F-statistics of 0.18 and 240.40 for men, and 0.15 and 202.91 for women. Further adding controls for individual characteristics only has a moderate effect on the

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[24] I report the results of pooled OLS on 2001/04 panel. The results are not affected when I restrict the panel to one year only.

[25] Controls include a time trend, dummies for household status, dummies for marital status, individual age-group dummies (four years by four years from 23 to 62 and one for 63-64), household composition variables (number of dependent members aged 0, 1-5, 6-10, 11-15, and 16-17), and dummies for educational attainment (medium and high, relative to low), a dummy for suffering from a war-time disability.

[26] As the treatment variable is binary, the interpretation of the coefficient on displacement is not directly obvious. As exposed by Manning (2004), in order for the estimation to capture the ATE, the predicted values of the treatment conditional on the instruments need to be concentrated around 0 and 1. The support of the first-stage predictions (Figure not reported, see Kondylis, 2007b) suggests that it is the case. Moreover, in the presence of heterogenous treatment effects, the relationship between the mean of the dependent, conditional on the instrument, and the treatment variable, also conditioned on the instrument, is by construction non-linear. In the absence of an adequate functional form to model this non-linearity, stringent distributional assumptions would have to be made on the treatment variable to retrieve an instrument-independent measure of the treatment on the treated and of the ATE. Hence, the identification strategy used in this study captures an instrument-dependent measure of the ATE which has internal validity.

adjusted R-squared, which strengthen the notion previously discussed that selection into displacement did not overwhelmingly operate along observable characteristics. Overall, these results confirm that the intensity of the conflict as measured by casualties is a good predictor of the individual propensity to migrate.

3.3.1 Falsification Exercise on the First-Stage Estimates

I now propose a falsification exercise on the first-stage. Should 1992-95 casualties explain pre-war migrations, then this would suggest that my instrument is picking up economic disparities across municipalities rather than the real effect of violence as a "push" factor in the displacement decision. I use the fact that municipality of birth and last migration, whether in the 1992-2001 period or earlier, are recorded for all individuals in the data. I use the group of pre-1992 migrants to check whether the level of casualties in their municipality of birth (assuming this was their municipality of origin) can predict pre-war migration patterns.

All individuals who report having ever moved, including displaced persons whose pre-war municipality is not the same as their municipality of birth, are included into the group of non-war migrants. The proportion of non-war migrants thus obtained is 23 percent of the whole sample. The municipality of destination is, in this framework, set to be the pre-war municipality for the groups of non-war migrants, war displaced and stayers alike. The control group now consists of the stayers and the displaced who never moved before being displaced.[27] In formulas

$$M_i = \beta'_3 X + Casualties_B \delta_2 + \epsilon',$$

where M_i is a dummy for non-conflict migration and $Casualties_B$ is the level of casualties in the municipality of origin, in this case the municipality of birth. The results are reported in Table 8. The regression specifications are identical to those reported in Table 7. The R-squared associated to the unconditional specifications (columns 1 and 4) is 0 and the effect of casualties is negative, small and insignificant in all regressions, except in columns (3) and (6) when municipality of destination fixed-effects are included. Indeed, the coefficients are then significant but virtually equal to zero, suggesting an extremely rigid pattern of economic migrations over the territory. Overall, these results suggest that casualties in the municipality of birth cannot predict (economic) pre-war migrations and are, therefore, consistent with the idea that conflict incidence was not motivated by local pre-war economic performance.

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[27] I also run the same regression restricting the control group to stayers and obtain similar results.

3.4 Post-war local economic conditions

There are three concerns related to the spatial sorting of the population in post-war BiH: the self-selection of displaced people in their municipality of destination, the economic impact of inflows of displaced, and the impact of violence on post-war local economic performance. A displaced person is likely to self-select into her municipality of destination based on observed and unobserved characteristics, and the instrumental variable strategy proposed in this study does not address this issue. In the absence of a credible instrument, I include municipality of destination fixed-effects in all regressions to purge the estimates of this source of inconsistency. Since less than 50 percent of displaced individuals resettled in their municipality of origin, the coefficient on the effect of displacement using variations in violence across municipalities of origin in the first stage is still identified when municipality of destination fixed-effects are added in the second stage.

Inflows of displaced may also impact the economic performance of the municipality of destination. In this study, stayers are used as a control group to measure the effect of displacement on the displaced after resettlement in BiH. Although the impact of (net) inflows of displaced on the population of stayers is not directly measured, this effect is implicitly contained in the estimates of displacement proposed in this study.[28] As there are substantial variations in the inflow of displaced and returnees across municipalities, including municipality of destination fixed-effects should account for local disparities in the inflow of displaced persons, together with other local labor market conditions. The effect of high of migration inflows is ambiguous, as those inflows have both supply and demand effects that are likely to go in opposite directions. A large inflow of displaced people might have a negative impact on stayers' labor market outcomes by creating an over-supply of labor, which would lead the estimates to understate the (negative) effect of displacement in those regions more affected. Additionally, that some displaced persons return in higher number to their municipality of origin is likely to have a positive effect on their labor market outcomes by enhancing their ability to rely on her pre-war informal networks. There also are important effects on the demand side, as higher levels of in-migration also increase the demand for labor.

Finally, an additional source of concern is that violence during the war might determine the economic performance of a given municipality through the destruction of infrastructure and networks.[29] Since

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[28] Numerous studies document the effect of immigrant inflows on the natives' labor market outcomes. For instance, Card (1990) finds that the Mariel boat-lift had no impact on local wages and unemployment rate, even among the population of immigrants from earlier waves of migration, and argues that this is due to the capacity of Miami's labour market to adjust to this increase in labor supply. Dustmann *et al.* (2005) look at immigration to the UK and no overall significant effect on the natives. Conversely, Hunt (1992) finds that the massive waves of repatriation from Algeria into France after the independence war had a negative effect on the non-repatriates' labor market outcomes. Interestingly, she finds no evidence of a decrease in immigration to places where the proportion of repatriates were high. Angrist and Kugler (2003) look at the impact of immigration from the Balkans and into Europe on natives' labor market outcomes. Although they find no overall effect, their results suggest that the effect of immigration in more protective states is significantly negative. Dustmann *et al.* (2005) and Glitz (2006) also document a heterogenous effect across levels of educational attainment.

[29] Although I do not have access to records of local economic reconstruction *per se*, I use the fact that interviewers

casualties and economic performance are likely to be negatively correlated, this would cause the IV estimates of the effect of displacement to be downward biased.[30] Adding municipality of destination fixed-effects in subsequent regressions should absorb this source of inconsistency.

4 Results

4.1 Displacement and Labor Market Performance

In this sub-section, I turn to the central investigation of the effect of displacement on the labor market outcomes of the displaced as measured from the 2001/04 LBiH panel data. The sample is restricted to 18-64 year olds Bosnian and split along gender lines. A number of labor market outcome variables (work, unemployment and inactivity status, the natural log of hourly wage, and hours worked, including and excluding zeros) are regressed on a dummy for displacement status, controlling for a variety of observed individual and unobserved municipality characteristics, as used in the first-stage regression presented above. This amounts to estimating

$$y_{it} = \beta_1' X_{it} + D_i \gamma_1 + \mu_t + \omega_{it}, \tag{5}$$

where the error term contains a time-invariant individual heterogeneity component and in some specifications X_{it} includes municipality of destination fixed-effects. The pooled OLS estimates of the impact of displacement on work, unemployment, inactivity, wage, and hours over the 2001/04 panel are reported in columns (1)-(3) of Tables 10 and 11 for men and women respectively.[31] Column (1) presents the unconditional effect of displacement, while the estimates controlling for exogenous individual characteristics and municipality of destination fixed-effects are reported in columns (2) and (3) respectively. All standard-errors are clustered at the level of the municipality of origin.

The unconditional effect of displacement on work for Bosnian men is large, negative, and significant, implying a fall in the probability to be in work by 11.5 percentage points relative to stayers (Table 10, col. 1). Controlling for individual characteristics increases the point estimates (col. 2), and although

recorded housing conditions (good, incomplete, devastated, bad) for each household they visited to check for any systematic relationship between housing conditions and casualties in the municipality of destination at the household level (results not reported). I find that casualties have no explanatory power in all specifications except for the "incomplete" category. However, in that category the associated R-squared is below 0.1 percent. Moreover, only 1.81 and 1.61 percent of the total sample of Bosnian households have a house in "incomplete" and "devastated" conditions respectively, implying that the reconstruction effort discussed in section 1.1 was to a large extent successful.

[30] In the results section, I show that the effect of displacement on work is negative. As the bias in the OLS and this potential source of bias go in the same directions, the OLS and IV estimates hence offer "bounds" to the true value of the coefficient.

[31] In this version, I do not assume strict exogeneity between the controls and the error term and only report the results from the pooled OLS estimation. This also allows me to cluster the standard errors at the municipality of origin level despite the unbalanced nature of the panel. The results obtained using a random effects method of estimation did not significantly differ from the pooled OLS estimates.

the inclusion of municipality fixed-effects washes out part of the effect (now 0.075), the coefficient on displacement remains negative and significant (col. 3). It is quite important to note that, although the coefficient of interest varies sensibly across specifications, the standard errors on the parameter of interest are virtually unaffected by the inclusion of municipality fixed-effects. This suggests that, while accounting for unobserved local conditions, adding municipality of destination fixed-effects does not absorb much of the variations in the effect of displacement.

The effect of displacement on men's unemployment is large, positive, and significant in all specifications (cols. 1-3), associated to an increase in unemployment ranging from 9 to 15 percentage points. (Parameter constancy cannot be rejected across specifications.) These results suggest that, despite a fall in employment, displacement has no effect on Bosnian men's participation rate. I find no significant effect on wage and hours worked.[32]

The estimates of displacement on work for Bosnian women are qualitatively similar to those for Bosnian men, although of a lower magnitude, implying a fall in work on the order of 5 to 7 percentage points (Table 11). The effect on unemployment is only significantly positive in columns (1) and (2), as the inclusion of municipality fixed-effects indicates that the effect of displacement on unemployment for this group is largely attributable to local labor market characteristics (col. 3). As with the men, I find no across-the-board significant effect of displacement on wage and hours of work for Bosnian women.

2SLS Estimates

In order to purge the estimates of the effect of displacement on labor market outcomes from potential selection bias, I now re-estimate (5) using pooled IV-2SLS. Displacement is instrumented using the level of casualties in the municipality of origin (equation 4) as shown in Table 7. The results are shown in columns (4) to (6) of Tables 10 and 11 for men and women respectively. As above, the unconditional specification is presented in column (4) and individual characteristics and municipality of residence fixed-effects are added in columns (5) and (6) respectively.

Although mostly qualitatively in line with the OLS estimates of displacement, the IV estimates shed some light on the nature of the selection into displacement for Bosnian men. As the IV estimates of the effect of displacement on work and unemployment are in absolute terms systematically above those obtained with the one-step GLS, they imply a positive correlation between the latent effect of displacement and the probability of work. In other word, these results show that Bosnian men positively selected into displacement as the more "able" in terms of labor market performance were more likely to be displaced.

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[32] The effect on hours is only significant in the unconditional model when zeros are included, which suggests that the effect is entirely attributable to a difference in employment across groups and to observable characteristics and local labor market conditions.

Displacement is now associated to a fall in work by 16 to 29 percentage points and to an increase in unemployment by 19 to 22 percent across specifications (those variations are not significant). There still is no significant effect of displacement on Bosnian men's participation, wage, and hours.

In slight contrast, the IV estimates for Bosnian women indicate significant positive selection into displacement not only on work but also on inactivity. The IV-2SLS estimates indicate that the latent effect of displacement is, in the case of Bosnian women, not only positively correlated to a higher probability of being in work but also to higher participation in the labor market. Indeed, the estimates of the effect of displacement for Bosnian women now imply a larger fall in work on the order of 17-19 percentage points as well as a significant increase in inactivity by 11-18 percentage points, with no effect on unemployment.

Comparing the point estimates of the effect of conflict-induced displacement on labor market outcomes obtained across pooled OLS and IV-2SLS estimations suggests that the selection into displacement was of a positive nature. Given the absence of stark differences in levels of educational attainment across stayers and displaced (Table 5), a possible interpretation is that selection into displacement most likely operated along unobservable characteristics such as unobserved ability or access to network.

4.1.1 Reduced Form Estimates

In order to further assess the strength of the instrument, I check that the excluded instrument is statistically significant in the reduced form estimation. I regress casualties in the municipality of origin on labor market outcomes, adding individual characteristics and municipality of destination fixed-effects. The results are presented in Table 12; Panel A reports the results for men and Panel B for women. The results show that the statistical significance of casualties on various labor market outcomes is on par with that of the displacement variable in the IV specifications. This lends further support to the notion that casualties in the municipality offer a good source of identification.

4.2 Patterns of Assimilation

Figures 5 and 6 report patterns of assimilation of the displaced into the labor market over the 2001/04 panel for men and women respectively. For brevity, I only report the results on work. The coefficients and 95 percent confidence intervals reported are obtained by running the same IV-2SLS regression as reported in column 6 of Tables 10 and 11 (including controls and municipality of destination fixed-effects) separately for each year of the 2001/04 panel.[33]

The results for Bosnian men suggest that the magnitude of the negative effect of displacement on work is decreasing overtime, becoming insignificant in 2004. However, the differences in point estimates

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[33]By construction, there is no common support on years-since-return in the municipality of current residence across stayers and displaced. Indeed, only displaced have moved to their current residence between 1992 and 2001. Hence there is no suitable counterfactual to measure a yearly assimilation rate of the displaced into the labor market.

across years are not statistically significant (Figure 5).The pattern for displaced women's employment is contrasted with that of men (Figure 6). Whereas the negative effect of displacement on work is, in absolute terms, decreasing from 2001 to 2002, it then increases in 2003 and goes down again in 2004. However, the size of the standard errors does not allow to tell the coefficients apart across years.

In sum, although the point estimates across the years for men suggest complete assimilation of the displaced into the labor market over the 2001/04 period, the imprecision of the estimates does not allow for the non-assimilation hypothesis to be ruled out.

4.3 The Effect of Schooling

Tables 13 and 14 investigate the existence of an additional effect of education on the labor market outcomes of the displaced for men and women respectively. Empirically, a variety of labor market outcomes are regressed on a dummy for displacement, a dummy for having medium or high education (Med_High_Educ, relative to low), and the interaction between the displacement and education dummies.[34] The structure of Tables 13 and 14 is similar to that of Tables 10 and 11. For brevity the results for the unconditional specification are not reported. In formulas,

$$y_{it} = \beta' X_{it} + D_{it}.\gamma + Med\_High\_Educ.\delta_{11} + D_{it}D'_{it} * Med\_High\_Educ.\delta_{12} + u_{it}, \qquad (6)$$

where in some specifications X includes municipality fixed-effects. The standard-errors are clustered at the level of the municipality of origin.

The results show that education, while significantly affecting labor market outcomes in all specifications, has little or no additional effect for the displaced. The interaction between the dummy for education and the dummy for displaced status is only significant in the one-step work and unemployment estimations for men (Table 13, col. 1), although this is not robust to the inclusion of municipality fixed-effects (cols.2, 4). Similarly for women, the interaction of displacement with education is only significant for the full model on inactivity (col. 6), implying an increase in inactivity for educated women by about 12 percentage point relative to non-educated displaced women. However, this coefficient is only significant at the 10 percent level in all specifications.

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[34]In the absence of a credible instrument for education the estimates of the returns to education cannot be purged from the correlation between the latent effect of education and labour market outcomes (Card, 2001; Hausman and Taylor, 1981). Measuring those returns is not the main endeavor of the present regression analysis. This specification is estimated to test whether the displacement interaction bears any significance. Hence my estimates are likely to be flawed, either upwardly biased as a consequence of unobserved ability or downwardly biased resulting from a supply-side effect (Card, 2001).

4.4 Robustness Checks

4.4.1 Border Municipalities

The municipalities situated on the post-1995 Dayton border between the FBH and the RS might not be a representative sample before or after the war. Therefore, it is of concern that the population who originated from these municipalities could systematically differ from the rest of BiH or that the fighting in these regions was particularly intense relative to the rest of the country.[35] For instance, as the distance across entity is small along the border, displacement out of these municipality might be unrepresentative of the rest of BiH. Similarly, as these regions' might have suffered particularly strong infrastructure damages than the rest of the territory, migration out of those municipalities might have been particularly massive. These differences would affect the first-stage and results overall. As a check, I remove all individuals whose municipality of origin was situated on the Dayton border and who have moved as a result of the conflict.[36] The results are presented in Tables 15 and 16 for men and women respectively. Although the large reduction in sample sizes affects the precision of the estimates in some specifications, the point estimates are remarkably similar relative to the full sample (Tables 10 and 11) and parameter constancy can never be rejected at conventional levels.

4.4.2 Disruption in Schooling

Another source of concern is that the effect of the conflict might be heterogenous across age groups. One particular concern is that the negative effect of displacement measured in this study might be attributable to disruption in schooling and/or delayed entry on the labor market. As a check, I test whether removing the sub-sample of individuals who were of age 18 and below in 1992 substantially affects the estimates.[37] The results are presented in Tables 17 and 18. The results for Bosnian men and women are essentially similar across the full and restricted samples. These results suggest no marked disparities in the effect of displacement across age groups and that the negative effect of displacement measured on the full sample is not attributable to long term effects associated to disruption in schooling for the youngest age cohorts.

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[35] As documented by Burg and Shoup (1999), the Dayton borders were placed on the 1994 front lines.

[36] In reality, conditioning both on originating from and moving to a border municipality would constitute a better check. However, since the choice of the municipality of destination is an outcome variable in itself, it would lead to biased results.

[37] Another way to check for heterogeneity of the effect of displacement across age groups would be to interact dummies for age groups and displacement. However, the power of our instrument does not allow for the interactions to be precisely identified.

5 Discussion

5.1 The Nature of Conflict-Induced Displacement in BiH

The first main contribution of this study is to enhance the understanding of displacement in the context of conflict by formally documenting the nature of the selection into displacement. The IV estimates support the idea that displaced Bosnians positively self-selected into displacement, meaning that those who were "more able" in labor-market terms were more likely to decide to out-migrate. "More able" does not specifically refer to higher cognitive abilities but could also indicate better access to social networks outside the pre-war residence, which is likely to enhance post-displacement job prospects. This finding is rather intuitive since displacement is likely associated to a substantial loss of assets and having good post-displacement employment prospects would, by providing a form of insurance, increase the probability of displacement.

The results for women suggest that, whereas in the case of men selection only operated through the ability to find work, women's decision to go into displacement is also linked to participation in the labor market. Indeed, I find that the latent effect of displacement in the case of women is positively correlated to the probability to be in work but also negatively correlated to inactivity. Since women's labor market participation is likely tied not only to cognitive and networking abilities but also to unobserved cultural and sociological values, this suggests that those channels play an important role in determining a woman decision to migrate as a result of a conflict.

5.2 The Cost of Displacement

The second main contribution of this study is to measure the cost of displacement in terms of labor market outcomes for displaced Bosnians. The results imply a significant and large cost, as displaced Bosnians, men and women alike, are less likely to be in work by about 15 percentage points relative to stayers. There are two main contenders to explain the lower labor market performance of the displaced: first, the informality of the labor market in BiH combined to the loss of network access caused by displacement; and second, the assumption that the war resulted in worse employment disruption for displaced relative to stayers.

The informal nature of the labor market in BiH and its large share in total job creation are likely to have important implications on the nature and, therefore, the efficiency of the job-search across displacement status. As job offers in the informal sector are not openly advertised, the main implication is that job-seekers are more likely to find work within this sector by relying on their networks. For instance, the World Bank outlines the inadequacy of employment institutions as the main reason to explain the

inefficiencies of the labor market in BiH (Bank, 2002).[38]

Moreover, some evidence suggests that access to network plays a particularly important role for displaced individuals. Numerous studies in the literature on immigration suggest that networks are likely to play an important role in providing access to informal labor markets and credit in an informal economy. For instance, Edin *et al.* (2003) find that living in an enclave enhances the access to informal ethnic networks and improves immigrants' access to employment by improving the performance of refugee immigrants' job-search.[39] More specific to our study, a qualitative World Bank study of informality and vulnerability in post-conflict BiH (Bank, 1999) suggests that displaced persons have poorer access to social networks. Although I cannot test this assumption formally, assuming that displaced individuals have poorer access to informal networks relative to stayers, the informal nature of the job-search in an BiH is likely to further decrease their probability to find employment relative to stayers. This is somewhat corroborated by a simple descriptive analysis of the types of jobs held by working displaced relative to stayers. Table 19 describes, by displacement status, the sectors of occupation (primary, secondary, and tertiary) and the types of jobs (self-employment, paid employment, and family help) for those who declared working in the LBiH panel. Whereas displaced and stayers are equally represented in various sectors, displaced individuals are more likely to be self-employed and less likely to be in paid employment relative to stayers. Although the inference cannot be drawn from this descriptive evidence, it shows two things. First, it reinforces the notion that displaced individuals have similar skills to those of stayers. Second, it lends some support to the idea that the displaced have poorer access to informal networks and, therefore, are less likely to be in paid employment.

A second plausible explanation of displaced individuals' higher worklessness is that employment interruption was greater for those who were displaced. When a person is displaced, she might be more likely to become workless and to remain so during her exile. Some studies have suggested that employers might implement a "ranking" system, whereby those unemployed for longer period of times are, all else equal, less likely to be hired (Diamond and Blanchard, 1994). As the duration of the exile is not observed, this assumption cannot be formally tested. Moreover, it is not *a priori* obvious that the 1992/95 war was a less disruptive experience for displaced than for stayers in terms of employment disruption.

That the additional amount of men's worklessness attributable to displacement entirely translates into higher unemployment may also be explained by the high informality of the labor market in BiH

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[38] Despite the existence of Employment Institutions at the canton level, responsible for job counseling and job brokerage in BiH, the Bank reports that those structures are not used by either side of the market. Indeed, and although the direction of the causality is unclear, the Bank reports that formal job-seekers do not rely on their local EI to find work, and neither do formal businesses use these structures to post their openings. Moreover, qualitative evidence on the unemployed in BiH indicates that job-seekers rely on their informal networks as their main source of job offers.

[39] Similarly, Damm (2006) finds evidence of negative self-selection into segregated neighbourhoods together with positive returns to living in 'ghettos'. The literature on migration network also isolates some positive effect of having more developed migration networks in the place on migration on immigrants' labor market outcomes (Bauer *et al.*, 2000).

combined with displaced individuals' poorer access to networks. As Wahba and Zenou (2005) document in Egypt, a well-connected workless person is more likely to engage in "hidden" or "passive" unemployment, *i.e.* to rely on her employed relations to inform her of job opportunities and, thus, less likely to be categorised as unemployed according to the ILO definition. Conversely, a non-connected person cannot rely on a "passive" job-search and is more likely to be categorised as unemployed. Hence, that the amount of men's worklessness attributable to displacement systematically solely translates into higher unemployment might, in that sense, reflect the different natures of the job search across displacement status. Similarly, such mechanisms would help explain the high levels of inactivity among men (on the order of 20 percent). I cannot however rule out that this results from the fact that displaced Bosnian men, having lost relatively more assets during the war than their stayer counterparts, cannot "afford" idleness.

In contrast to men, the amount of women's worklessness attributable to displacement translates into higher inactivity with no effect on unemployment (once selection into displacement is accounted for). This might result from intra-household allocations that lead male members of the household to engage in labor market participation while (workless) female members devote themselves to household production, thus appearing as inactive under the ILO definition.[40] Hence that does not necessarily imply that they are not part of an active search, since male members might include them in their job-search.

6   Conclusion

The last 40 years have seen an increase in the incidence and the average length of civil conflicts (Azam *et al.*, 2001). As the positive relation between conflict resurgence and poverty is well established, promoting sustainable economic growth in a post-conflict context is a compelling priority. Since displacement is associated to high economic vulnerability, studying the access to the labor market for the displaced has particular policy relevance.

The central contribution of this study to the literature on the economics of civil conflict is twofold. First, it formally documents that the selection of Bosnian men and women into displacement during the 1992/95 BiH war was of a positive nature, as more "able" individuals in terms of labor market performance were more likely to be displaced. Second, it consistently estimates the causal effect of displacement on the post-war labor market performance of the displaced. My results suggest an increase in the probability of worklessness by over 15 percentage points for men and women alike. Overall, the magnitude of the negative effect of displacement on work, measured 6 years after the end of the 1992/95

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[40]Displacement and the lack of formal property rights are also likely to be associated. Field (2002) suggests that in the absence of property rights a household's labour supply decreases. As mentioned earlier, the presented specifications cannot provide consistent estimates when housing conditions are controlled for.

war, raises particular concern for the economic vulnerability of displaced persons in BiH.

A   Data Appendix: Constructed Variables

Conflict induced displacement

Two self-reported outcomes are used to further check for coherence in the definition of displacement: "what was the main reason for moving?" with the categories: war, property occupied, property devastated, security, no adequate living conditions, family reasons, job, health, other, and "what is your status in your current place of residence?" with the categories: permanent residence-with no moving during the war, permanent residence: displaced person/returnee, permanent residence: refugee/returnee, temporary residence: displaced person, temporary residence: refugee/displaced person, temporary residence: refugee, temporary residence: other. The suggested categories are rather wide, and using those self-reported outcomes to define displacement is not desirable. As a robustness check I exclude from the group of displaced those who declared having moved due to family or economic reasons as well as those who reported that their current status in their residence was "permanent, with no move during the war". I find this does not affect the results.

Labor Market Outcomes

The definition of "work" used in this study corresponds to those having reported doing any work during the survey week. Unemployment status is defined using the ILO definition[41], *i.e.* those having taken any step towards finding a job in the last month as opposed to referring to those registered within the unemployment bureau.[42] The inactive are those neither in work, unemployed, nor attending school. I use reported usual weekly hours to measure hours of work and exclude zeros in all subsequent computations. The hourly wage rate is built using reported usual hours of work and the last salary received for a given period of work. Log-linear regressions are reported for the wage effects.

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[41]In the data, among those who qualified as unemployed using the ILO definition, only 65.8% were registered with the unemployment bureau. Moreover, among those in work, 33.3% declared being registered as well, compared to only 13.7% of the inactive. The access to unemployment benefits (UB) in BiH is virtually inexistent. Indeed, only 0.22% of those registered with the unemployment bureau in the first wave (the only wave where UB were surveyed) reported benefiting from UB in the form of a cash transfer.

[42]Fares and Tiongson (2007) report a "routing" error in the data that prevents them from exploiting transitions from and into unemployment variable for 2003 wave of the panel. As we do not exploit job history but current occupation, this does not affect us. I check that our calculations are unchanged whether I include that year or not, and that the unemployment rate remains remarkably stable across waves.

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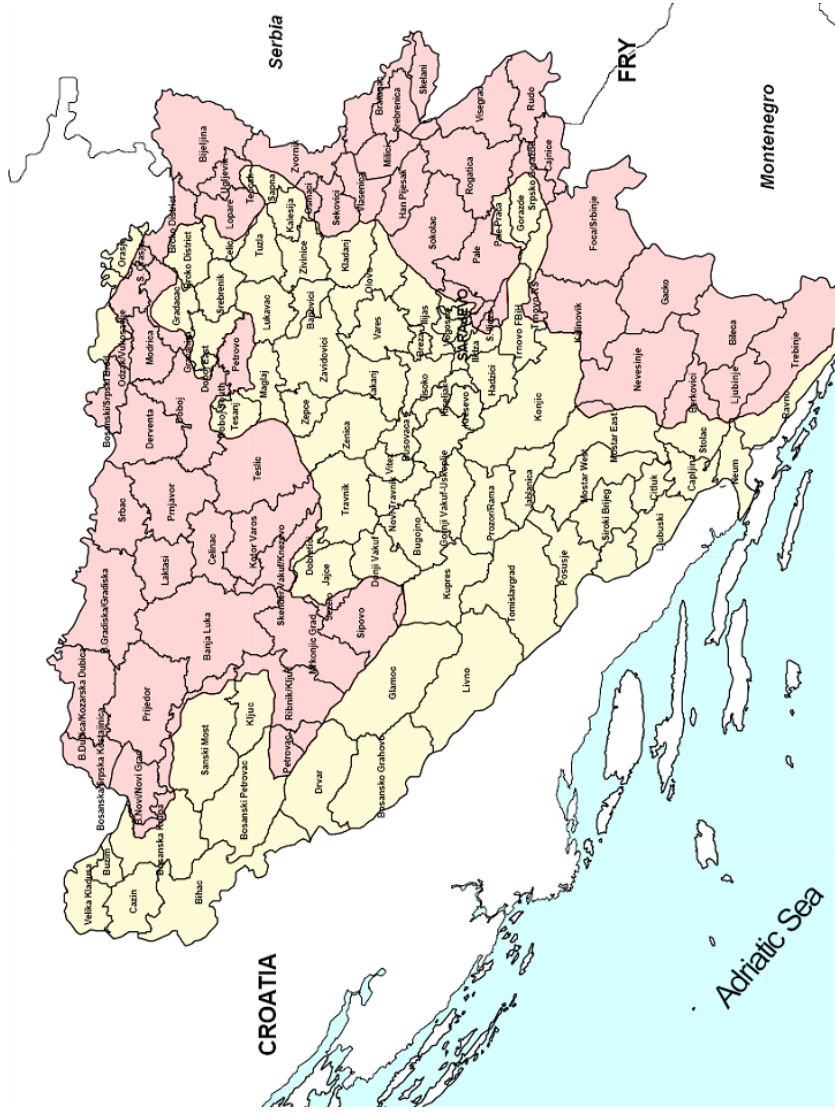
Figure 1: A Map of the municipalities of Bosnia and Herzegovina (Source: UNHCR for Bosnia and Herzegovina, 01/11/2003).

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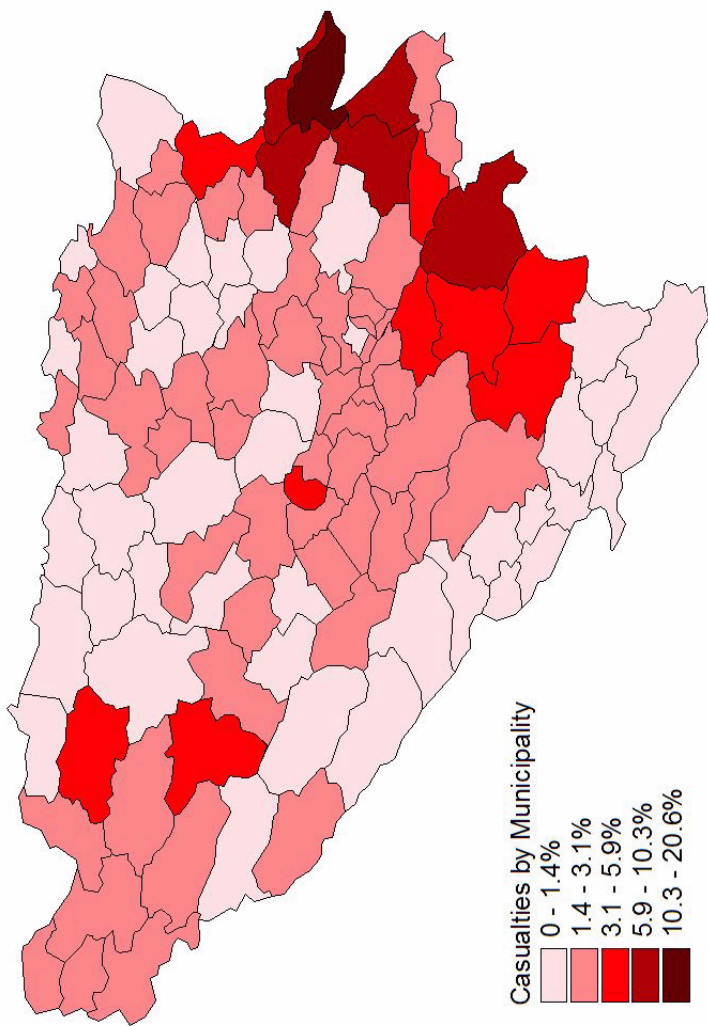
Figure 2: Proportion of war casualties by municipality. Each local proportion of casualties is computed as a proportion of the 1991 population. This proportion ranges from 0.1 to 20 percent.
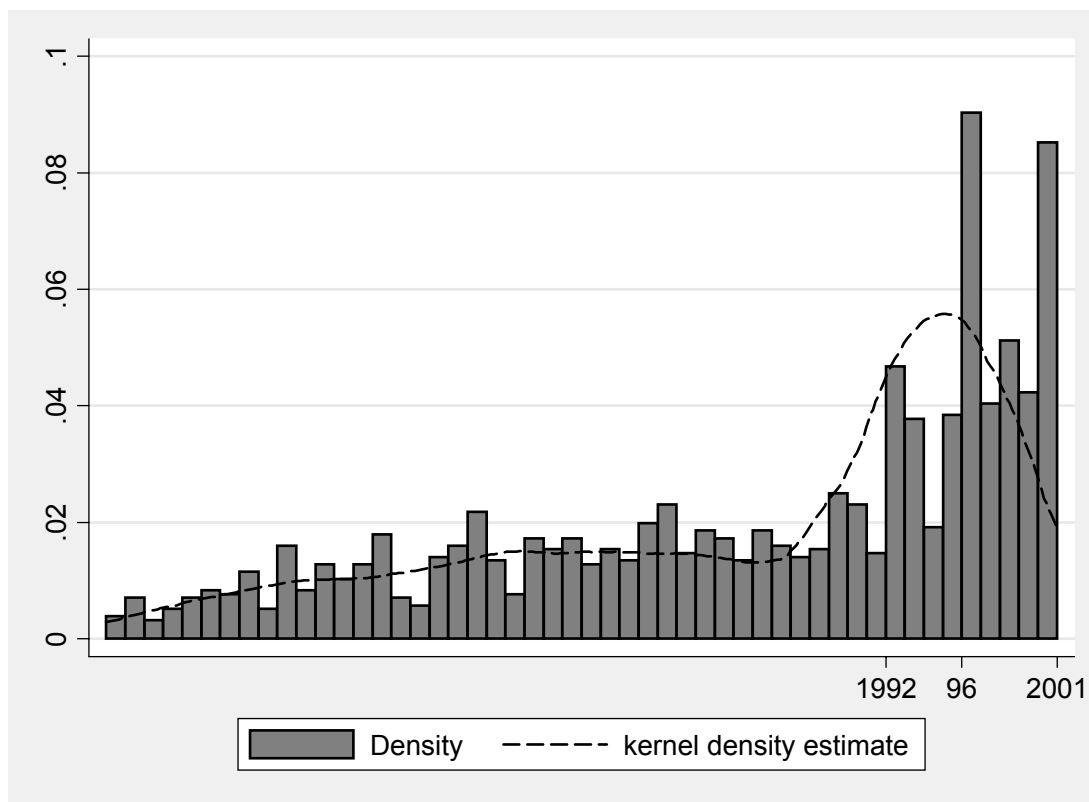
Figure 3: Histogram and kernel density estimates of the patterns of return migration over-time for Bosnians in BiH.
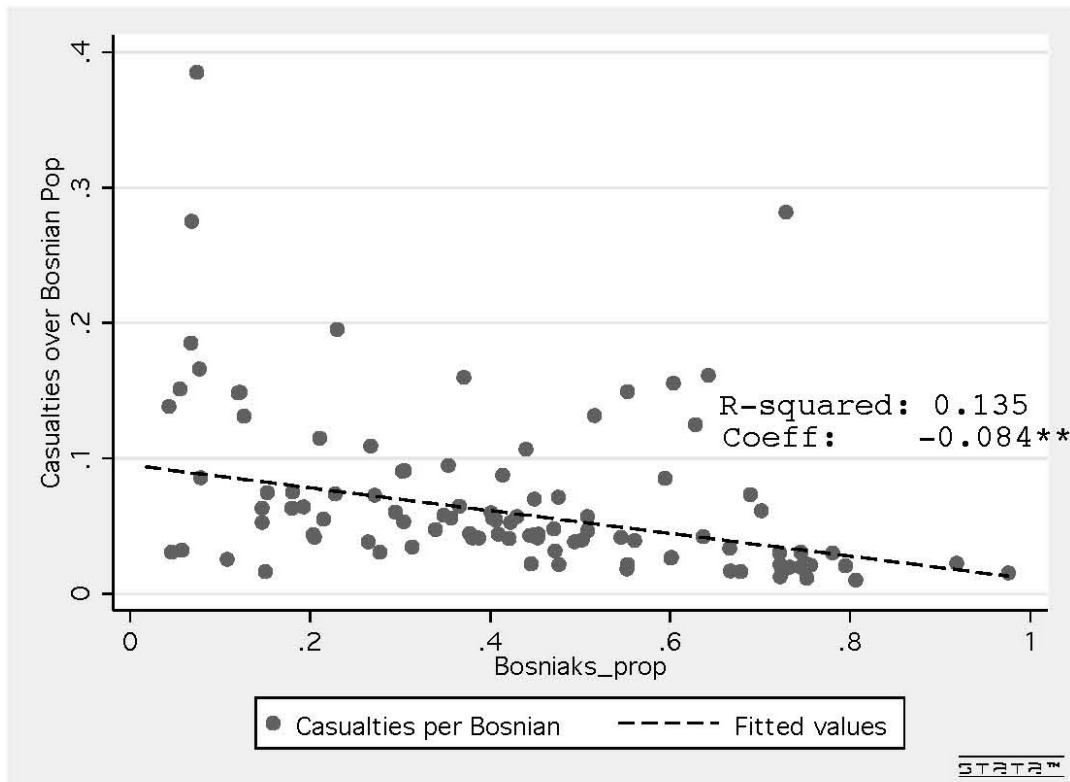
Figure 4: Pre-war ethnicity and war casualties at the municipality level. Results obtained using the RDCS population losses data and 1991 BiH Census data. On the Y-axis are the number of war casualties per Bosnians and on the X-axis the proportion of Bosnians in 1991. The scatter plot describes the data and the dotted lined presents the fitted values from a regression of proportion of Bosnians on the number of casualties per Bosnian. The associated regression R-squared is 0.135.
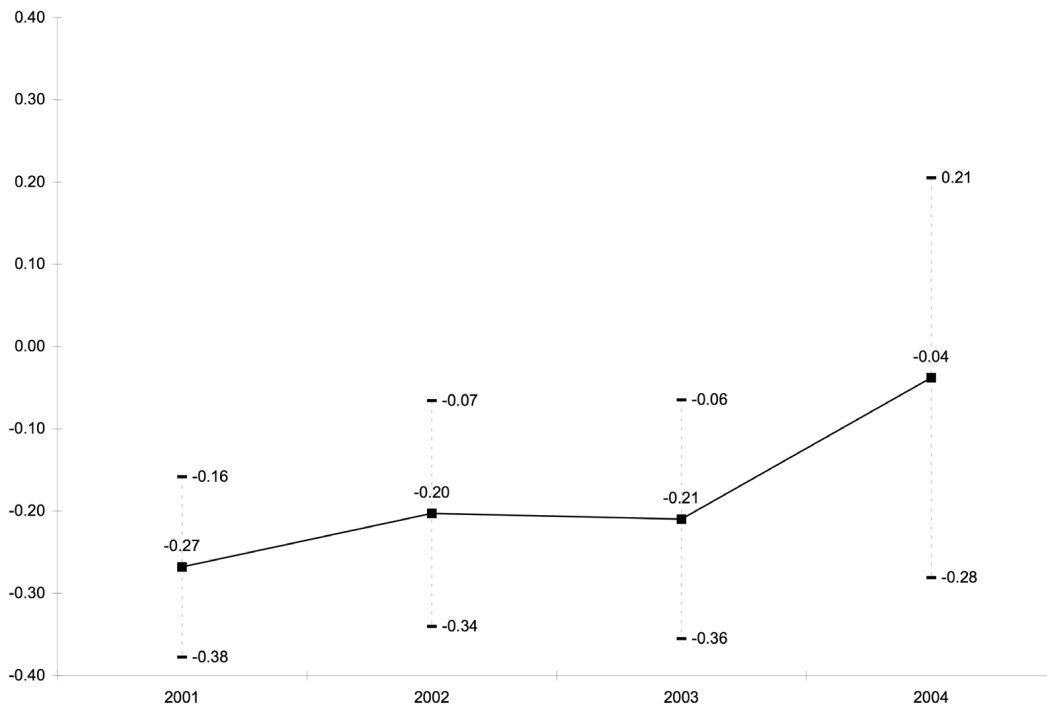
Figure 5: The effect of displacement on men's work for each year of the panel. The regression coefficients were obtained from 2SLS regression of the full model (controls and municipality of destination fixed-effects included) on each separate wave of the panel. Each coefficient is represented by a square and the values indicated on each side are the 95 percent confidence interval obtained with clustered standard errors at the level of the municipality of origin.
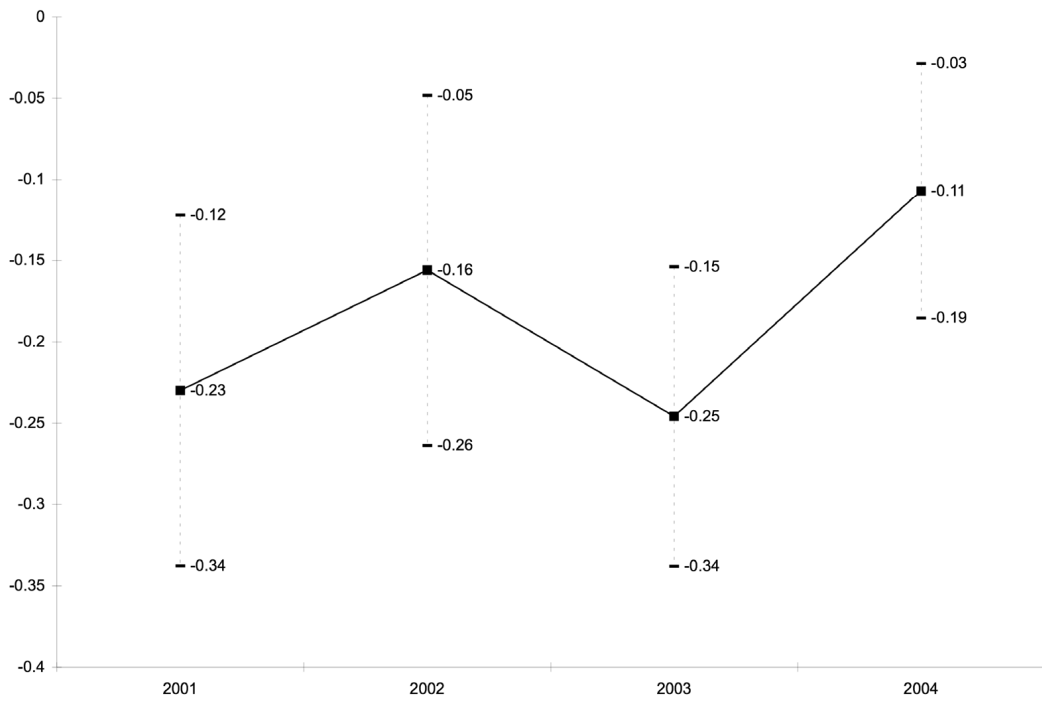
Figure 6: The effect of displacement on women's work for each year of the panel. The regression coefficients were obtained from 2SLS regression of the full model (controls and municipality of destination fixed-effects included) on each separate wave of the panel. Each coefficient is represented by a square and the values indicated on each side are the 95 percent confidence interval obtained with clustered standard errors at the level of the municipality of origin.

Table 1: Sample sizes, by gender, age group, entity and ethnicity (wave 1 of the panel only).

Age group	All entities and ethnicity	Federation				Republic			
		All	Bosnian	Serb	Croat	All	Bosnian	Serb	Croat
Men									
[18 ; 25]	376	202	144	2	36	174	15	157	.
[26 ; 40]	816	447	360	9	60	369	24	332	3
[41 ; 60]	1063	565	430	22	84	498	26	466	2
[61 ; 64]	174	85	59	4	21	89	8	78	1
Women									
[18 ; 25]	307	171	142	1	24	136	19	116	.
[26 ; 40]	829	494	407	8	60	335	14	308	2
[41 ; 60]	1162	629	476	20	90	533	37	484	7
[61 ; 64]	229	126	77	13	30	103	8	90	2

Table 2: Proportion of war migrants by gender, age group, entity and ethnicity (wave 1 of the panel only).

Age group	All entities and ethnicity	Federation			Republic		
		All	Bosnian	Serb	All	Bosnian	Serb
[18 ; 25]	0.246	0.150	0.146	0.000	0.349	0.067	0.281
[26 ; 40]	0.223	0.168	0.157	0.005	0.286	0.037	0.249
[41 ; 60]	0.218	0.140	0.125	0.007	0.295	0.040	0.255
[61 ; 64]	0.206	0.106	0.094	0.013	0.291	0.042	0.249
Total	0.224	0.149	0.138	0.005	0.302	0.044	0.257

Table 3: Men's time use - sample means.

Bosnian Men	Sample Size	Work	Unemp.	Inactive	Hours
By Age					
[18 ; 25]	689	0.363	0.464	0.173	42.8
[26 ; 40]	1525	0.675	0.238	0.087	43.0
[41 ; 60]	1845	0.693	0.125	0.182	41.5
[61 ; 64]	301	0.146	0.037	0.817	38.0
[18 ; 64]	4360	0.597	0.212	0.191	42.2
By entity					
Republic	296	0.348	0.480	0.172	43.4
Federation	4064	0.615	0.192	0.192	42.1
t of difference	--	9.132	11.862	-0.850	0.68
By displacement	**status**				
Stayers	3557	0.623	0.180	0.197	42.4
Displaced	803	0.483	0.352	0.164	40.7
t of difference	--	7.32	-10.93	2.13	1.75

Table 4: Women's time use - sample means.

Bosnian Women	Sample Size	Work	Unemp.	Inactive	Hours
By Age					
[18 ; 25]	641	0.229	0.438	0.332	41.4
[26 ; 40]	1655	0.302	0.280	0.419	38.5
[41 ; 60]	2074	0.237	0.118	0.645	37.7
[61 ; 64]	356	0.037	0.006	0.958	30.6
[18 ; 64]	4726	0.243	0.210	0.547	38.4
By entity					
Republic	310	0.219	0.261	0.519	40.2
Federation	4416	0.245	0.206	0.549	38.3
t of difference	--	-1.022	2.314	-1.010	0.99
By displacement	**status**				
Stayers	3442	0.262	0.193	0.546	38.6
Displaced	970	0.194	0.255	0.555	36.1
t of difference	--	4.81	-4.70	-0.30	2.72

Table 5: Eudcational attainment by gender, ethnicity, and displacement status. (wave 1 only).

	No schooling	Low education	Medium education	High education
Men				
All	0.017	0.275	0.616	0.092
By displacement status				
Stayers	0.012	0.273	0.605	0.088
Displaced	0.025	0.309	0.565	0.072
t of the difference	-2.67	-2.03	2.05	1.49
Women				
All	0.073	0.533	0.353	0.041
By displacement status				
Stayers	0.058	0.532	0.347	0.048
Displaced	0.091	0.528	0.350	0.019
t of the difference	-3.99	0.28	-0.16	4.48

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Table 6: War-time disability, chronic disease and casualties.

	Men				Women			
	(1)	(2)	(3)	Sample Size	(4)	(5)	(6)	Sample Size
Panel A								
Dependent: War disability								
Casualties	0.029	0.034	0.025	1071	0.014+	0.013	0.010	1179
	(0.028)	(0.025)	(0.024)		(0.008)	(0.009)	(0.012)	
Controls		✓	✓	--		✓	✓	--
Municipality Fixed-effect			✓	--			✓	--
Panel B								
Dependent: Chronic Disease								
Casualties	0.022	0.032	0.012	1071	-0.007	0.052	0.049	1179
	(0.071)	(0.058)	(0.061)		(0.050)	(0.042)	(0.044)	
Controls		✓	✓	--		✓	✓	--
Municipality Fixed-effect			✓	--			✓	--

Note: OLS regression results of the effect of casualties in the municipality of origin on: (Panel A) a binary variable for war-time disability status; (Panel B) a binary variable for current chronic disease suffering status. Sample restricted to wave 2 (year 2002) of the LBiH panel. Standard errors clustered at the level of the municipality of origin are in parentheses. Time trend and a constant are included in all specifications; Controls include: dummies for household status, dummies for marital status, individual age-group dummies (four years by four years from 23 to 62, and one for 63-64), household composition variables (number of dependent members aged 0, 1-5, 6-10, 11-15, and 16-17), dummies for educational attainment (medium and high); Standard errors in brackets. ** significant at 1%, * significant at 5%, † significant at 10%.

Table 7: First-stage estimates: The effect of the incidence of conflict at the municipality level on individual displacement decision.

First-stage estimates	Men			Women		
Dependent: Displaced						
	(1)	(2)	(3)	(4)	(5)	(6)
Instrument:						
Casualties	7.769**	7.852**	7.231**	5.642**	5.742**	5.789**
	(1.581)	(1.531)	(1.045)	(1.002)	(1.001)	(0.927)
Controls		✓	✓		✓	✓
Municipality Fixed-effect			✓			✓
Sample Size	1066	1066	1066	1180	1180	1180
Adjusted R-squared	0.18	0.19	0.35	0.15	0.23	0.32

Note: The Table reports the first-stage coefficients of the IV-2SLS regression (linear probability model), where displacement status is the dependent variable. Reported are the coefficients on Casualties (Proportion Missing/Killed) in a number of specifications. The sample is restricted to Bosnian men (left panel) and women (right panel) aged 18-64. Time trend and a constant are included in all specifications; Controls include: dummies for household status, dummies for marital status, individual age-group dummies (four years by four years from 23 to 62, and one for 63-64), household composition variables (number of dependent members aged 0, 1-5, 6-10, 11-15, and 16-17), dummies for educational attainment (medium and high); Standard errors in brackets. ** significant at 1%, * significant at 5%, † significant at 10%.

Table 8: Falsification Test on the first-stage estimates.

Placebo First-Stage Estimates	Men			Women		
Dependent: (non-conflict) migration status						
	(1)	(2)	(3)	(4)	(5)	(6)
Instrument:						
Casualties in the municipality of birth	-0.099	-0.147	-0.000**	-0.031	-0.310	-0.000**
	(0.548)	(0.540)	(0.000)	(0.438)	(0.450)	(0.000)
Controls		✓	✓		✓	✓
Municipality Fixed-effect			✓			✓
Sample Size	1066	1066	1066	1180	1180	1180
R-squared	0.00	0.08	0.31	0.00	0.05	0.26

Note: The Table reports the placebo first-stage coefficients of the OLS regression of non-war migrant status on casualties in the municipality of birth; non-war displacement is the dependent variable and non-migrants (stayers) are the control group. Reported are the coefficients on Casualties (Proportion Missing/Killed) in a number of specifications. The sample is restricted to Bosnian men (left panel) and women (right panel) aged 18-64. See also Note to Table 10.

Table 9: Conflict incidence and educational attainment.

Dependent:	(1) Proportion Missing/Killed	(2) Violence 0-20 pctiles	(3) Violence 20-40 pctiles	(4) Violence 40-60 pctiles	(5) Violence 60-80 pctiles	(6) Violence 80-100 pctiles
Bosnians						
Educ. med	-0.001	0.033	-0.133**	0.130**	-0.031+	-0.001
	(0.001)	(0.024)	(0.021)	(0.022)	(0.016)	(0.001)
Educ. high	-0.000	-0.045	-0.198**	0.298**	-0.049+	-0.000
	(0.001)	(0.040)	(0.035)	(0.037)	(0.028)	(0.001)
Observations	2144	2144	2144	2144	2144	2144

Note: This table reports the effect of educational attainment of the incidence of conflict in the municipality, at the individual level, using the first wave of the LiBH panel. The sample is restricted to Bosnian men and women aged 27 and above in 2001. The dependent variables are: the proportion of killed/missing (or "casualties", col.1), and five dummies indicating what range of the distribution of casualties the individual's pre-war municipality belongs to: 0-20th, 20-40th, 40-60th, 60-80th, and 80-100th percentiles in cols. (2) to (6) respectively. "Educ. med" indicates complete or incomplete high school, and "Educ. high", above complete high school (low education –primary school completed and below– is the excluded category). Individual characteristics are also included (see also Note to Table 10).

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Table 10: The effect of displacement on men's labour market outcomes, LiBH Waves 1-4 pooled.

Bosnian Men	OLS				2SLS			
	(1)	(2)	(3)	Sample Size	(4)	(5)	(6)	Sample Size
Dependent:								
Work	-0.115*	-0.126*	-0.075*	4350	-0.289**	-0.206**	-0.159**	4350
	(0.056)	(0.048)	(0.028)		(0.088)	(0.076)	(0.049)	
Unemp	0.154**	0.152**	0.093**	4350	0.222*	0.225**	0.194**	4350
	(0.052)	(0.044)	(0.025)		(0.092)	(0.074)	(0.065)	
Inactive	-0.039	-0.026	-0.019	4350	0.067	-0.019	-0.035	4350
	(0.024)	(0.022)	(0.017)		(0.058)	(0.045)	(0.061)	
Ln(wage)	0.100	0.085	0.084+	2366	-0.054	0.002	-0.166+	2366
	(0.080)	(0.060)	(0.047)		(0.171)	(0.165)	(0.093)	
Hours (zeros included)	-5.330*	-1.347	-0.851	4350	-12.179**	-0.842	-2.333	4350
	(2.609)	(0.873)	(1.271)		(4.290)	(1.853)	(1.562)	
Hours (zeros excluded)	-0.803	-1.509	-1.616	2437	2.196	-0.054	-2.630	2437
	(0.984)	(0.918)	(1.615)		(2.704)	(2.399)	(1.608)	
Controls		✓	✓	--		✓	✓	--
Municipality Fixed-effect			✓	--			✓	--

Note: The Table reports the pooled OLS (left panel) and 2SLS (right panel) regression coefficients on displacement over the 2001/04 LBiH panel. The dependent variables are dummies for work, unemployment, inactivity, the natural log of the monthly wage and weekly hours (with zeros included or not). The sample is restricted to Bosnian men aged 18-64. Time trend and a constant are included in all specifications; Controls include: dummies for household status, dummies for marital status, individual age-group dummies (four years by four years from 23 to 62, and one for 63-64), household composition variables (number of dependent members aged 0, 1-5, 6-10, 11-15, and 16-17), dummies for educational attainment (medium and high), a dummy for suffering from a war-time disability; Standard errors clustered at the municipality of origin level in parentheses. ** significant at 1%, * significant at 5%, † significant at 10%.

Table 11: The effect of displacement on women's labour market outcomes, LiBH Waves 1-4 pooled.

Bosnian Women	OLS				2SLS			
	(1)	(2)	(3)	Sample Size	(4)	(5)	(6)	Sample Size
Dependent:								
Work	-0.072*	-0.061*	-0.055*	4740	-0.175*	-0.173*	-0.187**	4740
	(0.034)	(0.026)	(0.025)		(0.069)	(0.074)	(0.041)	
Unemp	0.055**	0.039+	0.025	4740	0.048	0.058	0.011	4740
	(0.017)	(0.020)	(0.016)		(0.049)	(0.042)	(0.026)	
Inactive	0.017	0.023	0.031	4740	0.126+	0.115	0.176**	4740
	(0.039)	(0.026)	(0.025)		(0.064)	(0.070)	(0.037)	
Ln(wage)	-0.062	-0.019	0.017	973	0.503	0.346+	0.182+	973
	(0.082)	(0.052)	(0.044)		(0.300)	(0.201)	(0.102)	
Hours (zeros included)	-3.547**	-0.438	-0.470	4740	-7.692**	-0.234	-0.124	4740
	(1.242)	(0.339)	(0.423)		(2.735)	(0.375)	(0.663)	
Hours (zeros excluded)	-1.450	-1.905	-2.199*	1080	2.372	-0.891	-6.514*	1080
	(1.038)	(1.183)	(0.936)		(2.287)	(2.344)	(2.471)	
Controls		✓	✓	--		✓	✓	--
Municipality Fixed-effect			✓	--			✓	--

Note: The Table reports the pooled OLS (left panel) and 2SLS (right panel) regression coefficients on displacement over the 2001/04 LBiH panel. The dependent variables are dummies for work, unemployment, inactivity, the natural log of the monthly wage and weekly hours (with zeros included or not). The sample is restricted to Bosnian women aged 18-64. Standard errors clustered at the municipality of origin level in parentheses. See also Note to Table 10.

Table 12: Reduced form estimates of casualties on labor market outcomes.

Independent: Casualties	(1)	(2)	(3)
Panel A: Men			
Dependent:			
Work	-2.177+	-1.635+	-1.099*
	(1.152)	(0.856)	(0.430)
Unemployment	1.680+	1.699*	1.334**
	(0.988)	(0.842)	(0.359)
Inactivity	0.497	-0.064	-0.235
	(0.496)	(0.340)	(0.367)
Controls		✓	✓
Municipality Fixed-Effects			✓
Observations	4350	4350	4350
Panel B: Women			
Dependent:			
Work	-0.930**	-0.954**	-1.028**
	(0.279)	(0.283)	(0.153)
Unemployment	0.256	0.297	0.043
	(0.277)	(0.244)	(0.137)
Inactivity	0.674**	0.658*	0.985**
	(0.244)	(0.281)	(0.162)
Controls		✓	✓
Municipality Fixed-Effects			✓
Observations	4740	4740	4740

Note: The Table reports the pooled OLS regression coefficients of the effect of casualties in the municipality of origin on labor market outcomes over the 2001/04 LBiH panel. The dependent variables are dummies for work, unemployment and inactivity. The sample is restricted to Bosnian men aged 18-64. Time trend and a constant are included in all specifications; Controls include: dummies for household status, dummies for marital status, individual age-group dummies (four years by four years from 23 to 62, and one for 63-64), household composition variables (number of dependent members aged 0, 1-5, 6-10, 11-15, and 16-17), dummies for educational attainment (medium and high), a dummy for suffering from a war-time disability; Standard errors clustered at the municipality of origin level in parentheses. ** significant at 1%, * significant at 5%, [†] significant at 10%.

Table 13: The effect of education on displaced men's labour market outcomes.

Men	Work		Unemployed		Inac tive	
	(1)	(2)	(3)	(4)	(5)	(6)
OLS						
Displaced	-0.208**	-0.096*	0.232**	0.123**	-0.024	-0.027
	(0.064)	(0.047)	(0.046)	(0.044)	(0.039)	(0.033)
Med_High Ed	0.098*	0.135**	-0.036	-0.053**	-0.062**	-0.082**
	(0.045)	(0.022)	(0.028)	(0.016)	(0.022)	(0.015)
Displaced *	0.119	0.041	-0.117*	-0.045	-0.003	0.004
Med_High Ed	(0.079)	(0.066)	(0.055)	(0.062)	(0.045)	(0.036)
Controls	✓	✓	✓	✓	✓	✓
Municipality Fixed-effects		✓		✓		✓
IV-2SLS						
Displaced	-0.232+	-0.073	0.336**	0.218+	-0.103	-0.144+
	(0.117)	(0.086)	(0.078)	(0.113)	(0.073)	(0.082)
Med_High Ed	0.116*	0.160**	-0.032	-0.054*	-0.084**	-0.105**
	(0.047)	(0.027)	(0.027)	(0.022)	(0.028)	(0.022)
Displaced *	0.017	-0.088	-0.137	-0.048	0.120	0.136+
Med_High Ed	(0.120)	(0.101)	(0.095)	(0.124)	(0.074)	(0.075)
Controls	✓	✓	✓	✓	✓	✓
Municipality Fixed-effects		✓		✓		✓
Observations	4350	4350	4350	4350	4350	4350

Note: This table reports the effects, for men, of displacement, education, and the additional effect of education for the displaced, on the following labor market outcomes: work, unemployment, and inactivity. The upper panel presents the results from pooled OLS regressions over the 2001/04 LBiH panel, and the lower from the 2SLS regressions, instrumenting for displacement. The sample is restricted to Bosnian men aged 18-64. Standard errors clustered at the level of the municipality of origin are in parentheses. See also notes to Table 10.

Table 14: The effect of education on displaced women's labour market outcomes.

Women						
	Work		Unemployed		Inactive	
	(1)	(2)	(3)	(4)	(5)	(6)
OLS						
Displaced	-0.034	-0.021	0.039	0.015	-0.005	0.007
	(0.025)	(0.020)	(0.026)	(0.019)	(0.020)	(0.020)
Med_High Ed	0.267**	0.250**	0.058*	0.039*	-0.324**	-0.289**
	(0.025)	(0.028)	(0.022)	(0.016)	(0.022)	(0.027)
Displaced *	-0.080+	-0.072+	0.004	0.020	0.076**	0.052+
Med_High Ed	(0.041)	(0.038)	(0.034)	(0.029)	(0.027)	(0.028)
Controls	✓	✓	✓	✓	✓	✓
Municipality Fixed-effects		✓		✓		✓
IV-2SLS						
Displaced	-0.133*	-0.158**	0.059	0.010	0.073+	0.148**
	(0.054)	(0.025)	(0.042)	(0.024)	(0.038)	(0.035)
Med_High Ed	0.274**	0.246**	0.064**	0.059**	-0.338**	-0.306**
	(0.025)	(0.029)	(0.024)	(0.022)	(0.020)	(0.025)
Displaced *	-0.121+	-0.064	-0.018	-0.057	0.139+	0.122+
Med_High Ed	(0.071)	(0.061)	(0.045)	(0.040)	(0.081)	(0.070)
Controls	✓	✓	✓	✓	✓	✓
Municipality Fixed-effects		✓		✓		✓
Observations	4740	4740	4740	4740	4740	4740

Note: This table reports the effects, for women, of displacement, education, and the additional effect of education for the displaced, on the following labor market outcomes: work, unemployment, and inactivity. The upper panel presents the results of pooled OLS regressions over the 2001/04 LBiH panel, and the lower, of 2SLS regressions, instrumenting for displacement. The sample is restricted to Bosnian women aged 18-64. Standard errors clustered at the level of the municipality of origin are in parentheses. See also notes to Table 10.

Table 15: The effect of displacement on men's labour market outcomes: robustness check on border municipalities.

Bosnian Men	OLS				2SLS			
	(1)	(2)	(3)	Sample Size	(4)	(5)	(6)	Sample Size
Dependent:								
Work	-0.055	-0.094**	-0.081*	2752	-0.358**	-0.233**	-0.253**	2752
	(0.048)	(0.027)	(0.032)		(0.109)	(0.052)	(0.066)	
Unemp	0.111*	0.125**	0.129**	2752	0.226*	0.262**	0.379**	2752
	(0.044)	(0.036)	(0.036)		(0.084)	(0.061)	(0.048)	
Inactive	-0.056+	-0.031	-0.048*	2752	0.133	-0.029	-0.125*	2752
	(0.029)	(0.025)	(0.023)		(0.137)	(0.069)	(0.047)	
Controls		✓	✓	--		✓	✓	--
Municipality Fixed-effect			✓	--			✓	--

Note: Robustness check – This table replicates the regression results reported in Table 10 when the sample is restricted to Bosnian men aged 18-64 whose pre-war place of residence was not situation of the post-Dayton border between the RS and the FBH. See also notes to Table 10.

Table 16: The effect of displacement on women's labour market outcomes: robustness check on border municipalities.

Bosnian Women	OLS				2SLS			
	(1)	(2)	(3)	Sample Size	(4)	(5)	(6)	Sample Size
Dependent:								
Work	-0.057	-0.046	-0.043	3086	-0.184*	-0.171*	-0.213**	3086
	(0.048)	(0.040)	(0.040)		(0.075)	(0.073)	(0.036)	
Unemp	0.047*	0.025	0.015	3086	0.026	0.057	0.023	3086
	(0.020)	(0.021)	(0.020)		(0.067)	(0.057)	(0.023)	
Inactive	0.010	0.021	0.028	3086	0.158**	0.114*	0.190**	3086
	(0.051)	(0.037)	(0.040)		(0.054)	(0.056)	(0.033)	
Controls		✓	✓	--		✓	✓	--
Municipality Fixed-effect			✓	--			✓	--

Note: Robustness check – This table replicates the regression results reported in Table 11 when the sample restricted to Bosnian women aged 18-64 whose pre-war place of residence was not situation of the post-Dayton border between the RS and the FBH. See also notes to Table 10.

Table 17: The effect of displacement on men's labour market outcomes: robustness check on schooling age in 1992.

Bosnian Men	OLS				2SLS			
	(1)	(2)	(3)	Sample Size	(4)	(5)	(6)	Sample Size
Dependent:								
Work	-0.119*	-0.132*	-0.078*	3662	-0.299**	-0.210**	-0.158**	3662
	(0.056)	(0.050)	(0.029)		(0.089)	(0.077)	(0.053)	
Unemp	0.145**	0.145**	0.094**	3662	0.219*	0.213**	0.187**	3662
	(0.046)	(0.043)	(0.025)		(0.087)	(0.072)	(0.062)	
Inactive	-0.026	-0.014	-0.016	3662	0.079	-0.003	-0.029	3662
	(0.028)	(0.024)	(0.020)		(0.059)	(0.048)	(0.057)	
Controls		✓	✓	--		✓	✓	--
Municipality Fixed-effect			✓	--			✓	--

Note: Robustness check – Sample restricted to Bosnian men aged 27 and above in 2001. See also notes to Table 10.

Table 18: The effect of displacement on women's labour market outcomes: robustness check on schooling age in 1992.

Bosnian Women	OLS				2SLS			
	(1)	(2)	(3)	Sample Size	(4)	(5)	(6)	Sample Size
Dependent:								
Work	-0.070+	-0.072*	-0.058*	4098	-0.220**	-0.226**	-0.206**	4098
	(0.037)	(0.029)	(0.027)		(0.067)	(0.078)	(0.058)	
Unemp	0.071**	0.053*	0.036+	4098	0.081*	0.089*	0.018	4098
	(0.022)	(0.022)	(0.018)		(0.034)	(0.037)	(0.037)	
Inactive	-0.001	0.019	0.023	4098	0.138*	0.137+	0.189**	4098
	(0.047)	(0.031)	(0.029)		(0.061)	(0.069)	(0.036)	
Controls		✓	✓	--		✓	✓	--
Municipality Fixed-effect			✓	--			✓	--

Note: Robustness check – Sample restricted to Bosnian men aged 27 and above in 2001. See also notes to Table 10.

Table 19: Sector of Occupation and Job Types by gender and displacement status.

	Stayer (Mean)	*Displaced (Mean)*	*t-stat of difference*
Men			
Sectors			
Primary	0.166	0.160	0.31
Secondary	0.424	0.457	-1.19
Tertiary	0.399	0.360	1.45
Job Types			
Self-Employed	0.399	0.509	-4.04
Paid Employee	0.590	0.483	3.90
Help Family	0.009	0.005	0.81
Sample Size	2222	381	--
Women			
Sectors			
Primary	0.140	0.158	-0.65
Secondary	0.315	0.278	1.12
Tertiary	0.535	0.556	-0.58
Job Types			
Self-Employed	0.357	0.427	-2.01
Paid Employee	0.610	0.535	2.10
Help Family	0.032	0.037	-0.42
Sample Size	910	241	--

Note: Descriptive statistics of the sectors of activity and job type for those currently in work – Sample restricted to Bosnian men (upper panel) and women (lower panel) aged 18-64.