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A 'bright' side of war?

Armed conflict and female teen marriage in Azerbaijan

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Abstract Does exposure to armed conflict influence female teen marriage? Despite increasing attention to early marriage, its drivers and consequences, quantitative research on whether teen unions are affected by situations of armed violence is minimal. This paper addresses this gap by examining the relationship between exposure to the conflict in Nagorno-Karabakh over 1992- 1996 and teen marriage outcomes in Azerbaijan. Using data from the 2006 Demographic and Health Survey and from the Uppsala Conflict Data Program, I compare cohorts at risk of teen union before and during the conflict climax years with a modelling strategy that exploits information on forced displacement and spatial variation in conflict exposure. Results show that exposure to war violence in adolescent ages, its intensity and frequency, are associated with a lower risk of teen marriage. The largest reductions are observed in the cohorts who spent most of their adolescent ages under conflict and who were displaced as a result. For never- migrant conflict-affected girls, declines extend to the youngest cohorts. The combination of age at conflict exposure, its duration and the experience of disruptive events like forced migration matters for teen marriage outcomes.

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Introduction

There are at least three reasons why demographers and policymakers should be concerned with whether armed violence affects early union formation. The first relates to the scale of the issue: globally, over 650 million women alive today – or 1 in 5 – married in adolescence, and the highest rates of teen unions, i.e., marriages involving girls aged 12-19, are in countries with great levels of political violence (UNICEF 2020, 2013). With a growing number of people and children living in conflict-torn contexts, the issue evidently has the potential to impact the lives of increasingly many girls and families worldwide (Østby et al. 2020; UNOCHA 2019). Second, early marriage is a violation of the Universal Declaration of Human Rights that has profound and lasting consequences on individuals, e.g., educational and socio-economic disadvantage (Dahl 2010; Lyngstad 2006), poor pregnancy outcomes and higher maternal mortality (Ganchimeg et al. 2014; Nove et al. 2014), domestic abuse and union dissolution (Kiplesund and Morton 2014; Teachman 2002), and implications for future generations and other aspects of social life, including gender equality and public health (UNICEF 2005; Mourtada et al. 2017; Nour et al. 2006). Situations of armed violence exacerbate these human and social costs (Mazurana et al. 2019). Third, in many low- and middle-income countries, shifts in union formation are strongly tied to changes in the timing of childbearing, future fertility patterns and long-term population dynamics. If women marry sooner, *ceteris paribus*, reasonably, their lifetime fertility will be higher and contribute to population growth (Onagoruwa and Wodon 2018). Anticipating similar scenarios is key for post-conflict reconstruction strategies, development and resource allocation (Duflo 2005; Thiede et al. 2020).

Nonetheless, demographic research on the relationship between armed violence and teen marriage is remarkably scarce. This study addresses this lacuna. Specifically, it examines whether women in Azerbaijan who were exposed to the Nagorno-Karabakh conflict with Armenia and reached their teens in its climax years (1992-1996) had different early marriage

trajectories as compared to their non-exposed peers and to women who were 'at risk' of teen union in the pre-conflict Soviet era.

In theory, the relationship could go either way. War may promote early unions through mechanisms that include the search of economic and/or physical security for girls and their families, nationalist pro-natalist policies and reinforced gender roles (Neal et al. 2016). Alternatively, armed conflict could induce families to postpone the marriages of their young daughters because of financial hardship, forced migration and disrupted social networks, among others (Shemyakina 2013; Staveteig 2011). The extent of these competing scenarios further depends on pre-existing trends in age at marriage, the duration and ages at conflict exposure (Neal et al. 2016).

Net of a recent mixed-methods study on early marriage practices among Syrian refugees in Jordan (Sieverding et al. 2020), quantitative research so far considered overall marriage patterns only, and yielded inconclusive answers with regards to the sign, and even to the actual presence of a relationship (De Walque 2006; Khawaja and Randall 2006; Jayaraman et al. 2009; Randall 2005; Saxena et al. 2004; Shemyakina 2013; Staveteig 2011; Valente 2011). Inasmuch as this literature provides valuable contributions, the focus on general marriage outcomes overlooks the particular vulnerabilities of young population segments in conflict**.** Further, most of this evidence relies on time-trend comparisons and rarely studies deal with conflict-related migration.

To tackle these issues, I use data from the 2006 Azerbaijan Demographic and Health Survey and conflict information from the Uppsala Conflict Data Program. I estimate survival models specified with a difference-in-difference logic that exploits data on forced displacement, spatial variation in conflict exposure and a cohort specification that accounts for the risk marrying in teen ages before and during the war. The results provide evidence of a significant and robust negative relationship between exposure to conflict, its intensity and frequency, and teen union

formation, with the largest reductions characterising the cohorts who spent most of their teens under active conflict conditions. Further, findings on response heterogeneity by conflict-related migration suggest displacement as a plausible driver of the lower early marriage levels of these cohorts.

This paper makes a unique contribution to the literature on households' demographic responses to war and socio-economic turmoil as the first to provide empirical evidence directly on teen marriage. Moreover, unlike other accounts of the demographic consequences of armed violence, the available data and peculiar characteristics of the Nagorno-Karabakh conflict allow to explicitly handle and examine forced migration. Albeit findings cannot be interpreted as strictly causal, the use of a design strategy seeking to isolate as much as possible the impact of conflict represents another improvement to the relatively narrow methodological approaches used until now in related research. The study context is also highly pertinent to the research purpose. Since independence and the onset of the dispute with Armenia, Azerbaijan has reported an increasingly high share of marriages involving teenagers (Statistical Committee of Azerbaijan (SSC) 2011) and today it has one of the greatest rates of adolescent union in Eurasia (UNFPA 2012, 2014). Differently from the other handful settings studied previously (e.g., Rwanda or Tajikistan), where conflicts reached a peaceful settlement, Azerbaijan's case also allows investigating the issue in relation to a conflict that was officially 'frozen' until 2020 (Cornell 2017), when violence re-escalated. This makes the findings of tangible interest for policy in Azerbaijan, and in many other turbulent settings where unsettled conflicts have increasingly begun to evolve into similar simmering dynamics.

Armed conflict and teen unions: pathways and factors

Despite growing political and programmatic attention to early unions and to women's vulnerabilities in conflict, knowledge on the influence of armed violence on female adolescent marriage is largely limited to qualitative studies, which tend to suggest conflict-related

increases (Kohno et al. 2020; Mourtada et al. 2017; Schlecht et al. 2013). Quantitative research assessing magnitude and drivers – or even just confirming the existence and direction of the relationship at the population-level – is scarce (Neal et al. 2016). To date, only Sieverding et al. (2020)'s mixed-method study on Syrian refugees in Jordan examined changes in early marriage associated with conflict with solid statistical analyses. The authors found no evidence of increases in rates, in contrast with what was alleged by some qualitative reports and other accounts overlooking the selectiveness of the study population.

A handful more demographic studies have at least focused on population-level changes in general marriage patterns associated with armed conflict, offering mixed results. Some of these document declines in union formation in wartime. For example, Khawaja and Randall (2006) and Saxena et al. (2004) find decreasing rates of union formation during the second Palestinian Intifada and the Lebanese civil war respectively. In both cases, the declines occurred for most women, including girls aged 15-19. Union postponement and increasing marriage age were also observed during the Bosnian war (Staveteig 2011), the Rwandan genocide (Jayaraman et al. 2009; Verpoorten and Schindler 2012) and, at least temporarily, under the Khmer Rouge regime in Cambodia (De Walque 2006). In the one study examining a former Soviet context most similar to Azerbaijan, Shemyakina (2013) shows that women in conflict-torn areas who attained marriage age during or just after the Tajikistan civil war were less likely to marry than their non-exposed counterparts.

However, other analyses report conflict-related marriage increases, even in the same contexts as some of studies introduced above. For instance, both Staveteig (2011) and Clifford et al. (2010) note a faster entry into marriage and increasing rates of union formation, especially for young women, during the peak years of the Rwandan and Tajik conflicts. Although these seemingly stand at variance with previously mentioned findings, the discordance is the result of different methodologies. While Staveteig (2011) and Clifford et al. (2010) analyse only temporal changes in trends within the whole population, the other papers rely on more

advanced statistical techniques and, importantly, seek to identify women exposed to war more accurately (Jayaraman et al. 2009; Verpoorten and Schindler 2012; Shemyakina 2013). Marriage increases are though also documented in research on Nepal that uses finer measures of conflict exposure (Valente 2011; Williams et al. 2012), and in a study looking specifically at the sub-group of displaced Tuareg in Mali (Randall 2005). Here, a short-term increase in the share of marriages involving young women reverted back as the Tuareg rebellion ended.

Results from these studies are evidently not univocal, presumably because of their different methodologies and limited attention to age-groups, sub-populations (e.g., displaced), and to the impact of secular shifts, migration or other simultaneous factors. Yet, these papers have the merit of proposing several general explanatory mechanisms that – though likely applying to young girls with different intensities than, for instance, to women in their mid/late-20s – can serve to guide expectations specific to teen unions and support interpretation.

Most studies propose more than one mechanism – often acting concurrently and intersectionally – to explain conflict-related changes in marriage. Among these, the most salient for adolescents are economic and security-related factors (Neal et al. 2016). For example, in the studies that at least mention changes in marriage rates among adolescents, declines are explained by the stretching of households' resources, scant employment and housing options (Khawaja and Randall 2006; Saxena et al. 2004). As economic conditions deteriorate, conflict-affected families may divert spending from the payment of ceremonies to more immediate needs, e.g., health and re-location expenses, at least in the short-term, or may be unable to afford good-sized dowries (see p.12 for more on marriage-related payments in Azerbaijan) (Neal et al. 2016; Sieverding et al. 2020).

Economic reasons are also indicated as plausible drivers of conflict-related marriage increases (Randall 2005; Staveteig 2011; Valente 2011). In times of crisis, marriage may be perceived as a 'consumption-smoothing' tool generating economies of scales, and thus useful to pool

scarce resources (Rosenzweig 1989). We may then expect, as some qualitative papers suggest, conflict-stricken households to opt for "transactional" early marriages as means to secure financial support to young girls, offload family economic responsibilities and extend networks (Mourtada et al. 2017; UNICEF 2013; Hoogeveen et al. 2011). If conflict hits hard on schooling infrastructures, resulting in the permanent drop out of young cohorts, this option may appeal especially to the youngest and lowest-educated girls (Cetorelli 2014).

In addition to economic insecurity, armed conflict brings about physical risks to which girls are particularly vulnerable, and that may increase their likelihood of marriage. For example, households may expedite the marriages of girls to protect them and their honour from forms of physical harm like rape or abductions (Randall 2005; Sieverding et al. 2020; Shemyakina 2013). At the same time, the hunt for physical safety often entails forced migration. This can split existing couples, delay already organised marriages or disrupt social networks functional to finding partners (Hutchinson et al. 2016; Crawford et al. 2015). This latter aspect is particularly important for girls of adolescent ages when displaced, given that early unions often rely on consanguineous (kin) relationships (Sieverding et al. 2020) and/or are facilitated by parental social connections in the local community (Schaffnit et al. 2019). Women and girls may also experience reduced chances to marry if their physical security is violated during conflicts. For instance, those maimed, injured or raped may be perceived as "less desirable" by potential grooms or may themselves be reluctant to marry following conflict trauma (Staveteig 2011).

Another cited driver of war-induced changes in marriage patterns relates to variations in sex-ratio and shortages of men. While De Walque (2006) and Verpoorten and Schindler (2012) attribute the temporary declines observed in Cambodia and Rwanda to the mass mobilisation and excess mortality of young men, Staveteig (2011) argues that sex-ratio imbalances could increase the prevalence of informal or polygamous unions as widowed or young unmarried women look for sources of support. In the study setting most akin to Azerbaijan, Shemyakina

(2013) though finds no relationship between variation in local sex-ratios during Tajikistan's war and female age at marriage.

Broader structural factors are also mentioned (yet, rarely tested empirically) in studies on general marriage outcomes (Staveteig 2011). Of these, conflict-induced shifting gender dynamics, rising nationalism and the break-down of social cohesion are seemingly the most relevant to teens (Neal et al. 2016). Young women's increased participation to non-traditional roles, e.g., in the workforce or in the battlefield, may result in empowerment gains and greater control over life choices, including the deliberate decision to delay marriages (McKay and Mazurana 2004; Etchart and Baksh-Soodeen 2005). Alternatively, conflict may reinforce stereotypical gender attitudes and elevate the expectation of female domesticity. Together with pro-natalist narratives encouraging the "need" to maintain a demographic balance with the enemy and "compensate for" conflict losses, this may expose girls to higher social pressure to marry (Staveteig 2011; Chi et al. 2015). Finally, war impinges on social embeddedness, i.e., the breadth, depth and extent of social cohesion within a community (Takács 2005). The resultant reduced social trust can complicate the search for partners, notably in traditional societies where kin and intra-community are usually harnessed to arrange weddings (Jayaraman et al. 2009).

Overall, the impact of war on union dynamics is more complex than it may appear at first: not only there is conceptual and empirical ambiguity on the various, perhaps synchronous and offsetting drivers; even the sign of the relationship is unclear, particularly for teen marriage (Neal et al. 2016). It is also unknown whether girls' age, duration and type of exposure trigger different responses. The overarching aim here is therefore to determine as neatly as possible *whether* conflict exposure, in its spectrum of manifestations, is actually associated with teen union; then, to explore specificities, including differences by age at exposure, its intensity, frequency and length, and advance suggestions on plausible explanatory processes.

The study context

Post-Soviet Azerbaijan: socio-economic changes and the conflict with Armenia

Significant financial deterioration and instability characterised Azerbaijan's post-Soviet path to regime change (World Bank 2005; Singh and Laurila 2011). The transition period was further complicated by the outbreak of conflict violence with Armenia over Nagorno-Karabakh, a mountainous region officially recognised as part of Azerbaijan (UN Security Council 1993a-d; UN General Assembly 2008), but which Armenia regards as an Armenian historical area of residence (Human Rights Watch (HRW) 1994; Cornell 2001, 2017).[1]

The conflict traces its roots to the last years of the USSR and its structural arrangements. During the Soviet era, the region was granted an autonomous status – the Nagorno-Karabakh Autonomous Oblast (NKAO) – within the then Azerbaijan Soviet Socialist Republic, but its borders contained a sizable Armenian population (USSR Population Statistical Collect 1988; de Waal 2004). When the Soviet centre-dominated control system crumbled, tensions mounted in NKAO and demonstrations reclaiming Nagorno-Karabakh's membership to Armenia extended from Stepanakert/Khankendi (the capital of NKAO) to Yerevan (de Waal 2004). Violent rallies causing casualties took place also around Baku.

Confrontational politics turned into outright conflict in December 1991 when, with Armenian support, NKAO proclaimed independence from Azerbaijan (HRW 1992, 1994). Although disagreement between sources exists on the exact start and end dates of the hostilities, most analysts and official sources indicate early 1992 as the beginning of the full-blown war, 1992-1994 as its most violent period (Fig.A1, Appendix A), and the post-1994 armistice years (1995-1996) as a 'cooling-off' phase still characterised by instability, attacks on civilians and conflict-related population movements (HRW 1992, 1994; Commission on Security and Cooperation

[1] This section seeks to summarise as neutrally as possible the conflict's chronological developments using available official sources and documents. In no way I compare human rights violations on either sides or suggest any specific stance on the dispute.

in Europe and Washington (CSCE) 2012; International Crisis Group (ICG) 2005; Cornell 2015; Huseynov 2010; Krüger 2010).

Since then, the conflict has been described as "frozen" (Cornell 2017; Bebler 2015).[2] The resultant *de facto* Republic of Nagorno-Karabakh (also known as Republic of Artsakh), the Western parts of three other officially Azerbaijani districts (Agdam, Fizuli and Terter) and the region of Kelbajar-Lachin became entirely populated and controlled by ethnic Armenians. Altogether these territories comprise approximately 20% of Azerbaijan's internationally recognised territory (Racz 2016). Only the Eastern segments of Agdam, Fizuli and Terter remained under Azerbaijan's jurisdiction as parts of what, in Azerbaijani language, is known as the Upper-Karabakh (Yuxarı-Qarabağ) region (UN Security Council 1993a-d).[3]

An estimated 17,000-25,000 Azerbaijani died in the conflict (HRW 1994; de Waal 2004; Yunusov 2002). No official or consolidated gender/age-disaggregated estimate is available either for civilian or military deaths.[4] However, some evidence suggests that the killing of civilians and other atrocities, like rape and torture, occurred indiscriminately on both sides, and that Azerbaijani military losses were predominantly males (Amnesty International 1993; HRW 1994; UNDP 2007).[5] The conflict further imbued an already patriarchal society with a

[2] The term 'frozen' conflict defines a post-conflict situation where active armed fighting has ceased, but no stable peace agreement has been reached between contenders. Although in 1994 an armistice halted open combat, this was not accompanied by a peace deal.

[3] There is substantial debate over the names given to the region and adjacent districts (Broers (2019: p.85) for a comprehensive discussion). From here and throughout, I use the term "Nagorno-Karabakh" to refer to all the territories under the control of Armenian-supported separatists until survey time, i.e., the *de facto* Republic of Nagorno-Karabakh/Artsakh (which encompasses the Western parts of Agdam, Fizuli and Terter) and the Kelbajar-Lachin economic region. I employ "Upper-Karabakh" to refer to the Eastern parts of Agdam, Fizuli and Terter. See Fig.1 for more.

[4] Estimates from Azerbaijan's government are limited to "hundreds" of deaths and injured (Supreme Court of the Government of Azerbaijan 2005).

[5] According to the only source (in Russian) citing women's involvement in direct combat, around 100 female soldiers fought in the conflict (Oganian and Mkhitarian 2001: p.246). It is unclear whether the figure includes Armenian-Karabakhi women only or also ethnic Azerbaijani. Differently from male deaths, killed or injured women in this conflict have been described as "invisible victims" (Twum et al. 2019).

nationalist rhetoric that celebrated male fighters as heroic "martyrs", while valued women for their roles of wives and mothers of future defenders (Twum et al. 2019).

Antagonistic nationalism was fuelled by the plight of displacement. The conflict caused the mass expulsion of *all* ethnic Azerbaijani from Armenia and Nagorno-Karabakh. Although exact numbers are still contested, over 750,000 Azerbaijani – seemingly equally divided by gender (UNHCR 2009) – had to relocate to safer areas within Azerbaijan and were granted IDP/refugee status (CSCE 2017; HRW 1994). This heavy inflow, about 10-15% of the country's then total population of 8 million, for years made Azerbaijan the country with the largest per capita number of IDPs in its national population (UNHCR 2009; Greenway 2009). As of 2016, one in 15 Azerbaijani was still displaced and none lived in Nagorno-Karabakh (UNHCR 2017).

Marriage traditions and early unions in Azerbaijan

Marriages are central to Azerbaijani culture, and have important socio-economic functions (Tohidi 1999). The formalisation of unions involves large spending for celebrations and expensive financial transactions, including the dowry paid by the bride's family (*cəhiz*), the bride payment made by the groom (*başlığ*) and other inter-families material exchanges. This borrowing and lending of currency and assets then serves to enact social status and expand networks (Yalçın-Heckmann 2001).

For these reasons, and the social stigma attached to late marriage or singlehood, early marriages were common in pre-Soviet Azerbaijan (UN Azerbaijan 2015; Havilov 1991). In the Soviet period, however, rates declined sharply due to several measures targeting Islamic and customary marriage practices, e.g., bans on child marriage, polygamy, arbitrary divorce, and to mandatory schooling for women (Edgar 2006; Heyat 2014; Lapidus 1978).

Since independence in 1991, and even after the 1995 legal prohibition to contract marriage before 18, official figures have yet reported an increasing share of marriages involving

teenagers (SSC 2011). These numbers are likely an undercount since official statistics only include marriages registered at State agencies, whereas unions involving adolescents tend to be first celebrated with unofficial religious ceremonies (*kebin* or *nikah*) and formally registered once the youngest spouse (typically the bride) reaches the legal marriageable age (UNFPA 2014).

Data and measures

Marriage data

Teen marriage is here defined as unions involving girls aged 12–19 (Dahl 2010). Information on the timing of marriage come from the 2006 Azerbaijan Demographic and Health Survey (AZ-DHS), which contains complete marital histories for a nationally representative sample of 8,444 women aged 15–49 years. This was generated in two stages: clusters were first selected in Baku and in Azerbaijan's other administrative units using the 1999 Population Census as sampling frame. Households were listed in each cluster and systematically selected, with an overall response rate of 98% (SSC and Macro International Inc. 2008). For security reasons, the sample excluded the Nakhchivan exclave and, due to their contested status, the Kelbajar-Lachin economic region and the Western parts of Agdam, Fizuli and Terter (Fig.1). In 2006, these latter were *de facto* part of the Republic of Nagorno-Karabakh and only populated by ethnic Armenians (National Statistical Service of Nagorno-Karabakh 2006), not the focus of this paper.

As weddings involving adolescents sometimes go unregistered until spouses grow older, to effectively capture the date of marriage rather than its registration the survey asks respondents: *"In what month and year did you start living with your (first) husband/partner as if married?"*. I use this question to construct my dependent variable and analyse 'survival' time to teen marriage.

Conflict data and indicators

To determine changes in teen union associated with exposure to war, it is crucial to correctly identify the *exposed*. This is usually challenging for the simple reason that conflict zones are complex territories for data collection. To overcome this inherent difficulty, I use two main data sources: the AZ-DHS and the Uppsala Conflict Data Program Georeferenced Event Dataset (UCDP-GED). The former allows to construct a discrete 'broad' indicator, intended to capture conflict exposure in its widest manifestations; the latter, to construct two continuous indicators that help delving more into the specific relationship with its frequency and the intensity. I next describe the construction of each measure in detail.

1. The AZ-DHS: Overall conflict exposure indicator

The first measure – the *overall conflict exposure* indicator – is a binary variable that I construct exploiting the rare conflict-sensitive questions included in the AZ-DHS. Unlike most household surveys, the AZ-DHS asks all respondents aged 16+ about their IDP/refugee status. If an interviewee identifies either as a refugee or an IDP from Nagorno-Karabakh, s/he is then asked about the country or district s/he moved from as a result. This permits the identification of a first group of conflict-exposed: women directly affected by the conflict – whether in Armenia or Nagorno-Karabakh – *and* who also experienced resultant forced displacement.

Next, the questionnaire asks about the length of respondents' stay in the current place of residence. As in Torrisi (2020), I use this information to identify a second group of conflict-affected respondents: women who always resided (or migrated pre-conflict, i.e., before 1992, in the Upper-Karabakh region, namely in Azerbaijan-controlled and sampled areas of Agdam, Fizuli and Terter. These women were not forced out of their territories and, perhaps, their specific villages did not suffer from major disruptions. However, these were still affected by deadly conflict events. Importantly, due to residential proximity to the core conflict zones and the contested status of their districts, these women likely faced recurring indirect exposure and subtle conflict-related insecurities (e.g., fear of coercive acts, warfare extending to their

territories or land expropriation), with potential behavioural consequences for family-related decision-making. I also include in this group a few non-IDP/refugee women (*n*=54) who migrated to these districts during conflict years.

Lastly, I use AZ-DHS information to identify a third group: non-refugee/IDP women who had one or more male members of their natal household (father, brother, uncle, granddad) or the mother, if she was the household head, who declared being displaced by the war. In the initial phase of the exodus, registration costs (e.g., travel to registration points, paperwork fees) were relatively high, while food allowances were granted to families as long as their head was a registered IDP/refugee (UNHCR 2009; ICG 2012; Twum et al. 2019). Although the survey was implemented sufficiently after to make-up for any initial under-registration, it is possible that some exposed women went unregistered (and hence unreported) for the above reasons. This coding procedure thus seeks to tackle this potential source of underreporting.[6]

Overall, by combining and defining these three groups who may have been exposed to conflict directly (by experiencing violence and displacement themselves) or indirectly (through physical proximity, or having exposed family members) as "*conflict exposed*", this first indicator serves as a starting point to capture exposure in all its possible manifestations. Subsequently, I separate its specific components to learn about potential heterogeneity across migration status.

2. The UCDP-GED: Conflict intensity and frequency indicators

I complement the discrete indicator with two supplementary continuous variables for *frequency* and *intensity* of exposure, using conflict event and fatality data from the UCDP-GED. This is

[6] Evidently, this strategy captures only any underreporting of women living with their origin, conflict-affected families in 2006, not those living outside (e.g., with partners). While I cannot rule this out, evidence suggests that even when IDP women married non-IDP men, these rarely forwent their status because of its associated social protection and assistance benefits, and allegedly as a preventative measure against divorce (ICG 2005).

an openly available dataset providing spatial and chronological coordinates on conflict episodes and casualties worldwide (Croicu and Sundberg 2016).[7]

Several studies examining conflict effects on other outcomes exploit the UCDP-GED georeferenced nature and link the dataset directly with geolocated survey clusters (e.g., Østby 2020). Unfortunately, the AZ-DHS did not gather fine-grained GIS cluster data that would allow similar standard procedures, and only provides numerical information on women's current district of residence. This implies that we know their economic region of residence (e.g., Aran, Baku, Ganja-Qazakh), but only the numeric code of their specific district in that region.[8] However, the AZ-DHS makes it possible to trace back IDPs' origin district before they fled Nagorno-Karabakh. We also know that women in Upper-Karabakh reside in the sampled parts of either Agdam, Fizuli or Terter. Accordingly, I can creatively exploit UCDP-GED data and link them to the groups used to construct the binary indicator.

I do so in a sequential manner. First, I mapped the exact location of all conflict events and related fatalities occurred between January 1992-December 1996 as recorded by the UCPD-GED. Figure 1 shows their spatial distribution. About 81% of events (blue dots) and almost all casualties (orange dots) occurred in Agdam, Fizuli and Terter or in areas characterised by complete forced migration (Nagorno-Karabakh and Kelbajar-Lachin). This allows capturing intensity and frequency of exposure with a good degree of accuracy. Second, I calculated the district-level number of conflict episodes (*frequency*) and fatalities per 1,000 population as per the 1989 USSR Population Census (*intensity*) between 1992-1996. Third, I matched the computed values to the groups earlier identified as "conflict exposed".

Based on their origin district in Nagorno-Karabakh, I assigned the specific district-level values of each continuous indicator to IDP women (and to women with an IDP/refugee household

[7] UCDP-GED data are widely used in research and judged to be of the highest quality available for this study's aims.

[8] It was not possible to obtain a list of district names matching numerical codes due to confidentiality.

member as described above). I assigned to refugees from Armenia the average value of conflict events and deaths occurred across all districts in Nagorno-Karabakh because we do not know where these women lived in Armenia and hence the exact extent of violence they experienced when there. The values are similar to averages of conflict episodes and fatalities occurred across districts of Armenia. Finally, I assign to permanent residents of Upper-Karabakh districts (Agdam, Fizuli, Terter) averages of conflict events and fatalities occurred in these three districts between 1992-1996. For the few women who migrated to these districts during the conflict years, I calculate the same conflict measures, but starting with the year they arrived rather than 1992. For instance, the mean number of conflict events across the three districts in Upper-Karabakh was 13 between 1993-1996 and 9 between 1994-1996. If a woman moved to these districts in 1993, she is considered exposed to 13 events; to 9 if she moved in 1994. All other women, including non-IDP/refugees residing in districts affected by some conflict events in otherwise relatively peaceful regions, e.g., Ganja-Qazakh (North-West), are considered as exposed to no events/fatalities. I address this potential measurement error in the robustness checks.

Again, these continuous measures are to be understood as finer, yet supplementary to the binary indicator. Table A1 (Appendix A) summarises all conflict measures and their mutual relationship.

Empirical strategy

I study the link between exposure to the Nagorno-Karabakh conflict and teen marriage in Azerbaijan adopting a difference-in-difference (DID) logic that leverages on variation in exposure across cohorts and space (i.e., where respondents lived at the time of the war). In its simplest form, the DID design envisages two populations and two time points. In the first period, both populations are exposed to the same conditions. In the second, a "treatment" unrolls in one population ("treated"), but not in the other ("control/comparison"). Following

this standard language, the "treatment" condition is here determined by conflict exposure. The design of this paper slightly differs from the traditional DID in its time component: rather than using a pre/post-treatment time-period variable, I rely on cohort variation. This is because the main goal is to focus on teen ages, and conflict peaked in specific years; once these are fixed, the only variation comes from women's year of birth. This strategy also allows to fully harness the survey retrospective nature in lack of pre-/post-conflict rounds.

Following Shemyakina (2010) and O'Brein (2020), I specify cohorts based on women's entry into/exit from the pool of marriageable adolescents and their ages between 1992-1996. Table A2 in Appendix A shows women's age at conflict start (1992), after it peaked and ended (1996) and at data collection (2006). It further presents the corresponding year in which they 'started' (turned 12) and 'ceased' (turned 19) to be at risk of teen union, and the sample size of each cohort. Women aged 21+ at conflict onset (born $1957 \leq k \leq 1971$) were teenagers before the dissolution of the Soviet Union and, hence were too old to have their teen marriage outcomes impacted by armed violence. I refer to this group as the "*Soviet cohort"*. Conversely, I define as the "*War-cohort*" women who turned 12-19 during the peak conflict years (hence born $1974 \leq k \leq 1984$). I later further disaggregate this group into women who passed their *late* (born $1974 \leq k \leq 1977$), *almost entire* ($1978 \leq k \leq 1980$) or *early* ($1981 \leq k \leq 1984$) teens under conflict to examine differences across groups who spent more or less of their adolescent time 'at risk' of marriage in war conditions.

I estimate complementary log-log (cloglog) survival models to exploit marriage history data and the time-to-event nature of the outcome variable. Survival models are preferred to standard OLS models as they allow accounting for censoring of the observations and exit from the risk-set at different times for each subject. Further, I chose a cloglog link function because the survey records duration data in discrete units, and the probability of the event is small. The cloglog model is also the discrete-time analog of a proportional hazard model and thus coefficients, once exponentiated, can be interpreted as hazard ratios (Allison 1982).

Equation 1 presents the basic statistical framework for empirical analysis:

$$\log(-\log(1-\pi_{ikdt})) = \alpha_t + \gamma\ Conflict\ Exposure_i + \beta\ (Conflict\ Exposure_i \times War\text{-}Cohort_{ik}) + \theta_k + \lambda_d + \upsilon_i + \varepsilon_{ikd} \quad (1)$$

where π_{ikdt} is the conditional probability of teen marriage at interval *t* for woman *i* in cohort *k* in district *d* at wartime, provided that she has not already married. The term $War\text{-}Cohort_{ik}$ indicates women who turned 12-19 during conflict. In the main specification, $Conflict\ Exposure_i$ is the binary *overall conflict exposure* indicator. The coefficient β of the interaction term captures the relationship of interest. That is, it identifies the relationship between overall conflict exposure and the probability of entering teen marriage in the *War-cohort*. In alternative specifications, I relax the binary indicator into the continuous frequency (events) and intensity (fatalities) conflict indicators.

α_t is the duration function indicating how risk depends on time (effect of age on the hazard) and is specified by breaking the hazard function into *n* categories (<5 years, 5-6 years and so on) during which the risk of the outcome is assumed constant for women with the same pattern of covariates. Exposure to the risk of teen marriage starts at age 12 for all women and ends on the date of teen marriage. Women who had not married in their teens are censored just before their 20th birthday. θ_k and λ_d are birth-year and district dummies, respectively. These control for the underlying trend in teen unions due to belonging to an older versus a younger cohort, and for time-invariant local conditions affecting marriage patterns independent of conflict. Models also adjust for residence type. Given endogeneity (women marrying earlier tend to leave school prematurely), the main models do not control for education.

Finally, I add a 'frailty' term υ_i at the individual-level, which allows for unobserved heterogeneity. This is interpreted as the residual between-women variance due to unmeasured time-invariant attributes that might influence one's 'susceptibility' to marriage, but that cannot

be accounted, e.g., women's parental education and wealth at conflict time or union characteristics like arranged/forced marriages (South 2001; Uecker and Stokes 2008; Wiik 2009). This is analogical to individual fixed effects in standard panel data models. Moreover, 'frailty' helps the correct modelling of duration dependence. Namely, it prevents a biased estimation of the coefficients due to the "premature" exit from of subjects whose omitted characteristics make them at "high-risk" of the outcome (Jenkins 1995). All regressions are estimated using sampling weights and standard errors clustered at the primary sampling unit level.

I exclude respondents married before age 12 (<1%) and those aged 19 and below at survey time because of right censoring on the outcome variable. In the main specifications, I exclude cohorts aged 19-20 in 1992 (born 1973-1972) as their treatment status is less clear-cut: some of them might have married during the conflict, but they were not exposed to violence during most of their adolescence. The USSR breakup possibly "contaminated" their marriage prospects more than the conflict itself. I address this issue in the checks. Following these restrictions, the sample comprises women aged 22-49 in 2006 (N=6,011), i.e., born between 1957-1971 and 1974-1984. Sample descriptive statistics are in Table A3 (Appendix A).

Any causal interpretation and accuracy of the estimates relies on the assumptions that trends in teen unions would have been the same across the "*War"* and "*Soviet"* cohorts in the absence of conflict, and that there were no omitted time-varying effects associated with the conflict indicators. I test the plausibility of these assumptions, including parallel trends and balance of covariates, as much as data allow in Appendix B. Lastly, it is worth emphasising again that conflict-due migration was largely involuntary and universal (all ethnic Azerbaijani in Nagorno-Karabakh/Amenia were expelled from their home territories), and displacement was the main form of internal migration during the years of turmoil (international emigration concerned mainly ethnic Russians and Armenians (Allahveranov et al. 2012; Rowland 2004;

Aliyev 2006). Return was not possible, and expellees were culturally and ethnically akin to non-movers and to residents in non-conflict areas.[9] Unfortunately, no data source allows examining mortality during the flight and related selection in survival. However, the above features should free the operationalisation of the conflict indicators and estimates from other serious selectivity issues. 'Frailty' terms correct for the selective impact of unobserved factors influencing marriage risk, and I dedicate special attention to heterogeneity by displacement status.

Results

Descriptive analyses of entry into teen marriage

Table 1 presents measures of central tendency and the cumulative probability of being married by ages 15 to 19 for women born 1957-1984. This includes women who reached their teens during more stable Soviet years (born 1957-1968), who did so partially during the first years of socio-economic instability (1969-1973) and women attaining adolescent ages almost entirely during the conflict and post-Soviet early transition period (1974-1984).

Some interesting patterns arise: first, the mean and median ages at marriage are higher for the 1957-1971 cohorts reaching teen ages in a more stable macro-economic and social environment. The decrease in measures of central tendency characterising younger cohorts seems attributable to a rising proportion of girls marrying in teen ages. For instance, the share of girls born in 1974-1977 married by age 16 and 17 was, respectively, almost 8% and 12% points higher compared to the 1969-1971 cohorts. The same proportions were about 7% and 9% larger than for women born just before (1972-1973). Very similar increases characterise the 1978-1980 cohort. This latter group shows the highest proportion of married by age 15

[9] Among observable characteristics in the analytic sample (available upon request), the only significant differences were in urban/rural residence and household wealth, with non-migrant conflict-exposed women being more likely to reside in rural areas and poorer. This is expected given that IDPs/refugees clustered in urban areas and often rely on dedicated state financial support.

(3.33%) and 16 (10.18%). Thus, there appears a pattern of earlier entry into union for the cohorts reaching teen ages in the precarious conflict and independence period.

Second, among these women, the proportion unmarried by age 20 steadily increased. For instance, while 61% of women born in 1974-1977 were still single at age 20, the share was about 4% and 11% points higher in the 1978-1980 and 1981-1984 cohorts, respectively. Seemingly, the "rush" to marry was more prevalent in older *War-cohorts* and only occurred at the youngest teen ages (15-17) for those born after 1977.

The Kaplan-Meier curves in Fig.2 describe these patterns more succinctly and with greater focus on conflict exposure. Differences between the *Soviet* and *War-cohorts* the are irrelevant until age 15 (Panel A). By age 16, though, the curves start diverging more visibly, with a slower entrance at all following ages for the *Soviet cohort*. The largest gap is between ages 17-18 (8 vs. 17%).

The faster entry into marriage of the *War-cohort*, however, is only one part of the story. Not all women born in the 1974-1984 decade were exposed to the Nagorno-Karabakh conflict. Kaplan-Meier estimates for teen marriage by overall conflict exposure show very little, if no difference between the groups (Fig.2, Panel B). Only after age 18 the curves marginally separate: women exposed to the conflict married slightly later than non-exposed.

As the above descriptions do not supply a univocal picture of the war exposure/cohort relationship, I graphically investigate trends in rates of teen marriage by birth cohort ("time") and overall conflict exposure ("treatment"). On the left-hand side of Fig.3 are rates by conflict exposure for the *Soviet cohorts*, namely women who were too old at conflict onset to have their chances of marrying in adolescence affected by violence. On the right, rates for women in the *War-cohorts* who were either exposed or not to the conflict. Trends for women in the *Soviet cohorts* with differential exposure to conflict are similar and generally move in parallel. Conversely, there is a wider divergence in the *War-cohorts*: the non-exposed have higher and

broadly stable teen marriage rates, whereas those of their conflict-affected peers follow a marked, albeit fluctuating, declining pattern. This visual inspection thus suggests a peculiarly different behaviour for women enduring conflict during adolescence as compared to both non-exposed same-age women and older ones. It also alleviates concerns linked to diverse pre-conflict marriage trends between groups differently exposed to the "treatment", and thus reinforces the logic of the modelling strategy, whose results I report next.[10]

Survival models

Table 2 shows estimates of survival models specified with a DID logic in exponentiated form (hazard ratio, HR). Coefficients greater/lower than one denote a higher/lower risk of teen union compared to the reference category. The first two columns report the results of the baseline specification without controls, except duration dependence (Col.1), and adjusted estimates (Col.2) for the main independent variable (*overall conflict exposure* indicator).

Both models reveal a significantly negative interaction between *War-cohort* and *Conflict exposure*: the risk of teen union is about 34% points lower (Col.1: HR 0.659, 95% C.I. 0.447-0.972) for exposed women born in 1974-1984 as compared to their non-exposed peers and older women (with the same unobserved characteristics). The sign and magnitude are similar when controls are included (Col.2). The minor amount of variance due to unobserved woman-level characteristics suggests that reductions for the conflict-exposed do not simply result from selection due to unobserved factors.

When I use the continuous conflict frequency and intensity measures, results confirm a significant negative association (Col.3-4). For instance, one standard deviation increase in

[10] Additional supporting information on the empirical strategy and assumption checks are in Appendix B (Tables B1–B3). Before running survival models, I performed linear probability models including an interaction between *War-cohort* and each conflict indicator on a set of dependent variables indicating the probability of marriage by each age from 15-19 on both the full sample and on samples of those at risk of each outcome by 1996. Results (not presented for space reasons) show no significant differences, except at age 19, for conflict-exposed women born 1974-1984, although the relationship is negative at most cut-offs.

district fatalities (2.5 casualties) lowers teen marriage risk by about 14% in the *War-cohort*. The continuous conflict measures have similar coefficient sizes and trajectories due to their strong correlation and can be best visualised in Fig.4. While the predicted probability of entering union in teen ages is approximately the same for women in the *War-cohort* and *Soviet cohorts* who did not experience any violence, it increases much less rapidly for the former as the number of conflict events and fatalities increases. Further, the coefficient of *conflict frequency* is positive and significant, denoting that intense violence occurred in locations with higher levels of teen marriage.

Alternative measures and robustness checks

Results are robust to various checks (Appendix B). First, I restricted the *Soviet cohort* to include only women aged 21-31 in 1992 (born 1961-1971). At the detriment of sample size, this makes this group as close, and therefore, as comparable as possible to the *War-cohort*. Estimates do not change substantively (Table B4). The coefficient size is now larger for all conflict indicators, which strengthens the finding of a negative association.

Second, I run Eq.(1) now including the 1972-1973 cohorts in the sample. Women born in 1972 were aged 20 at conflict onset, while the 1973 cohort was 19. Initially, I code both as belonging to the *Soviet cohort*. Next, I split them so that the former is assigned to the *Soviet cohort*, while the 1973 cohort to the *War-cohort*. The direction and size of the relationship remain unchanged in both specifications (Tables B5-B6). In the first model, though, the reduction is stronger ($p<0.01$) for the binary and frequency indicators, suggesting that the largest differences emerged for women aged 18 or below at the start of the full-blown war.

Third, I recoded the continuous conflict measures into three categories for "No events/fatalities", "Medium" (between one and the 95th percentile, i.e., 24 events and 5.6 fatalities) and "High" (above the 95th percentile). Estimates show that the reduction was essentially driven by medium frequency of exposure and high-intensity violence (Table B7).

Fourth, I estimated models excluding non-displaced women residing in the Ganja-Qazakh region, where a few conflict events also took place. Results are unchanged with respect to the main models, except for the conflict frequency measure (not shown). Here, the relationship is still negative, but is no longer significant (p=0.08).

Models estimated with a logit-link function, alternative specifications of duration dependence (e.g., quadratic, cubic), cut-offs for early unions (e.g., survival time to marriage from 12 to 16/18 to focus on the earliest ages at marriage), shorter conflict time-window (1992-1994/95), and including an education dummy (completed mandatory 9-years of schooling) did not yield different results.

Due to data availability, the approach this study takes is not that of a traditional DID. Hence, performing its entire battery of sensitivity tests was not feasible. However, the robustness of the findings to different specifications, thresholds and definitions shown here, and the checks presented in Tables B1–B3 are reassuring as for the validity of the main results.

Heterogeneity

1. Does length of exposure to conflict matter?

Determined the presence and sign of the relationship, the next relevant question concerns whether all conflict-affected women in the *War-cohort* experienced systematic declines, or if these were limited to specific cohorts. Reasonably, we could expect the strongest relationship for girls who spent most of their time 'at risk' of teen union under conflict, i.e., those aged 12-14 at conflict onset (16-18 at denouement). I therefore re-estimated the models using a finer cohort measure, that spells out the relationship for women born in 1974-1977 (aged 15+ in 1992), 1978-1980 (14-12) and 1981-1984 (11-8).

Results in Table 3 show a lower risk of teen union for all *War-cohort* sub-groups. Essentially though, the reduction is significant only for the hypothesised 1978-1980 cohort (HR: 0.327; 95% C.I. 0.156-0.689). No differences in risk characterise women who experienced conflict

predominantly in their late teens or childhood.[11] To further ease interpretation, Fig.5 shows predicted probabilities for each combinations of the interaction term from Col.1.

The striking different early marriage behaviour between exposed and non-exposed girls born 1978-1980 then suggests that in Azerbaijan, entry into teen unions was neither immediately manipulated by families as a response to conflict threats, nor the impact extended to cohorts attaining adolescent ages towards the later stage of the war. Rather, the negative association characterised only those experiencing conflict the longest when 'at risk'.

2. The role of forced migration

As the literature review suggested, several underlying forces could explain the lower levels of early marriage for the cohorts longest exposed to violence when teens. One is forced migration. For these girls and their families, displacement occurred precisely in ages when they would be more likely to take a decision about teen marriage and search suitable spouses. Their displacement and resultant disruption in livelihoods/social networks conceivably hindered union formation. In contrast, younger displaced girls had technically more time and relatively more stable conditions (e.g., in tent settlements with better access to social and economic assistance) to meet future grooms before actually becoming 'at risk' of teen marriage. Slightly older women in the *War-cohort* could have had their marriages already arranged before the conflict and, perhaps, sought to relocate to areas near to or with their prospective husbands. I test these hypotheses by adding an interaction between each 3-year *War-cohort* and women's conflict-related migration status, spelling out the categories of the *overall conflict exposure* indicator. Figure 6 presents predicted probabilities of teen marriage from the model (Table A4, Appendix A for full estimates).

[11] Findings were robust to other cohort groupings, e.g., single-, two- and four-years. Statistically significant negative associations effectively characterised conflict-affected women born in 1975, 1978-1980 and 1982.

The probability of teen marriage was quite low and similar across groups in the *Soviet cohorts*. Non-significant increases characterise all women aged 18-15 at conflict onset (1974-1977 cohorts), particularly non-migrants of Upper-Karabakh. Teen marriage probability dropped sharply for forcibly displaced women born in 1978-1980, whereas no significant changes mark their non-exposed counterparts. As hypothesised, the decline in the displaced group is limited to this cohort and does not "spread" onto the following one.

Notably, for non-migrants in Upper-Karabakh, there are significant reductions in the 1978-1980 cohorts that further extend to girls who began to be 'at risk' of marriage towards the end of the conflict. Their chances of becoming teen brides are close to zero. Although this finding may be the result of small cell numbers, other mechanisms may explain the peculiar behaviour of this group, e.g., a short supply of male partners. Unfortunately, similar sex-ratio factors are hard to test with available data.[12]

Limitations

While the finding of a reduction in unions is clear and robust to various measurements and checks, examining teen marriage outcomes disaggregated by conflict sub-groups and cohorts exposes the research to the estimation risks structural to small samples. Moreover, the cross-sectional character of most AZ-DHS variables, and limited access to other data sources, prevented examining many theoretically plausible mechanisms. This is regrettable especially for sex-ratio factors and known determinants of early unions such as parental/household characteristics (Kohno et al. 2020; Pesando and Abufhele 2019). The selective impact of the latter is accounted for by 'frailty' terms included in the models.

[12] Due to high politicisation of the conflict, data are insufficient to test whether a conflict-caused decline in sex-ratio in districts of Upper-Karabakh was related to the lower marriage probability of these, and other conflict-affected women. Pre-conflict (USSR) district-level census data are not available disaggregated by age, sex (and ethnicity) nor are post-independence population data. Even if post-conflict measures were possible to construct with the AZ-DHS or 1995 Living Standard Survey, these would be hardly useful in the absence of pre-conflict information on district-level age and sex structure.

Again, although the study sought to thoroughly exploit the depth and breadth of available data, it is possible that the lack of GIS cluster information and migration histories for all women created measurement errors in the conflict indicators. The use of multiple conflict measures and the fact that intense conflict occurred in Nagorno- and Upper-Karabakh should limit this concern. Relatedly, while I cannot fully exclude social desirability bias and misreporting of displacement status, my coding procedure (including as conflict-exposed women with a displaced person in her natal household), the survey aims (not linked in any way to direct refugee/IDP assistance), the question used to identify forced migrants and the generally neutral attitudes towards displaced persons in this context (UNHCR 2009) reduce concerns over status under/over-reporting (EGRIS 2018). Equally, I found no evidence of marital age displacement by conflict characteristics in the AZ-DHS (Chantler 2012). This is also attenuated by the survey question used to capture union formation.

Finally, estimates are based on a sample of survivors residing in Azerbaijan in 2006 and there is no direct way to determine whether teen marriages were underestimated because of survival bias.

Overall, results should be interpreted carefully as a first attempt at providing answers to questions on *whether* and *how* violence is associated with early unions. Future research should strive for causal assessment and expand this line of inquiry into the "*whys"*. To confirm causality and investigate specific driving pathways though further efforts in developing new tools or refining existing ones, e.g., oversampling conflict-torn populations and including conflict-sensitive questions in surveys, are inevitably required (Bruck et al. 2016).

Discussion and conclusion

Does exposure to violent conflict affect teen marriage? Existing knowledge on this paramount question either comes from qualitative research unsuited to evaluate population-level relationships, or is extrapolated from quantitative studies focusing on changes in general

marriage outcomes, not early unions (Neal et al. 2016). These latter examine a few contexts, with a narrow set of methodological approaches that often hide differences across ages, duration and type of conflict exposure. The resultant evidence is largely inconclusive, and therefore of limited assistance to policy.

This study fills this knowledge gap and provides a first empirical test of the link between war exposure and early marriage. Exploiting rare information on forced displacement, geographic and cohort variation in exposure to the Nagorno-Karabakh conflict in Azerbaijan, findings reveal that experiencing conflict violence in adolescence reduced teen unions, and effectively for girls who spent most of their teens under active violence and, among them, forced migrants. Intense and frequent exposure were also associated with lower marriage risk.

These results echo findings from those studies investigating the broader conflict/marriage nexus in settings with similar conflict typology (Khawaja and Randall 2006) and institutional framework to Azerbaijan (Shemyakina 2013). They are also in part consistent with Sieverding et al. (2020)'s descriptive observation of decreasing early marriage probabilities in Syrians displaced to Jordan. As for coefficient size, the magnitude is comparable to changes in marriage law raising the minimum marriage age in the Americas (Bharadwaj 2015; Bellés-Obrero and Lombardi 2019), but seemingly larger than weather shocks (Corno et al. 2020). Additionally, results are likely a lower-bound of the true effect given that the conflict erupted in full in 1992, but tensions emerged in the late 1980s.

A decline in teen marriage for conflict-exposed girls is a welcome and, perhaps, unexpected result considering suggestions from qualitative accounts. Some caution in interpreting and generalising this finding is though warranted as the slowdown in teen marriage coincided with an antithetic general increase in the Azerbaijani population compared to the Soviet period. This therefore subsumes two kinds of differences: one *between* the Soviet and War-cohorts; the other *within* the War-cohorts. The first likely captures the diverse socio-economic incentives

and family regimes the *Soviet* and the *War-cohorts* experienced when teens. The former lived under a system where financial stability, security, family-related services and regulations were arguably provided and enforced by the central State authority; conversely, the *War-cohorts* reached adolescence as such value, economic and legal system collapsed. For those not affected among them, early marriages reasonably represented a source of stability against these swift socio-economic setbacks, a response observed in other ex-Soviet Central Asian countries (Clifford et al. 2010; Agadjanian and Makarova 2003; Dommaraju and Agadjanian 2008). The second difference then captures the extra variation *within* the War-cohorts due to the additional insecurity generated by conflict violence. Ergo, the final result is to be understood as a combination of experiencing the conflict *as well as* the transition to a new socio-political regime.

These findings thus provide new evidence on family formation decision-making in times of violence, and in relation to different stressors and sources of insecurity. Formally testing explanatory mechanisms was not possible due to data constraints. I nonetheless sought to disentangle associations by cohort and advance some speculations on driving channels. The observation that reductions occurred essentially in a single conflict-affected cohort suggests that the ages at conflict exposure, and duration when 'at risk' of teen marriage may matter more than the experience of violence itself. As this delay was particularly pronounced for displaced girls, there is reason to think that forced migration at specific ages constituted a pathway for marriage delay. In the earliest phases of displacement, forced migrants incurred in significant unplanned and emergency expenditures, e.g., rental payments, relocation travels and paperwork fees, that, along with low income-generating opportunities, strained their economic welfare (SORGU and World Bank 1995; IDMC 2007). As a result, these families perhaps could not afford the expected wealth transfers occasioned by weddings, opted or were forced to divert their limited resources on investments other than marriages that were not required to non-exposed households (Sieverding et al. 2020).

Moreover, forced migration from Nagorno-Karabakh and Armenia separated extended households and disrupted community ties (Amnesty International 2007; UN Commission on Human Rights 1999). At least in the initial post-displacement years, this sudden social fragmentation and loss of intangible assets perhaps frustrated the search for potential spouses of displaced families and girls then "suitable" for marriages. As conditions stabilised, new networks of support, trust and norms of reciprocity between neighbours who were strangers prior to displacement possibly favoured again partner selection and the arrangement of weddings. This could partially explain the lack of impact on IDPs/refugees born after 1980.

The sharp declines in unions for the youngest non-migrant cohorts suggest comparable, but longer disruptive changes on the social fabric due to conflict. A tentative explanation, that cannot be addressed with present data, relates to imbalances in sex-ratio. Conceivably, conflict-caused high male mortality and conscription imposed structural changes to the local marriage market of Upper-Karabakh, lowering the amount of available prospective husbands (De Walque 2006). Although Shemyakina (2013) did not find any relationship between declines in marriage and sex-ratio in Tajikistan, a country that experienced conflict around the same time and with socio-cultural and institutional backgrounds comparable to Azerbaijan, similar mechanisms should not be discarded and represent an important avenue for future research.

Delaying marriages from teen to more adult ages, even by a few years, is a desirable outcome for Azerbaijan and for girls in violent contexts. This though does not exclude adverse marriage outcomes from happening just a bit later than in adolescence. In humanitarian emergencies, young men's inability to afford bride price, their conscription and excess mortality could reduce match quality, leading women to marry older or less educated men (Grabska 2012; Sommers et al. 2011). Wide spousal age and educational difference are known predictors of marital dissolution (Burazeri et al. 2005) or domestic violence (La Mattina 2017; Mabsout and Van Staveren 2010). The share of conflict-exposed born in 1974-1984 eventually marrying a

man aged 10+ years older in the AZ-DHS is more than double than older women (16 vs. 6%). Together with changing marriage timing, conflict possibly constrained women's choices via a deteriorated pool of potential husbands.

Policy intervention should consider all these aspects. As conflict-induced declines in early unions imply that young women will depend for longer on their families and/or own resources, it is critical to ensure access to learning opportunities that can make girls prospectively less reliant on future partners, or less acquiescent to unwanted marriage arrangements, which may present slightly later in their life-course. This would have broader positive spill-over and intergenerational effects. Above and beyond conflict, though, we need concerted policy and research efforts to tackle the rooted socio-cultural acceptance of unwanted early marriage and to effectively implement legal frameworks for child and adolescent protection where, as in Azerbaijan, its prevalence is high**.**

TABLES

Table 1 Cumulative probabilities of teen marriage by birth cohort (1957–1984)

3-year birth cohort	Mean marriage age	Median marriage age	Age first married (%)						*N* (weighted)
			15 and below	16 and below	17 and below	18 and below	19 and below	Not married by 20	
1957-1959	21.78	21	1.29%	3.71%	10.42%	21.89%	31.14%	68.86%	525
1960-1962	22.51	22	0.56%	1.81%	6.58%	16.53%	24.40%	75.60%	767
1963-1965	22.04	22	1.40%	1.73%	7.51%	16.77%	27.55%	72.45%	779
1966-1968	21.43	21	0.98%	2.88%	9.35%	20.46%	31.17%	68.83%	702
1969-1971	21.91	21	0.39%	1.11%	6.41%	14.71%	29.82%	70.18%	696
1972-1973	21.22	20	0.67%	1.87%	9.82%	22.41%	39.56%	60.44%	403
1974-1977	**20.79**	**20**	**3.18%**	**8.91%**	**18.36%**	**28.50%**	**38.90%**	**61.10%**	**804**
1978-1980	**20.16**	**20**	**3.33%**	**10.18%**	**16.99%**	**25.62%**	**35.33%**	**64.67%**	**665**
1981-1984	**19.68**	**20**	**2.14%**	**5.82%**	**12.84%**	**19.80%**	**27.67%**	**72.33%**	**1,073**
Obs.									6,414

Source: 2006 AZ-DHS.
Note that *N* indicates the total number of women in the sample weighted using provided sample weights. Cohorts of women who reached teen ages during the conflict years are highlighted in bold.

Table 2 Results of discrete-time clog-log models of the transition to teen marriage

	HR of teen union			
	(1)	(2)	(3)	(4)
War-cohort (1974-1984) * Conflict measure	0.659*	0.635*	0.983*	0.942*
	[0.45,0.97]	[0.43,0.94]	[0.97,0.99]	[0.88,0.98]
Overall conflict exposure (ref: not exposed)				
Exposed	1.001	1.268		
	[0.72,1.39]	[0.74,2.17]		
Conflict events			1.037*	
			[1.00,1.07]	
Conflict fatalities per 1,000				1.093
				[0.96,1.24]
District dummies	No	Yes	Yes	Yes
Year of birth dummies	No	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes
σ_u^2	1.559	1.127	1.141	1.121
N person-years	44,885	44,885	44,885	44,885

Source: 2006 AZ-DHS.

Notes: Sample consists of women born 1957–1984 (ages 22–49 in 2006), excluding women born 1972-1973. Subjects enter analysis at age 12. Columns represent hazard ratios . Robust standard errors clustered at the PSU level. The "War-cohort" includes women born 1974-1984. The binary indicator "overall conflict exposure" includes IDP/refugee women, non-migrant women residing in Karabakh and non-displaced women with at least one male member of their family of origin (or mother) who identified as IDP/refugee. All regressions control for duration since start of exposure (<5 years, 5-6 years and so on) and rural/urban residence, and include a constant not shown. Models are specified with individual-level frailty terms (σ_u^2) and are weighted using provided sampling weights. * p<0.05, ** p<0.01, *** p<0.001.

Table 3 Results of discrete-time clog-log models of the transition to teen marriage by granular cohorts

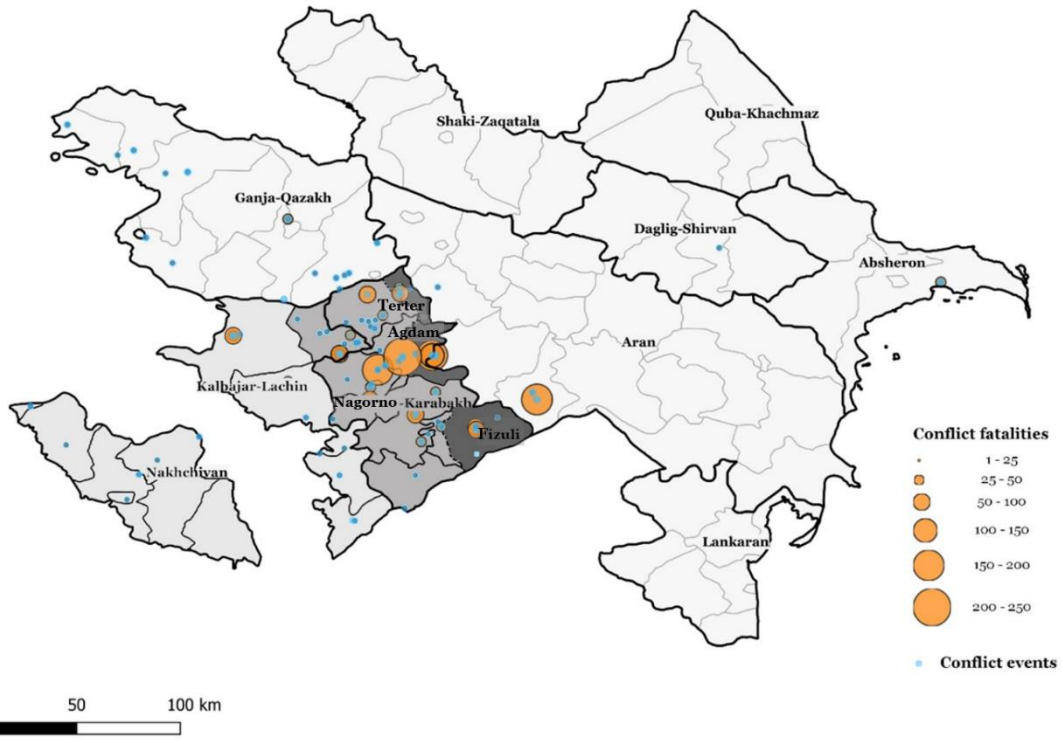
	HR of teen union		
	(1)	(2)	(3)
Conflict measure * Born in			
1974-1977	0.866	0.992	0.969
	[0.50,1.49]	[0.97,1.00]	[0.92,1.02]
1978-1980	0.327**	0.958*	0.846*
	[0.16,0.69]	[0.92,0.97]	[0.74,0.96]
1981-1984	0.668	0.986	0.958
	[0.37,1.20]	[0.96,1.01]	[0.87,1.06]
Overall conflict exposure (ref: not exposed)			
Exposed	1.246		
	[0.73,2.12]		
Conflict frequency (events)		1.037*	
		[1.00,1.07]	
Conflict intensity (fatalities per 1,000)			1.107
			[0.94,1.30]
District dummies	Yes	Yes	Yes
Year of birth dummies	Yes	Yes	Yes
Controls	Yes	Yes	Yes
σ_u^2	1.181	1.152	1.134
N person-years	44,885	44,885	44,885

Source: 2006 AZ-DHS.

Notes: Sample consists of women born 1957–1984 (ages 22–49 in 2006), excluding women born 1972-1973. Subjects enter analysis at age 12. Columns represent hazard ratios. 95% confidence intervals are in parentheses. Robust standard errors clustered at the PSU level. The "War-cohort" includes women born 1974-1984. The binary indicator "overall conflict exposure" includes IDP/refugee women, non-migrant women residing in Karabakh and non-displaced women with at least one male member of their family of origin (or mother) who identified as IDP/refugee. All regressions control for duration since start of exposure (<5 years, 5-6 years and so on) and rural/urban residence, and include a constant not shown. Models are specified with individual-level frailty terms (σ_u^2) and are weighted using provided sampling weights. * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

FIGURES

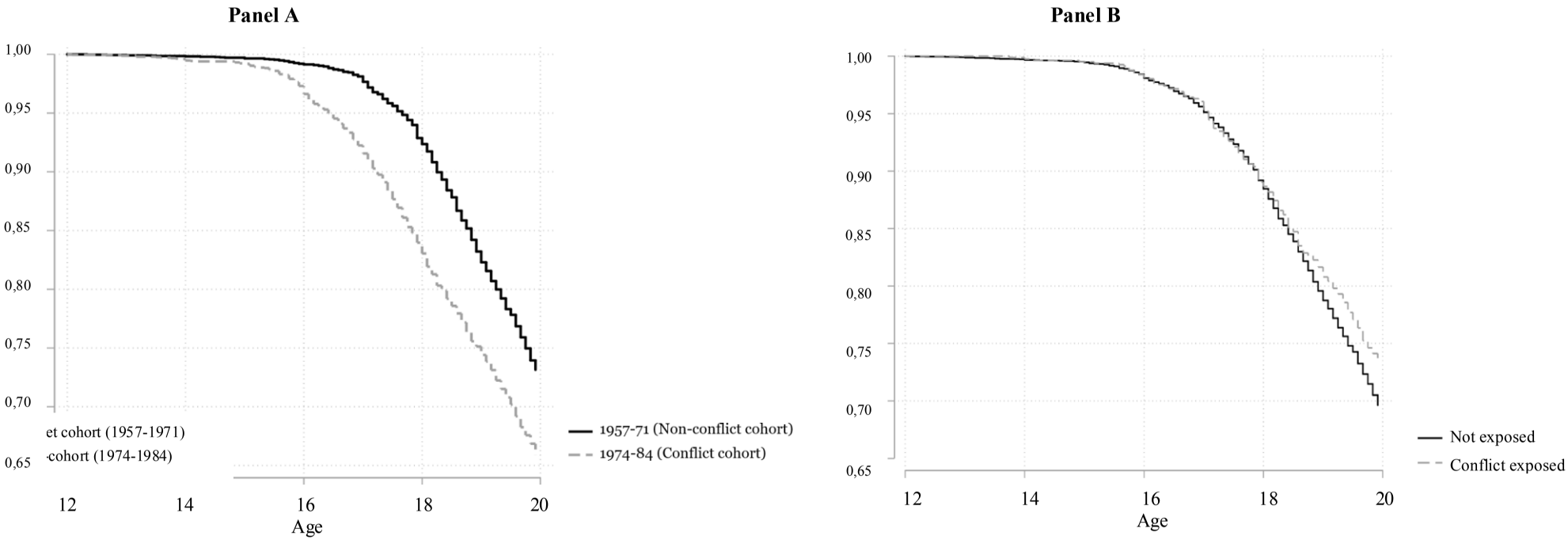
Fig 1 Map of conflict events and fatalities in Azerbaijan 1992-1996



Source: UCDP-GED (2020).

Notes: The map shows the 9 economic regions of mainland Azerbaijan (66 districts), and the exclave of Nakhchivan (7 districts). The non-sampled Nakhchivan and Kelbajar-Lachin economic regions are highlighted in light grey; the *de facto* Republic of Nagorno-Karabakh (also known as Republic of Artsakh, in 2006 under full-Armenian control and populated only by ethnic Armenians) and the sampled parts of the contested districts of Agdam, Terter and Fizuli (Upper-Karabakh) are respectively in progressively darker grey. Blue dots indicate conflict events. Larger orange dots denote increasingly high number of conflict fatalities as measured by UCDP-GED best estimate. Multiple conflict events occurred in the same location, so blue dots sometimes overlap.

Fig 2 Kaplan-Meier curves for teen marriage by cohorts (Panel A) and conflict exposure (Panel B)

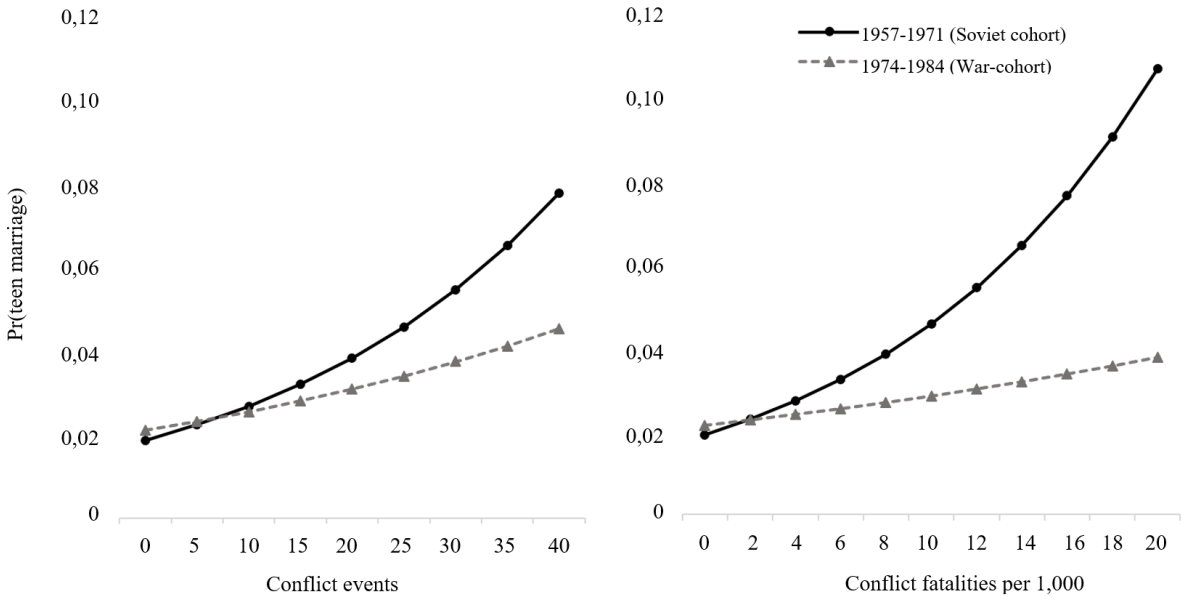


Source: 2006 AZ-DHS. Author's own calculation.

Fig 3 Trends in teen marriage by conflict status and cohorts

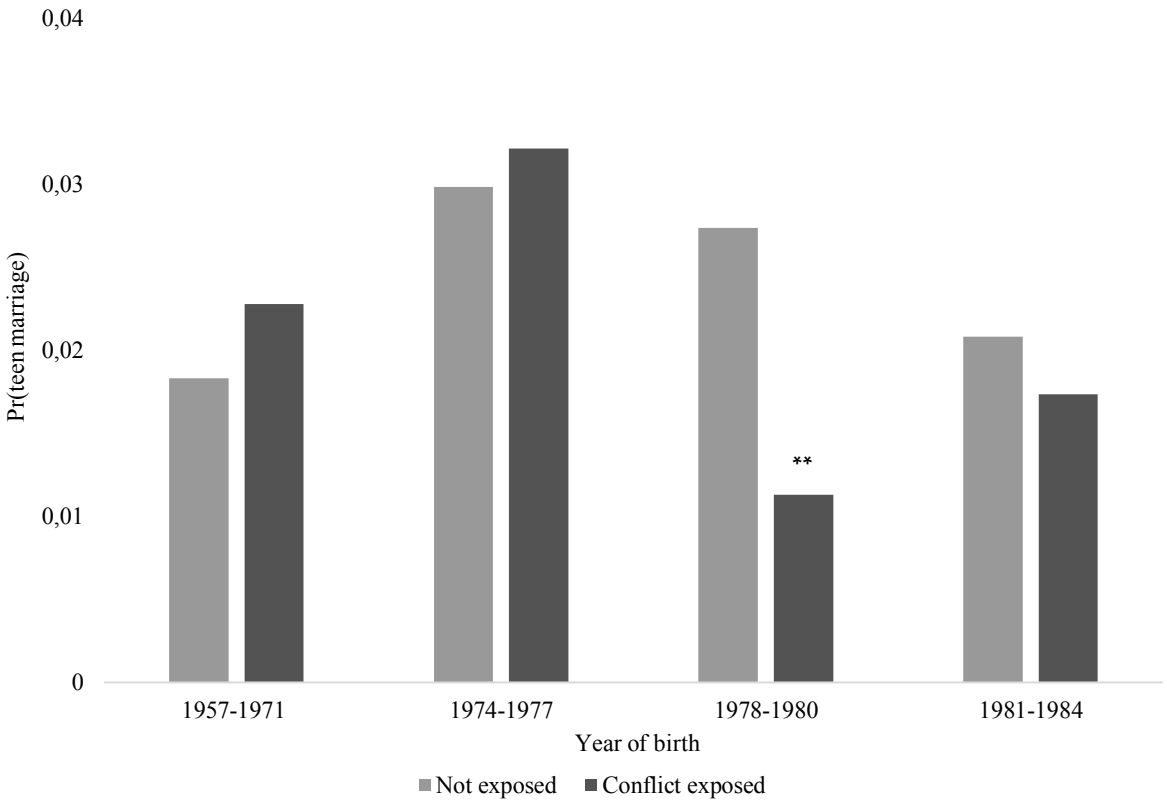
Source: 2006 AZ-DHS. Author's own calculation.

Fig 4 Predicted probabilities of teen marriage by conflict exposure frequency and intensity



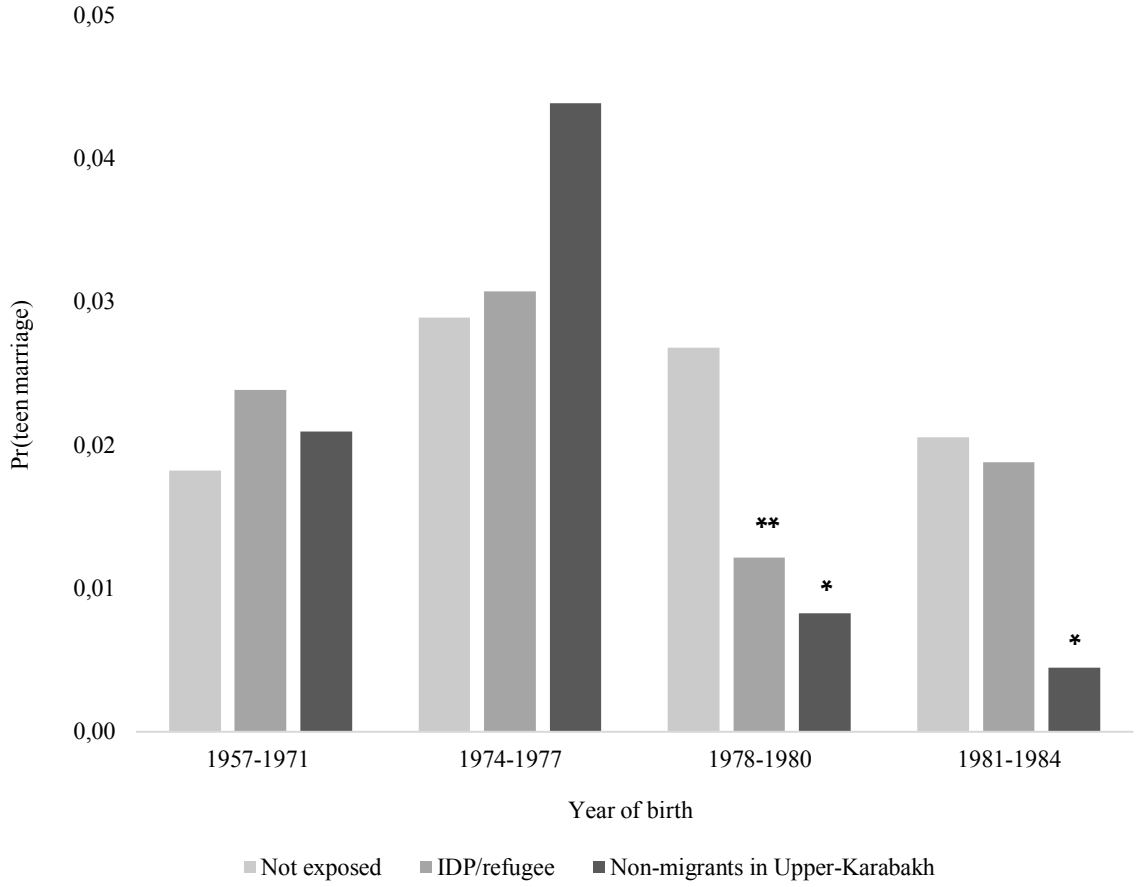
Source: As per Table 2, Column (3) and (4).

Fig 5 Predicted probabilities of teen marriage by conflict exposure and granular cohorts



Source: As per Table 3, Column (1). * p<0.05, ** p<0.01, *** p<0.001.

Fig 6 Predicted probabilities of teen marriage by conflict-related migration status and granular cohorts



Source: As per Table A4 (Appendix A), Column (1). * p<0.05, ** p<0.01, *** p<0.001.

APPENDIX A (ELECTRONIC SUPPLEMENTARY MATERIAL)

Appendix A contains supplementary material detailing (a) definitions of conflict measures used in the text, (Table A1); (b) identification of conflict cohorts (Table A2), (c) descriptive statistics of the analytical sample (Table A3), models by conflict-migration status (Table A4).

Table A1 Definitions of conflict measures

Group	*N*/% (weighted)	Binary indicator	Continous frequency indicator	Continous intensity indicator
Panel A: Conflict exposure				
IDPs from Nagorno-Karabakh; Non-refugees/IDPs with one displaced male member (or the mother, if household head) in origin family	301 5.00%	1 "Exposed to conflict"	Number of conflict events in 1992-1996 in district of origin	Number of conflict fatalities in 1992-1996 in district of origin per 1,000 population (as of the 1989 USSR Population Census figures, *omitted thereafter*)
Refugees from Armenia	168 2.79%	1 "Exposed to conflict"	Mean number of conflict events in 1992-1996 in all Nagorno-Karabakh districts	Mean number of conflict fatalities in 1992-1996 in all Nagorno-Karabakh districts per 1,000 population
Permanent residents in contested districts (including non IDP/refugees migrating there during conflict)	175 2.91%	1 "Exposed to conflict"	Mean number of conflict events in 1992 (or year of arrival for migrants)-1996 in contested districts (Agdam, Terter and Fizuli)	Mean number of conflict fatalities in 1992 (or year of arrival for migrants)-1996 in contested districts (Agdam, Terter and Fizuli) per 1,000 population
All other women	5,367 89.30%	0 "Not exposed to conflict"	None	None
Panel B: Conflict cohort				
Born 1974-1984 (aged 12-19 between 1992-1996)	2,542 42.28%	1 "War-cohort"		
Born 1957-1971 (aged 21+ in 1992)	3,469 57.72%	0 "Soviet cohort"		

Notes: In Panel A, the table first shows the different groups making up the binary overall conflict exposure indicator, their counts and relative percentages in the analytic sample. It then shows how the continuous indicators were calculated for each group, in the absence of specific information on the district of residence for some groups (e.g., for refugees from Armenia) and considering their different locations during the conflict. In Panel B, it shows cohort grouping for women who attained their teen ages between 1992-1996.

Table A2 Table of women's attained ages during conflict by birth cohort

	Age in 1992	Age in 1996	Age in 2006	Year woman attains age 12	Year woman attains age 19	Weighted *N*
1957	35	39	49	1969	1977	131
1958	34	38	48	1970	1978	195
1959	33	37	47	1971	1979	199
1960	32	36	46	1972	1980	236
1961	31	35	45	1973	1981	274
1962	30	34	44	1974	1982	257
1963	29	33	43	1975	1983	254
1964	28	32	42	1976	1984	265
1965	27	31	41	1977	1985	260
1966	26	30	40	1978	1986	250
1967	25	29	39	1979	1987	220
1968	24	28	38	1980	1988	233
1969	23	27	37	1981	1989	211
1970	22	26	36	1982	1990	250
1971	21	25	35	1983	1991	235
1972	20	24	34	1984	1992	197
1973	19	23	33	1985	1993	206
1974	**18**	22	32	**1986**	**1994**	**203**
1975	**17**	21	31	**1987**	**1995**	**185**
1976	**16**	20	30	**1988**	**1996**	**213**
1977	**15**	**19**	29	**1989**	**1997**	**203**
1978	**14**	**18**	28	**1990**	**1998**	**205**
1979	**13**	**17**	27	**1991**	**1999**	**196**
1980	**12**	**16**	26	**1992**	**2000**	**264**
1981	11	**15**	25	**1993**	**2001**	**220**
1982	10	**14**	24	**1994**	**2002**	**291**
1983	9	**13**	23	**1995**	**2003**	**305**
1984	8	**12**	22	**1996**	**2004**	**256**
Overall Total	8-35	12-39	22-49			6,414

Source: 2006 AZ-DHS.
Notes: N indicates the total number of women in the sample (including women born 1972-1973) weighted using provided sample weights. In bold are birth cohorts of women who reached teen ages (12-19) between 1992-1996 and hence were 'at risk' of teen union during the peak years of the Nagorno-Karabakh conflict.

Table A3 Descriptive statistics of the weighted sample used in the analyses

	N	Mean or %	*s.d.*	Min	Max	Person-years
Conflict frequency (events)	6,011	1.97	6.69	0	73	44,885
Conflict intensity (fatalities per 1,000)	6,011	0.56	2.48	0	66.38	44,885
Age	6,011	35.08	8.36	21	49	44,885
Conflict status						
Non-exposed	5,367	89.30%				40,062
Exposed	644	10.70%				4,823
Cohort						
1957-1959	525	8.73%				3,921
1960-1962	767	12.76%				5,857
1963-1965	779	12.95%				5,901
1966-1968	702	11.69%				5,296
1969-1971	696	11.59%				5,309
1974-1977	804	13.37%				5,787
1978-1980	665	11.06%				4,827
1981-1984	1,073	17.85%				7,987
Conflict cohort						
1957-1971	3,469	57.72%				26,280
1974-1984	2,542	42.28%				18,605
Residence type						
Rural	2,599	43.75%				19,205
Urban	3,412	56.25%				25,680
Married in teen ages						
Yes	1,846	30.71%				11,572
No	4,165	69.29%				33,313
Married by 15						
Yes	104	1.69%				-
No	5,907	98.31%				-
Married by 16						
Yes	280	4.64%				-
No	5,731	95.36%				-
Married by 17						
Yes	675	11.19%				-
No	5,336	88.81%				-
Married by 18						
Yes	1,229	20.32%				-
No	4,782	79.68%				-

Source: 2006 AZ-DHS.
Notes: All indicators are presented using provided sample weights.

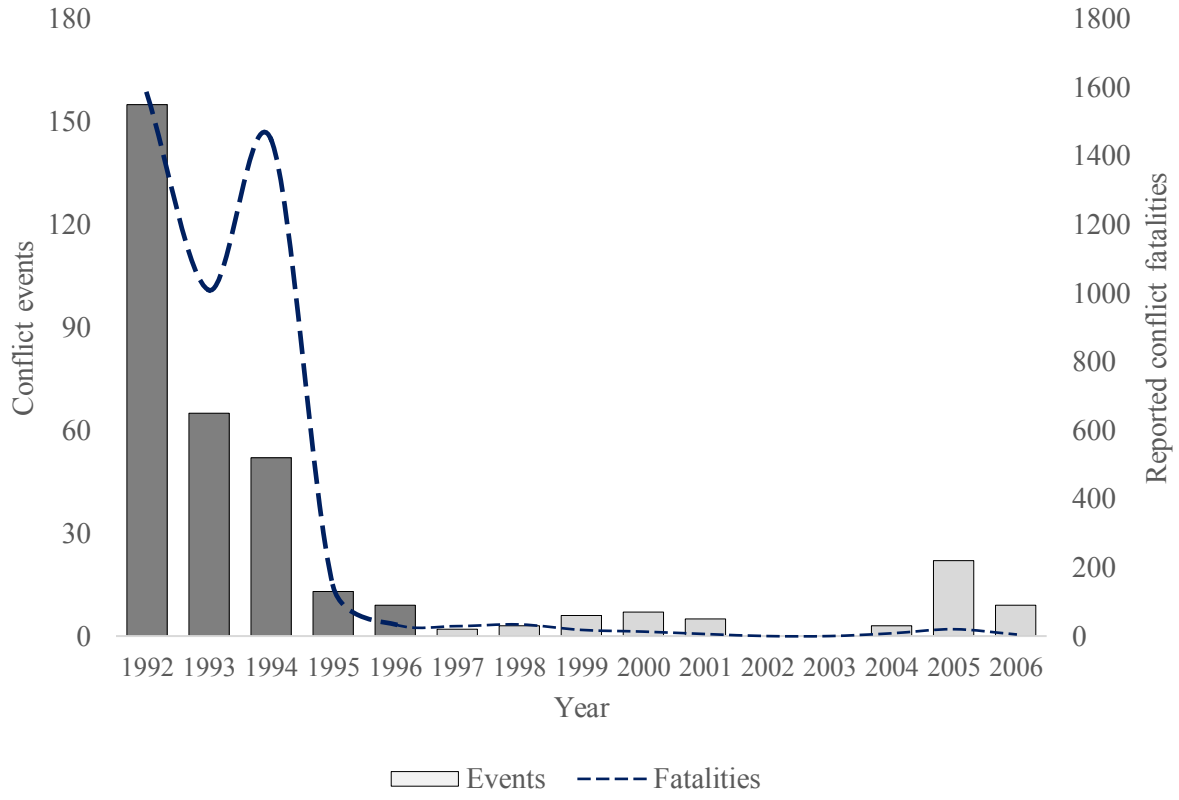
Table A4 Results of discrete-time clog-log models of the transition to teen marriage with granular cohorts and migration status

	HR of teen union
	(1)
Conflict migration status * Born in	
IDP/refugee *	
1974-1977	0.810
	[0.44,1.49]
1978-1980	0.331**
	[0.15,0.73]
1981-1984	0.693
	[0.37,1.28]
Non-migrant in Upper-Karabakh *	
1974-1977	1.362
	[0.55,3.37]
1978-1980	0.256*
	[0.25,0.98]
1981-1984	0.182*
	[0.04,0.87]
Overall conflict exposure (ref: not exposed)	
IDP/refugee	1.323
	[0.693,2.52]
Karabakh non-migrant	1.163
	[0.72,1.88]
District dummies	Yes
Year of birth dummies	Yes
Controls	Yes
σ_u^2	1.169
N person-years	44,885

Source: 2006 AZ-DHS.

Notes: Sample consists of women born during 1957–1984 (ages 22–49 in 2006), excluding women born 1972-1973. Subjects enter analysis at age 12. Columns represent hazard ratios. 95% confidence intervals are in parentheses. Robust standard errors clustered at the PSU level. The "War-cohort" includes women born 1974-1984. Non-migrants in Upper-Karabakh include women who always resided (or migrated pre-conflict or during conflict) in the Azerbaijani controlled parts of the contested districts of Agdam, Fizuli and Terter. All regressions control for duration since start of exposure (<5 years, 5-6 years and so on) and rural/urban residence, and include a constant not shown. Models are specified with individual-level frailty terms (σ_u^2) and are weighted using provided sampling weights. * p<0.05, ** p<0.01, *** p<0.001.

Fig A1 Yearly conflict events and fatalities, Azerbaijan 1992-2006



Source*:* UCDP-GED (2021).

Notes: Darker bars and thicker dashed line highlight events and fatalities in peak conflict years. The y-axis on left-hand side refers to conflict events, the y-axis on the right-hand side to the number of reported fatalities.

APPENDIX B (ELECTRONIC SUPPLEMENTARY MATERIAL)

Appendix B contains supplementary material detailing (a) assumption checks in support to the difference-in-difference logic applied in the analyses, including balance of covariates and parallel trends (Tables B1-B3); (b) results from alternative model specifications, including alternative cohort grouping (Tables B4-B6) and conflict measures (Table B7).

Preliminary checks

1. Balance in War- and Soviet-cohort characteristics

A preliminary concern is the possibility that older and younger cohorts differ systematically and in relation to the conflict (selection into treatment). One way of testing this would be analysing whether the origin households of slightly younger and slightly older women were similar in terms of various characteristics, including conflict exposure. Unfortunately, the AZ-DHS offers limited information on women's natal household and does so only for those – whether married or not – who were still living with their origin families at survey time. These are predominantly the youngest respondents. Alternatively, one can look at whether all younger and older women are similar at least in terms of observable characteristics. I follow both approaches for completeness. Table B1 shows the distribution or means of several observable characteristics for women in the *Soviet* vs. *War-cohorts*, while Table B2 for women still living with their origin household (*N*=1,274).

There is substantial balance across covariates between the *Soviet cohort* and the *War-cohort*, regardless of whether they still live with their origin family. In Table B1, the only significant difference is observed in the age variable which is expected by default. This strengthens the interpretability of results. In Table B2 (women still residing with their natal household in 2006), the only other notable difference is in household wealth, which favours the *War-cohort*. This might suggest that girls in the *War-cohort* were somehow wealthier than older cohorts and thus, if affected by the conflict, their families could afford keeping them within the household rather

than “cashing them in” to prospective husbands. However, these covariates were all measured in 2006 and there is no way to know whether responses for characteristics like wealth changed over time and compared to the conflict years. Furthermore, women still residing with their origin family at survey time, especially older ones, are likely to be a selected group and hence not necessarily displaying an accurate pattern. For these reasons, I do not control for household wealth in the main models. Even when included, results do not change substantively in all model specifications.

Table B1 Descriptive statistics of Soviet and War-cohorts

	Cohort				
	1957-1971 ("Soviet")		1974-1984 ("War")		
	N	Mean or %	*N*	Mean or %	*p-value*
Conflict frequency (events)	3,469	1.96	2,542	1.99	
Conflict intensity (fatalities per 1,000)	3,469	0.54	2,542	0.59	
Overall conflict exposure					
Non-exposed	3,107	88.56%	2,260	88.94%	
Exposed	362	10.44%	282	11.06%	
Age	3,469	41.48	2,542	26.35	***
Years of education	3,469	10.88	2,542	11.00	
Household wealth					
Poor	1,336	38.50%	947	37.26%	
Middle	674	19.44%	534	21.00%	
Rich	1,459	42.06%	1,061	41.74%	
Residence type					
Rural	1,497	43.17%	1,103	43.36%	
Urban	1,972	56.83%	1,439	56.64%	
Ethnicity					
Azerbaijani	3,256	93.85%	2,397	94.03%	
Talish	67	1.92%	40	1.78%	
Russian	22	0.63%	5	0.45%	
Other	124	3.60%	100	3.74%	
Religion					
Muslim	3,435	99.01%	2,531	99.24%	
Other	34	0.99%	11	0.76%	
Married in teen ages					
Yes	2,478	14.33%	855	24.38%	***
No	991	85.67%	1,687	75.62%	
Married by 15					
Yes	32	0.90%	73	2.85%	***
No	3,437	99.10%	2,469	97.15%	
Married by 16					
Yes	76	2.16%	206	8.05%	***
No	3,395	97.84%	2,336	91.95%	
Married by 17					
Yes	274	7.90%	401	15.76%	***
No	3,195	92.10%	2,141	84.24%	
Married by 18					
Yes	618	17.82%	611	24.00%	***
No	2,851	82.18%	1,931	76.00%	

Source: 2006 AZ-DHS.
Notes: All indicators are presented using provided sample weights. * p<0.05, ** p<0.01, *** p<0.001.

Table B2 Descriptive characteristics of women of Soviet and War-cohorts living with their origin households in 2006

	Cohort				
	1957-1971 ("Soviet")		1974-1984 ("War")		
	N	Mean or %	*N*	Mean or %	*p-value*
Conflict frequency (events)	424	2.09	850	1.74	
Conflict intensity (fatalities per 1,000)	424	0.57	850	0.53	
Overall conflict exposure					
Non-exposed	377	88.82%	761	89.53%	
Exposed	47	11.18%	89	10.47%	
Age	424	39.78	850	25.31	***
Years of education	424	10.71	850	11.51	
Number of household members	424	5.52	850	5.37	
Household wealth					
Poor	204	48.06%	284	33.43%	
Middle	73	17.23%	173	20.38%	**
Rich	147	34.71%	393	46.18%	
Residence type					
Rural	219	48.44%	499	41.20%	
Urban	205	51.56%	351	58.80%	
Ethnicity					
Azerbaijani	395	92.96%	802	94.32%	
Talish	7	1.70%	12	1.43%	
Russian	8	1.90%	2	0.20%	
Other	14	3.44%	34	4.05%	
Religion					
Muslim	415	97.86%	845	99.38%	
Other	9	2.14%	5	0.62%	
Married in teen ages					
Yes	39	9.14%	74	8.76%	
No	385	90.86%	776	91.24%	
Married by 15					
Yes	3	0.71%	10	1.13%	
No	421	99.29%	840	98.87%	
Married by 16					
Yes	6	1.48%	30	3.58%	
No	418	98.52%	820	96.42%	
Married by 17					
Yes	9	2.10%	45	5.23%	*
No	415	97.90%	805	94.77%	
Married by 18					
Yes	19	4.44%	57	6.66%	
No	405	95.56%	793	93.34%	

Source: 2006 AZ-DHS.
Notes: All indicators are presented using provided sample weights. * p<0.05, ** p<0.01, *** p<0.001

2. Parallel trends

Figure 3 in the main text provides suggestive evidence that trends in teen marriage would have been the same for exposed and non-exposed in the absence of conflict. As an additional test, I follow Valente (2011) and estimate Equation (1) replacing the $War\text{-}Cohort_{ik}$ indicator in the interaction term with a set of dummies for the five youngest cohorts who, due to their year of birth, could not have had their teen marriage patterns affected by conflict ($1967 \leq k \leq 1971$). These models are then estimated on the sample of the oldest ten *Soviet cohorts* i.e., women born between 1961-1971.[a] The interaction terms between each of the five youngest cohorts and each conflict indicators are not jointly nor individually significant, suggesting no systematic differences in pre-conflict early marriage trends between groups with *future* different exposure to the conflict. Additionally, these results help dismissing the possibility that the main findings are driven by events occurred before conflict onset. Table B3 reports the full results of this "placebo" experiment.

[a] Models run on the full counterfactual sample (born 1957-1971) yielded similar estimates.

Table B3 Test for pre-conflict difference in early marriage

Sample: Born 1961-1971

	Overall conflict exposure	Conflict frequency (events)	Conflict intensity (fatalities per 1,000)
Age in 1992 * Conflict measure			
25 (born 1967) * Conflict measure	0.97 [0.41,2.30]	1.01 [0.97,1.05]	1.01 [0.87,1.18]
24 (born 1968) * Conflict measure	0.85 [0.29,2.47]	0.99 [0.94,1.04]	0.96 [0.79,1.16]
23 (born 1969) * Conflict measure	0.84 [0.29,2.43]	0.99 [0.95,1.05]	0.98 [0.82,1.17]
22 (born 1970) * Conflict measure	1.68 [0.72,3.93]	1.02 [0.98,1.06]	1.08 [0.94,1.24]
21 (born 1971) * Conflict measure	0.58 [0.22,1.56]	0.97 [0.94,1.04]	0.95 [0.80,1.14]
District dummies[b]	Yes	Yes	Yes
Year of birth dummies	Yes	Yes	Yes
***N* Person-years**	20,597	20,597	20,597
F-test pre-conflict trend difference	0.658	0.720	0.836

Source: 2006 AZ-DHS.

Notes: Sample consists of women born 1961–1971 (ages 35–45 in 2006 and 21–31 at the start of the conflict in 1992). Columns represent hazard ratios. 95% confidence intervals are in parentheses. Robust standard errors clustered at PSU level are in parentheses. Reference category for the cohort measure is "Born in 1961-1966". The binary indicator "overall conflict exposure" includes IDP/refugee women, non-migrant women residing in Karabakh and non-displaced women with at least one member of their family of origin who identified as IDP/refugee. All regressions are specified with frailty terms (σ_u^2) at the individual level. Models control for duration since start of exposure (<5 years, 5-6 years and so on) and rural/urban residence, and include a constant not shown. Subjects enter analysis at age 12. * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

[b] Models estimated without district dummies yielded similar estimates (available upon request).

Table B4 Results of discrete-time clog-log models of the transition to teen marriage using alternative (restricted) control group

	HR of teen union		
	(1)	(2)	(3)
War-cohort (1974-1984) * Conflict measure	0.565*	0.979*	0.931*
	[0.37,0.87]	[0.96,0.99]	[0.87,0.99]
Overall conflict exposure (ref: not exposed)			
Exposed	1.465		
	[0.81,2.63]		
Conflict frequency (events)		1.037*	
		[1.01.1.07]	
Conflict intensity (fatalities per 1,000)			1.091
			[0.94,1.27]
District dummies	Yes	Yes	Yes
Year of birth dummies	Yes	Yes	Yes
Controls	Yes	Yes	Yes
σ_u^2	1.100	1.104	0.740
N person-years	38,934	38,934	38,934

Source: 2006 AZ-DHS.

Notes: Sample consists of women born 1961–1984 (ages 22–45 in 2006), excluding women born 1972-1973. Subjects enter analysis at age 12. Columns represent hazard ratios. 95% confidence intervals are in parentheses. Robust standard errors clustered at the PSU level. The "War-cohort" includes women born 1974-1984. The binary indicator "overall conflict exposure" includes IDP/refugee women, non-migrant women residing in Karabakh and non-displaced women with at least one member of their family of origin who identified as IDP/refugee. All regressions control for duration since start of exposure (<5 years, 5-6 years and so on) and rural/urban residence, and include a constant not shown. Models are specified with individual-level frailty terms (σ_u^2) and are weighted using provided sampling weights. * p<0.05, ** p<0.01, *** p<0.001.

Table B5 Results of discrete-time clog-log models of the transition to teen marriage including 1972-1973 cohorts (both coded as 'Soviet cohorts')

	HR of teen union		
	(1)	(2)	(3)
War-cohort (1974-1984) * Conflict measure	0.560**	0.979**	0.928*
	[0.55,0.81]	[0.96,0.98]	[0.87,0.99]
Overall conflict exposure (ref: not exposed)			
Exposed	1.333		
	[0.81,2.20]		
Conflict frequency (events)		1.037*	
		[1.01,1.07]	
Conflict intensity (fatalities per 1,000)			1.104
			[0.98,1.24]
District dummies	Yes	Yes	Yes
Year of birth dummies	Yes	Yes	Yes
Controls	Yes	Yes	Yes
σ_u^2	0.796	1.125	0.668
N person-years	47,960	47,960	47,960

Source: 2006 AZ-DHS.

Notes: Sample consists of women born 1957–1984 (ages 22–49 in 2006), including women born 1972-1973. Subjects enter analysis at age 12. Columns represent hazard ratios. 95% confidence intervals are in parentheses. Robust standard errors clustered at the PSU level. The "War-cohort" includes women born 1974-1984. The binary indicator "overall conflict exposure" includes IDP/refugee women, non-migrant women residing in Karabakh and non-displaced women with at least one member of their family of origin who identified as IDP/refugee. All regressions control for duration since start of exposure (<5 years, 5-6 years and so on) and rural/urban residence, and include a constant not shown. Models are specified with individual-level frailty terms (σ_u^2) and are weighted using provided sampling weights. * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Table B6 Results of discrete-time clog-log models of the transition to teen marriage including 1972-1973 cohorts (1973 coded as 'War-cohort')

	HR of teen union		
	(1)	(2)	(3)
War-cohort (1973-1984) * Conflict measure	0.690*	0.985*	0.948*
	[0.47,0.98]	[0.97,0.99]	[0.89,0.99]
Overall conflict exposure (ref: not exposed)			
Exposed	1.243		
	[0.75,2.05]		
Conflict frequency (events)		1.035*	
		[1.01,1.07]	
Conflict intensity (fatalities per 1,000)			1.098
			[0.98,1.23]
District dummies	Yes	Yes	Yes
Year of birth dummies	Yes	Yes	Yes
Controls	Yes	Yes	Yes
σ_u^2	1.127	1.144	1.093
N person-years	47,960	47,960	47,960

Source: 2006 AZ-DHS.

Notes: Sample consists of women born during 1957–1984 (ages 22–49 in 2006), including women born 1972-1973. Subjects enter analysis at age 12. Columns represent hazard ratios. 95% confidence intervals are in parentheses. Robust standard errors clustered at the PSU level. The "War-cohort" includes women born 1974-1984. The binary indicator "overall conflict exposure" includes IDP/refugee women, non-migrant women residing in Karabakh and non-displaced women with at least one member of their family of origin who identified as IDP/refugee. All regressions control for duration since start of exposure (<5 years, 5-6 years and so on) and rural/urban residence, and include a constant not shown. Models are specified with individual-level frailty terms (σ_u^2) and are weighted using provided sampling weights. * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Table B7 Results of discrete-time clog-log models of the transition to teen marriage using categorical conflict frequency and intensity indicators

	HR of teen union	
	(1)	(2)
War-cohort (1974-1984) * Conflict frequency (events)		
Medium (<25)	0.641* [0.41,0.98]	
High (25+)	0.619 [0.25,1.50]	
Number of conflict events		
Medium (<25)	1.571 [0.74,3.34]	
High (25+)	1.006 [0.37,2.73]	
War-cohort (1974-1984) * Conflict frequency (fatalities per 1,000)		
Medium (<6 per 1,000)		0.544* [0.28,0.94]
High (6+ per 1,000)		0.852* [0.49,0.97]
Number of conflict fatalities		
Medium		0.883 [0.53,1.92]
High		1.836 [0.98,3.45]
District dummies	Yes	Yes
Year of birth dummies	Yes	Yes
Controls	Yes	Yes
σ_u^2	1.346	1.237
N person-years	44,885	44,885

Source: 2006 AZ-DHS.

Notes: Reference categories: "No conflict events", "No conflict fatalities". Medium" (between 1 and the 95th percentile) and "High" (above the 95th percentile). Sample consists of women born during 1957–1984 (ages 22–49 in 2006), excluding women born 1972-1973. Subjects enter analysis at age 12. Columns represent hazard ratios. 95% confidence intervals are in parentheses. Robust standard errors clustered at the PSU level. Conflict exposure and War-cohort variables as per Data and measures section. All regressions control for dummies since start of exposure (<5 years, 5-6 years and so on) and rural/urban residence, and include a constant not shown. Models are specified with individual-level frailty terms (σu2) and are weighted using provided sampling weights. * p<0.05, ** p<0.01, *** p<0.001.

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