The Violent Legacy of Victimization: Post-Conflict Evidence on Asylum Seekers, Crimes and Public Policy in Switzerland *

Mathieu Couttenier† Veronica Preotu‡ Dominic Rohner§ Mathias Thoenig¶

February 14, 2017

Abstract

We study empirically how past exposure to conflict in origin countries makes migrants more violent prone in their host country, focusing on asylum seekers in Switzerland. We exploit a novel and unique dataset on all crimes reported in Switzerland by nationalities of perpetrators *and* victims over 2009-2012. Our baseline result is that cohorts exposed to civil conflicts/mass killings during childhood are 40 percent more prone to violent crimes than the average cohort. We exploit cross-region heterogeneity in public policies within Switzerland to document which integration policies are able to mitigate the detrimental effect of past conflict exposure on violent criminality.

Keywords: Violent Crime, Persistence of Violence, Civil Conflict, Mass Killing, Migration, Refugees.

JEL Classification: D74, F22, K42, Z18.

*We thank Markus Hersche, Laurenz Baertsch and Timothy Schaffer for excellent research assistance. Helpful comments from Yann Algan, Nicolas Berman, Marius Brühlhart, Matteo Cervellati, Paola Conconi, Quoc-Anh Do, Denise Efionayi-Mäder, John Huber, Erzo Luttmer, Thierry Mayer, Maria Petrova, Nicolas van de Sijpe, Uwe Sunde, Jean-Claude Thoenig, and Katia Zhuravskaya, as well as participants to seminars and conferences at NBER SI political economy group, 4th TEMPO CEPR conference in Nottingham, Warwick/Princeton Political Economy Conference in Venice, Political Economy Workshop at NYU, Barcelona GSE Summer Forum, EEA Annual meeting in Geneva, LIEPP/Sciences Po/HEC Lausanne workshop in Lausanne, Bologna, Lausanne, Paris School of Economics, Autonoma Barcelona, Lucerne, Cergy, Montpellier, Graduate Institute Geneva, Lille, Paris 1, Oxford, Bath, Sheffield and Paris Dauphine are gratefully acknowledged. Mathieu Couttenier and Mathias Thoenig acknowledge financial support from the ERC Starting Grant GRIEVANCES-313327, and Dominic Rohner gratefully acknowledged funding from the SNF-grant "The Economics of Ethnic Conflict" (100017-150159 / 1). We kindly thank as well Anne-Corinne Vollenweider and Philippe Hayoz from Swiss Federal Statistics Office and Beat Friedli, Veronika Moser, Pierre-Yves Dubois and Simon Sieber of the Swiss Federal Office for Migration for sharing their exhaustive data on crime and asylum seekers with us, and Nicole Wichmann and various cantonal authorities for providing information on cantonal integration policies.

†University of Geneva, mathieu.couttenier@unige.ch
‡University of Geneva, veronica.preotu@unige.ch
§University of Lausanne and CEPR, dominic.rohner@unil.ch
¶University of Lausanne and CEPR, mathias.thoenig@unil.ch

1 Introduction

Violence breeds violence. Political violence is often persistent and wars tend to recur,[1] and there is much anecdotal evidence that exposure to a conflict context makes people more violence prone. Various mechanisms explain why people tend to reproduce violence when they are haunted by the fact of either having perpetrated or witnessed violence in the past – psychological trauma, a collapse of trust and moral values, or economic deprivation, to name a few. Beyond case studies and anecdotes, it turns out that the identification of a causal impact of past exposure to conflict on future proneness to violence and unlawful behavior is challenging. The reason is simple: In most cases people remain in the same environment that made war break out in the first place, which makes it hard to isolate the individual effects of war exposure from the impact of the surroundings (e.g. weak institutions, natural resource abundance or ethnic cleavages). This lack of systematic evidence is worrying, as the persistence of violence and crime, and the vicious cycles leading to war recurrence are key issues in development economics, and are of foremost importance for post-conflict reconstruction.

In this paper we analyze empirically whether the past exposure to conflict in origin countries makes migrants more violence prone in their host country, focusing on asylum seekers in Switzerland. Studying crimes committed by migrants is of course subject to methodological challenges, as a higher crime propensity of migrants with past conflict exposure could be driven by various confounding factors. First, the context of the destination country (here, Switzerland) could bias the results due to spatial sorting of crime prone individuals who may self-select into crime-facilitating environments (e.g. deprived areas with a restricted social network and low labor market opportunities). Second, one has to deal with the issue of the selection into migration of particular population groups (e.g. over-representation of genocide perpetrators among migrants). Third, pre-conflict slow moving characteristics of the home country could co-determine crime-proneness and war outbreaks (e.g. poverty, culture of violence, low social capital).

Several institutional features make Switzerland an ideal laboratory to tackle these methodological issues. In particular, we exploit the fact that asylum seekers are exogenously assigned to (and forced to reside in) one of the 26 Swiss administrative regions (i.e. *cantons*) following a distribution key that allocates quotas based on canton population size only and not on migrants' characteristics. We also make use of an original and exhaustive dataset on violent crimes in Switzerland over the 2009-2012 period that has the crucial feature of documenting the nationalities of perpetrators. We combine this information with a new and fine-grained dataset on all asylum seekers living in Switzerland during the same period and we estimate a crime regression at the cohort level. Controlling for unobserved heterogeneity thanks to a battery of fixed effects (i.e. age, gender, nationality $\times$ year), our main source of identification corresponds to variations in crime-propensities across co-

[1] Civil conflicts are persistent: 68 percent of all war outbreaks took place in countries where multiple conflicts were recorded (Collier and Hoeffler, 2004). DeRouen and Bercovitch (2008) document that more than three quarters of all civil wars stem from enduring rivalries. Many studies find that past wars are strong predictors of future wars (see, e.g., Walter, 2004; Quinn et al., 2007; Collier et al., 2009; and Besley and Reynal-Querol, 2014).

horts from the same nationality and migration wave, with different exposures to civil conflicts and mass killings (i.e. born before/after). For the sake of causal identification, ruling out self-selection into conflict exposure is also important. With this respect, our data allows us to isolate one group that was *not* on the perpetrators' side: Cohorts who were children in wartime.

Our baseline result is that cohorts exposed to civil conflicts/mass killings during childhood (below 12 years old) are 40 percent more prone to violent crimes than the average cohort. This violence premium is stable through the lifecycle, is present both for civil conflict and mass killing exposure, and is attenuated i) for women; ii) for property crimes; and iii) for low-intensity conflicts. Our findings are robust to alternative estimation techniques, alternative disaggregation levels and an alternative victimization variable. We also check external validity using the full sample of economic migrants in Switzerland (roughly one fifth of the total population). The effect remains strong and statistically significant: For economic migrants, the violence premium of past conflict exposure during childhood amounts to 36 percent.

The crime effect of past conflict exposure that we detect at the cohort level encompasses, among others, direct and indirect forms of victimization at the individual level, such as being personally targeted by acts of violence (e.g. being injured or witnessing the killing of a family member), being exposed to a war context with prevailing economic deprivation and social capital depletion (e.g. growing up in poverty without access to adequate schooling, collapse of moral values). Conflict-induced compositional effects at the population-level may also drive part of the results (e.g. only the physically strongest or most aggressive may survive the war and migrate). Our baseline analysis does not discriminate between any of the above channels and we believe them all to be potentially important and part of the effect we are interested in. Indeed, from the perspective of a receiver country, but also in a post-war reconstruction context, all these possible facets of the violent legacy of victimization are highly policy-relevant.

To further refine the analysis and restrict the array of potential channels, we exploit information on the nationalities of both perpetrators and victims. We estimate a *bilateral crime regression* that documents the propensity of cohorts of a given nationality to target victims from specific nationalities. Crucially, this makes possible the inclusion of cohort fixed effects, resulting in the causal inference being purely based on bilateral characteristics –an approach grounded in the gravity trade literature. All cohort-specific unobserved heterogeneity being filtered-out, most of the channels listed in the previous paragraph are muted. The results show that the over-propensity to target victims from their own nationality is more than doubled for cohorts exposed to conflict during childhood. This finding is consistent with theories of war recurrence stressing the role of persistence in the destruction of social ties and in intra-national hostility.

Finally we exploit the fact that Switzerland is a federal state with large variations in institutions and public policies across its 26 cantons. Our question of interest is whether there exists some set of integration policies that can mitigate the risk of increased criminality for conflict exposed individuals. Our main finding is that fostering perspectives for labor market integration of asylum seekers can eliminate the effect of conflict exposure. In particular, the unlimited opportunity to apply rapidly for jobs in all sectors, the promotion of labor market access and the provision of measures such as coaching training and internships is able to eliminate the crime inducing impact of conflict exposure. We also find that the offer of social integration measures such as language and civic education courses is a strong rampart against the risk of conflict exposure boosting future crime propensity.

Note that due to the absence of a randomization scheme in the implementation of policies at the canton-level, our exercise of policy evaluation can barely go beyond correlations. Though limited, this preliminary evidence is, to our best knowledge, new to the literature and fills a gap by documenting how public policies can tackle the recurrence of violence in the aftermath of conflict. Besides being of academic interest, the question of what factors could make immigrants crime prone is also of big societal importance. In many developed Western countries this topic fuels heated and politically loaded debates, triggering the rise of populist parties. In this respect, one policy relevant conclusion of the current paper is that the crime risk of asylum seekers with conflict background can be very strongly reduced by putting in place public policies that offer opportunities, and at the same time get the incentives right for law-abiding behavior.

The remainder of the paper is organized as follows. Section 2 contains the review of the related literature, and section 3 presents the data. Section 4 explains our identification strategy, deals with the exogenous allocation of asylum seekers in Switzerland, and displays our baseline results, as well as a battery of robustness checks. Bilateral crime regressions documenting violence toward specific nationalities are studied in section 5. Section 6 analyzes the role of public policies and Section 7 is concerned with external validity and applies the analysis to the much larger group of economic migrants. Finally, Section 8 concludes.

2 Literature Review

Since the pioneering work of Becker (1968), the literature on the economics of crime has studied a variety of salient covariates of criminal behavior[2], but the nexus between migration and crime has only received limited attention. Notable exceptions are the papers by Bianchi et al. (2012) who study the relationship between immigration and crime across Italian provinces, by Bell et al. (2013) who study the impact of two waves of immigrants to the UK, and by Butcher and Piehl (1998) who study whether the proportion of immigrants who choose to move to particular US cities affects crime rates. However, in these countries migrants are able to self-select their location, and the available data is much less fine-grained than in Switzerland.

Also the literature on the effect of war experience has grown in recent years. On the theoretical front, Rohner et al. (2013) build a model of vicious cycles of war experience leading to low inter-group trust and hence less inter-group interactions, which in turns results in a higher likelihood of

[2]Prominent topics in this literature include the role of police activity (Levitt, 1997; Kelly, 2000; Di Tella and Schargrodsky, 2004; Draca et al., 2011) the impact of poverty and inequality (Kelly, 2000; Fajnzylber et al., 2002), the effects of unemployment and recessions (Öster and Agell, 2007; Fajnzylber et al., 2002; Fougère et al., 2009), the impact of mineral discoveries (Couttenier et al., 2014) and the role of illegal drugs (Grogger and Willis, 2000) and urbanization (Glaeser and Sacerdote, 1999).

future violence. There is also a growing empirical literature focusing on the effects of war experience on education, health, collective action and trust. While the impact of conflict on education and health has been found to be unequivocally detrimental, the effect of war on collective action and trust is still a very much open question, with several papers finding conflict victims to often be surprisingly resilient (see the survey by Bauer et al., 2016).[3] Particularly relevant for our current paper is the literature on the persistence of violence. In particular, Miguel et al. (2011) find a strong positive relationship between the extent of civil conflict in a player's home country and his propensity to behave violently on the soccer field, as measured by yellow and red cards. These findings are consistent with either a violent legacy of war experience, or alternatively with the existence of unobserved country-level characteristics such as for example cultural norms that jointly affect the war risk and individual violence proneness. Related to this, Grosjean (2014) argues that the "culture of honor" (enforcing violent vendetta) that was widespread in the Scottish and Scottish-Irish communities in the highlands was "imported" into the US by migrants from these regions in the 18th century. She shows that this violent culture has only persisted until today in the South of the US where institutions were weak at the time of migration. Fisman and Miguel (2007) show the persistence of social norms on corruption using data on parking tickets of diplomats from various countries in New York City.

There is also a literature that focuses on the impact of exposure to various events during childhood. The psychology literature finds a particularly large vulnerability to war trauma for children aged between 5 and 9 years, as they still lack consolidated identities (see Garbarino and Kostelny, 1996; Kuterovac-Jagodic, 2003; Barenbaum et al., 2004). Beyond the effects of war exposure, Giuliano and Spilimbergo (2013) find a persistent effect of having experienced a recession when young on individual beliefs that success in life depends more on luck than effort, support of more government redistribution, and tendency to vote for left-wing parties. In contrast, Gould et al. (2011) exploit random variation in the living conditions of Yemeni children who arrived in Israel in 1950 to identify a beneficial impact of a "modern environment" during early childhood (0-5 years of age) on various socio-economic outcomes later in life. Using a quasi-random assignment of refugees in Denmark, Damm and Dustmann (2014) find that the share of young criminals in a given neighborhood in a given assignment year increases the probability of a young man to commit a crime later in life and that this effect is especially strong for those from the same ethnic group. There is also experimental evidence that the formation of pro-social preferences, and in particular of preferences related to altruism, egalitarianism, meritocracy and envy, is particularly active before 12 years of age, and in particular between 6 and 12 years of age (Almas et al., 2010; Bauer et al., 2014; Bauer et al., 2015; Fehr et al., 2008; and Fehr et al., 2011).

[3] In particular, there are recent papers studying the effect of war exposure on education attainment (see Akresh and de Walque, 2010; Blattman and Annan, 2010; Leon, 2012; Shemyakina, 2011; and Swee, 2008), on mental health, and in particular on post-traumatic stress or anxiety (see Barenbaum et al., 2004; Dyregrov et al., 2000; and Derluyn et al., 2004), on political beliefs and participation and local collective action (see, e.g., Bellows and Miguel, 2009; Blattman, 2009; and Humphreys and Weinstein, 2007; Adhvaryu and Fenske, 2014), and on trust and social capital (Rohner et al., 2013b; Besley and Reynal-Querol, 2014; Fearon et al., 2009; Gilligan et al., 2010; Voors et al., 2012; Whitt and Wilson, 2007; and Cassar et al., 2013).

Finally, our paper is also related to the literature on the economics of immigration (cf. e.g. Borjas, 1994, 2003; Card, 1990, 2001; and Dustmann and Kirchkamp, 2002) and the strain of work exploiting exogenous allocation of migrants to study labor market outcomes (Edin et al, 2003, Beaman, 2012, Glitz, 2012, Hainmueller et al., 2016) and schooling (Gould et al., 2002).

Our paper is novel with respect to various dimensions: First, it is to the best of our knowledge the first paper that studies the effect of conflict exposure on crime later in life. Second, we can draw on fine-grained data on nationalities of perpetrators and victims to document the persistence of intra-national hostility. Third, the federalist organization and institutional heterogeneity of Switzerland allows us to study the impact of public policies on the persistence of violence.

3 Data and Descriptive Statistics

Switzerland is a federal state with 26 cantons (i.e. the main sub-national entities), a population of about 8 million people, and a strong humanitarian tradition. According to the Swiss Federal Statistical Office in 2012 about 23.3% of the population were foreign nationals. The number of asylum seekers – who are defined as individuals who have applied and are waiting for being approved the refugee status – is considerably smaller: Over the 2009-2012 period the yearly average of asylum seekers was around 30'000 individuals, corresponding to about 0.4% of the Swiss population. Most of these individuals originate from countries experiencing wars, genocides, political instability, and autocracy. The Swiss federal administration sets stringent conditions for the delivery of political asylum. In particular, individuals must demonstrate that a return to their home country would endanger their lives, and economic deprivation cannot be the official reason for requesting asylum to the Swiss administration. As a result, on average only 15 percent of asylum seekers obtain the asylum. The average processing time of the procedure of asylum request is around 300-400 days. Online Appendix C provides more details on the procedure of admission.

Our baseline sample consists of asylum seekers only, observed during their procedure of asylum request. This is a relatively homogeneous population with similar incentives and characteristics. We deliberately avoid to compare criminality of asylum seekers to the one of native residents, as this comparison could be driven by unobserved heterogeneity and detection policies biased towards specific groups. In fact, the identifying variation that we use is the comparison of violent crime propensities between asylum seekers with past exposure to conflict versus those without exposure.

3.1 Asylum Seekers, Economic Migrants and Conflicts

Data on Asylum Seekers and Economic Migrants. The Federal Office for Migration (FOM) provides us with non-publicly available administrative individual-level data for all asylum seekers and economic migrants arriving in Switzerland from 1992 onwards. For every person we know the beginning and end of stay, the location, nationality, age, gender, and the residence status (the permit held).[4] Table 1 displays some descriptive statistics on the population of asylum seekers (for economic migrants, see Section 7). As expected, the sample is not balanced in terms of gender and age. With 75% of males and 58% below 30 year old, young males -who are known for being the most violence prone individuals- are clearly over-represented among asylum seekers. Table 1 lists also the top ten countries of origin. Almost a third of individuals originate from either Eritrea, Sri Lanka or Nigeria.

Table 1: Share of Asylum seekers in Switzerland by Age, Country of Origin and Gender

Age Class	Share	Age Class	Share	Country	Share	Country	Share	Gender	Share
[16-17]	3.11	[45-49]	2.94	Eritrea	13.01	Tunisia	4.78	Male	75.08
[18-20]	10.89	[50-54]	1.61	Sri Lanka	9.09	Serbia	4.33	Female	24.92
[21-24]	19.73	[55-59]	0.92	Nigeria	8.57	Turkey	4.26		
[25-29]	24.78	[60-64]	0.57	Afghanistan	5.33	Iraq	4.15		
[30-34]	18.22	[65-69]	0.27	Somalia	5.10	Syria	3.92		
[35-39]	10.70	[70-79]	0.25						
[40-44]	6.00	[80+]	0.03						

Data on Past Exposure to Conflicts. Data on various forms of past exposure to conflict are used to construct our main explanatory variables. For *exposure to civil conflict* we retrieve information from UCDP/PRIO's "Armed Conflict Dataset" (UCDP/PRIO, v4-2013), which is by far the most widely used data on civil conflict. We include all civil conflicts reaching UCDP/PRIO's threshold of at least 25 battle-related fatalities. For *exposure to mass killings* we rely on the most widely used dataset on mass killings, collected by the "Political Instability Task Force" (Political Instability Task Force, 2013). They define mass killings as events that "involve the promotion, execution, and/or implied consent of sustained policies by governing elites or their agents – or in the case of civil war, either of the contending authorities – that result in the deaths of a substantial portion of a communal group or politicized non-communal group".[5] Note that exposure to mass killings of civilians is a very different type of violence exposure than the one for civil war. An event is only coded as civil war when fighting is two-sided and when battle-related casualties are sizable for all conflict parties. In contrast, mass killings of civilians are one-sided with civilians being helpless victims, and fighting not necessarily being related to battles. Hence, in many cases

[4] The main Swiss residence permits are the following. For EU/EFTA citizens there exist the "L EU/EFTA permit" (short-term residents), the "B EU/EFTA permit" (resident foreign nationals with a valid employment contract; permit is issued for 5 years, renewable), the "C EU/EFTA permit" (settled foreign nationals who have been in Switzerland for at least five years; the holder's right to settle in Switzerland is not subject to any time restrictions or conditions), and the "G EU/EFTA permit" (cross-border commuters). For non EU/EFTA citizens there exist again analogous "B", "C" and "G" permits, but in addition "Permit F" (former asylum seekers who have been granted temporary protection), and "Permit N" (asylum seekers). The law has also put in place a so-called "Permit S" (for former asylum seekers who have been granted refugee status), but it has hardly ever been used (yet) in practice, with asylum seekers obtaining permanent protection being awarded the "B" permit instead (see Hofmann und Buchmann, 2008: 20). For more information, see `https://www.sem.admin.ch/sem/en/home/themen/aufenthalt.html`.

[5] By this definition, killing episodes have in the last 50 years taken place in 28 different countries, and include all of the most notorious historical instances of large-scale massacres like, for example, the ones in Sudan, Rwanda, Bosnia or Cambodia.

mass killings can take the form of purges by the state against civilians rather than armed conflict between the state and armed rebels.

Our data on asylum seekers report no information on exposure to violence during conflict at the individual-level. Therefore we make the choice of measuring past conflict exposure at the cohort-level. Our baseline measure is Kid [1-12], a binary variable that codes for cohorts who were aged between 1 and 12 when civil conflict or mass killing occurred in their origin country. Notice that this cohort effect encompasses direct and indirect forms of exposure to conflict that we cannot disentangle, such that being personally targeted by acts of violence (e.g. being injured or witnessing the killing of a family member) or being exposed to a war context where economic deprivation and social capital depletion prevail. We focus on the first 12 years of age, in line with the substantial evidence that many preferences and attitudes are formed during this period of life.[6] Moreover, beyond its intrinsic interest, focusing on exposure to violence during childhood serves the purpose of causal analysis by alleviating endogeneity issues due to self-selection into violence, e.g. excluding former perpetrators (see Section 4.1). Finally, in some specifications, we split our variable of exposure into two categories, Kid [1-12] (only cc) and Kid [1-12] (only mk), that correspond respectively to specific exposure to Civil Conflict and Mass Killing. In our robustness analysis, we also build an alternative measure of victimization at the cohort-level, Women[1,+], a binary variable coding for cohorts of women who experienced (at any age) a conflict with systematic wartime rape in their origin country. To this purpose we use the data of Cohen (2013). She takes as starting point the list of major wars of Fearon and Laitin (2003) and uses a variety of data sources to determine which of these wars feature the systematic use of wartime rape by governments or insurgents.

3.2 Crime Data

The Federal Statistical Office (FSO) provides us with non-publicly available exhaustive data on all crimes detected by the police in Switzerland between 2009 and 2012. This individual-level dataset has been collected by local police services and covers all cases when somebody was charged with infractions to the (federal) Penal Code. Remarkably, the data convey precise information on the nationalities and residency status of victims and perpetrators of any detected crime, as well as on the place, time and type of the crime. Following the empirical literature crimes are sorted into two broad categories: violent crime (murders, injuries, threats, sexual assault...) and property crime (thefts, burglaries, robberies, scams...). Our main focus is on violent crimes perpetrated by asylum seekers. In the baseline analysis we make no distinction in term of nationalities or background of victims. This makes sense given that, in the data, violent asylum seekers target not only other asylum seekers but also the rest of the population: 35% of victims are themselves asylum seekers, 28% are foreign residents, 36% are natives. However intra-asylum seeker violence is clearly over-represented and victim targeting is not random –a pattern at the core of the mechanism we

[6] Garbarino and Kostelny, 1996; Kuterovac-Jagodic, 2003; Barenbaum et al., 2004; Fehr et al., 2008; Almas et al., 2010; Fehr et al., 2011; Bauer et al., 2014; Bauer et al., 2015.

investigate in Section 5.

For the sake of confidentiality the FSO prevents us from merging at the individual level crime data with migration data. Together with this legal provision, the fact that our explanatory variables of past exposure to violence are anyway measured at the cohort-level, leads us to conduct our statistical analysis at the level of a cohort of asylum seekers from nationality n, gender g, age group a, in year t. Hence, combining migration and crime datasets at the cohort-level, we build our main dependent variable, the *violent crime propensity*, labeled $\mathrm{CP}_{n,g,a,t}$, that corresponds to the yearly number of crimes perpetrated by a cohort divided by its size.[7] Note that, in our definition of a cohort, we lump together individuals by age brackets rather than by year of birth brackets because age is a first-order determinant of criminality.[8] Given the short time span of our panel (2009-2012), these two coding options are in fact very close and they would be identical in the case of a cross-sectional dataset.

Exhaustive data of such high quality is only available in Switzerland for detection data of charges for crime, and not for data on final convictions by a court.[9] While of course the number of charges for crime are highly correlated with the number of convictions, there may be discrepancies if for example in some cantons and years the police authorities are more active and successful than in others. Such spatial differences in detection probabilities of a crime are however accounted for by the exogenous allocation of asylum seekers across the Swiss territory (or the inclusion of canton $\times$ year fixed effects in Section 7). There could also be a wedge between crime rates of nationals and foreigners if for example some police forces were to predominantly control foreign-looking individuals. This however would not bias our estimation as we restrict ourselves to within-asylum seeker comparisons and do not compare asylum seeker crime rates with crime rates of Swiss citizens.

3.3 Descriptive Statistics

We observe a total annual number of between 22790 and 32413 asylum seekers from 134 nationalities over the 2009-2012 period. After aggregating by nationality n, gender g, age group a, for each year t, this leaves us with 4820 cohorts, which are our units of observation. The average cohort is composed of 22 individuals. Notice that the variance in cohort size is large (standard deviation equals 63 individuals) and this feature of our micro-data calls for weighting our cohort-level regressions by

[7] This definition of a cohort-specific crime propensity is not affected by recidivism, as we count the number of crimes committed by different individuals (i.e. if person A commits two crimes and person B of the same cell none, this results in the same overall crime propensity as when both of them commit one crime each).

[8] While the exact age is reported for asylum seekers, we have only age brackets for the sample of economic migrants (see Section 7). For the sake of comparison we regroup asylum seekers in similar age brackets, that are 16-17, 18-20, 21-24, 25-29, 30-34, 35-39, 40-44, 45-49, 50-54, 55-59, 60-64, 65-69, 70-79, $>$ 80 years old.

[9] Due to the differences across cantons regarding the judicial procedures and duration of trials, the harmonization of individual conviction data is very hard and does not currently exist. Moreover, a meaningful harmonization of conviction data for asylum seekers would be even harder, as in many cases asylum seekers may get expelled before the end of the lengthy trial.

the number of individuals in each cohort (see our discussion on grouped data in Section 4).[10] Table 2 reports the main descriptive statistics for cohorts. Note first that 84% of cohorts originate from countries that have experienced at least one episode of civil conflict or mass killings since 1946. Among the 134 nationalities of origins, conflicts occurred in 90 countries, mass killings in 27 countries, and wartime rape in 46 countries. These nationalities are the ones that contribute to our identifying variations. All these countries experienced violence in some, but not all years, leading to within-nationality, *inter-cohort* variations in exposure to violence: The sample mean of childhood exposure, KID [1-12], is equal to 48%. As for our alternative measure of exposure, WOMEN[1,+], we see that 38% of female cohorts have experienced a conflict where wartime rapes were pervasive. Finally, note that a substantial part of asylum seekers do not flee their country during war time, but years or even decades afterwards.[11] The average number of years since the last Civil Conflict/Mass Killing is around 10 years. An important aspect to be noticed is that the chances to be recognized as refugee depend on proving to have been personally persecuted so they do not rely solely on coming from a country experiencing civil conflict. The fact that asylum seekers come from countries with recent conflict increases accessibility and reliability of these proofs.

Table 2: Cohorts of Asylum Seekers - Summary Statistics

variable	mean	sd	max	min
Male	56.6	49.5	100	0
Cohort Size (# individuals)	21.8	63.2	958	1
Civil Conflict & Mass Killing	84.1	36.6	100	0
Wartime Rape	38.6	48.7	100	0
Distance to last CC or MK (years)	9.6	11.9	64	0
KID [1-12]	48.3	49.9	100	0
KID [1-12] (ONLY CC)	46.6	49.9	100	0
KID [1-12] (ONLY MK)	16.1	36.7	100	0
WOMEN[1,+] (WAR. RAPE)	37.6	48.4	100	0
$\mathrm{CP}_{n,g,a,t}$ (Violent Crime Propensity)	2.04	8.1	100	0

Note: Sample of 4820 cohorts of asylum seekers, 134 nationalities, 14 age brackets, 2009-2012. Except for cohort size and distance to last CC or MK, all figures represent percentages.

We now turn to cohort-level (violent) crime propensities. The sample average of $\mathrm{CP}_{n,g,a,t}$ is equal to 2.04% with large heterogeneity across cohorts (s.d. equals 8.1%), the main sources of variance being related to age. Figure 1 explores the age-crime nexus by reporting average

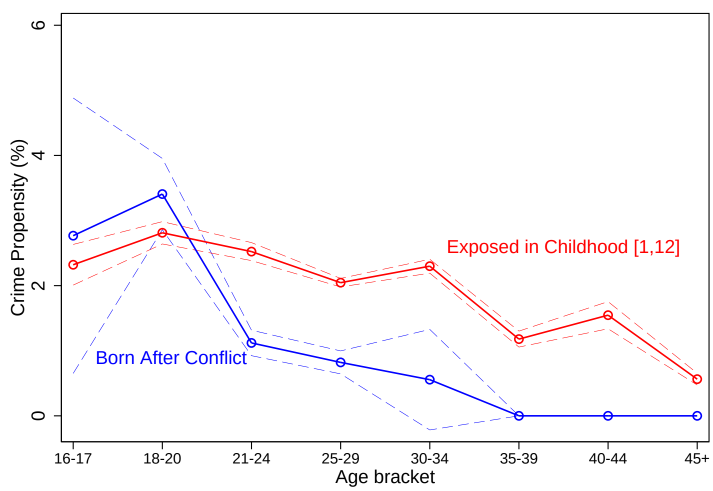
[10] On average, cohorts are composed of 29 asylum seekers for those exposed during childhood (aged between 1 and 12), 26 for those exposed after 12 years old and 41 for those born after the last conflict. As expected, there are larger inflows of asylum seekers from countries with active or recent conflict: there are on average 28 asylum seekers in cohorts from countries with current conflict, 37 in cohorts from countries where the last conflict occurred up to 10 years before and 12 asylum seekers in cohorts from countries where the last conflict occurred more than 10 years before.

[11] 41% of cohorts arrive in Switzerland in a year when active conflict is still raging in their home country. Further, only 2 percent of asylum seekers originate from a country that is coded as experiencing current one-sided mass killings, and 5 percent of female cohorts flee a country that is currently plagued by wartime rape.

propensity by age bracket for the two groups of cohorts at the core of our identification strategy: Cohorts exposed to CC or MK during childhood (in red) and those born after conflict (in blue).[12] For the two groups, we see a clear spike in violent crime in early adulthood and then a steady decrease across ages. Pattern and magnitude conform to the large evidence on age-crime curves that has been collected in the criminology literature for other populations-periods (see Freeman, 1999, for a review on determinants of criminal behavior).

The striking and novel point here relates to the crime differential between the two groups: While for very young cohorts the crime propensity is high for any of the two groups, from the age of 20 on an important gap widens up. In particular, cohorts with past exposure to conflict keep having high crime propensities until the age of around 40, while for cohorts born after conflict, the crime propensity drops already massively from age 21 onwards. After the age of 40 the two curves converge again on a low level. Across the considered age brackets, the average differential is equal to 0.85 percentage points, a substantial wedge that implies that cohorts exposed during childhood are on average 1.75 times more prone to violent crimes than cohorts born after a conflict. This graphical evidence illustrates our main result. The econometric analysis aims to confirm that this excess crime propensity is causally related to the exposure to violence in childhood, accounting for a variety of potential confounding factors.

Figure 1: Age-Violence Curves



Confidence intervals are set at 99%.

[12]We restrict ourselves to the subsample of cohorts from countries with conflict or mass killings background that are born after war or exposed during their childhood (KID [1-12] = 1). For each age class we average $\mathrm{CP}_{n,g,a,t}$ across cohorts and time. Because they represent less than 7% of all observations, all age brackets above 45 years old are regrouped in a single category.

4 The Impact of Past Exposure to Conflict on Violent Crimes

This section documents the causal impact of past exposure to conflict on violent crimes. Many policy-relevant channels contribute to this phenomenon and Section 5 focuses specifically on the role of intra-national grievances.

4.1 Identification Strategy

Our unit of observation is a cohort. The decision to perpetrate a crime or not is however made at the individual level. A specification based on micro-data would have the individual as unit of observation and would estimate a random-utility discrete-choice model, such as e.g. a binomial logit. For samples based on grouped data, like ours, Durlauf et al. (2010) show that the logit model translates into a linear specification where the dependent variable is the log of the odds ratio of the crime propensity. They recommend to implement this aggregate logit procedure only when the aggregation-level is sufficiently high such that sampling errors are limited and group-level crime frequencies approximate well the underlying crime probabilities. In our context, the average cohort size is not large (i.e. 21 individuals) and, more importantly, the variance is large, with many small cohorts –the ones with 5 individuals or less representing 59% of the sample. Hence, sampling errors become a salient issue and, together with the fact that crime is a rare event, this implies that the number of zeroes is very large ($\text{CP}_{n,g,a,t} = 0$ for 82% of cohorts), making the computation of odds ratio problematic. We consequently prefer a baseline specification that is compatible with zeroes (and ones as well) by estimating a linear crime regression with $\text{CP}_{n,g,a,t}$ as dependent variable. This choice follows the standard practice in the crime literature (see e.g. Bell et al., 2013). All our cohort-level regressions are weighted by the size of the cohort as recommended by Angrist and Pischke 2009 (Section 3.4.1, pp. 91-94) in the context of grouped data. Our baseline specifications retain analytical weights because the set of covariates is cohort-specific and our dependent variable (crime propensity) corresponds to the cohort-level average of crime occurrence across individuals.[13] Finally, in the robustness Section 4.4 we investigate alternative options and econometric specifications for dealing with small cohorts, zeroes/ones, and weighting schemes.

Our baseline crime regression corresponds to

$$\text{CP}_{n,g,a,t} = \alpha \times \text{K}\textsc{id}\ [\text{1-12}]_{n,a,t} + \sum_{k=13}^{k=80+} \beta(k) \times \textsc{expo}(k)_{n,a,t} + \mathbf{FE}_{n,t} + \mathbf{FE}_g + \mathbf{FE}_a + \varepsilon_{n,g,a,t}, \quad (1)$$

where $\text{CP}_{n,g,a,t}$ stands for the violent crime propensity of a cohort of nationality $(n)\times$ gender$(g)\times$ age bracket $(a)\times$ year (t). As discussed above, our main explanatory variable is $\text{K}\textsc{id}\ [\text{1-12}]_{n,a,t}$ that is a binary measure of childhood exposure. The set of control variables $\textsc{expo}(k)_{n,a,t}$ are also

[13] In Section 4.4 we show that the exact choice of the weighting procedure (analytical, frequency or probability) has a limited impact on the estimated standard deviations.

binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$. Hence, in equation 1, the implicit reference group consists of cohorts born after a conflict.[14] As a consequence, our parameter of interest α can be interpreted as the crime differential between cohorts exposed during their childhood and cohorts born after the conflict. Crucially, the richness of our dataset makes possible the inclusion of fixed effects that account for unobserved heterogeneity in nationality×year ($\mathbf{FE}_{n,t}$), in age ($\mathbf{FE}_a$) and in gender ($\mathbf{FE}_g$). Finally, robust standard errors are clustered at the nationality × year level. In the Online Appendix E we discuss in more details the potential econometric pitfalls.

All in all, we deal with a demanding empirical strategy: Our source of identification corresponds to variations in crime-propensities across cohorts of asylum seekers from the *same nationality, gender and migration wave* but with different exposure to conflict (i.e. born after war/exposed in childhood). Because these cohorts inevitably differ in terms of age, we must control for the direct effect of age by comparing them to other cohorts with similar age structure but born after a conflict in another country. To give an example, our strategy consists of computing the crime differential of two Rwandese cohorts, one born in 1996 (born after the 1994 genocide), and one born in 1990 (exposed during childhood), migrating to Switzerland in 2012. In order to control for age-crime effects, their crime differential is compared to the one of two Nigerian cohorts of same ages but both being born (in 1990 and 1996) after the 1967-1970 civil war. Our comparison of the blue and red crime-age curves in Figure 1, panel (a), follows the same logic.

Thus, our strategy is basically akin to a difference-in-difference in country × cohort.[15] The identifying assumption is that past exposure to conflict is the only reason why the decline in crime rates with age is smaller for asylum seekers exposed in childhood than for their co-nationals born after. A threat to our identification strategy would be that post-war contexts are systematically associated to a flattening of the age-crime curve for all cohorts. With this respect, a reassuring fact is the robustness of our results when all nationalities with no recent history of conflict are excluded from the sample (columns 3 to 5 in the baseline Table 3). In this case all in-sample cohorts have been exposed to a conflict or to a post-conflict context: The control group itself being composed of cohorts born after a conflict, the diff-in-diff results cannot be driven by a war-induced flattening of the age-crime curve for all cohorts. Another reassuring pattern is the observation in Table 5 of a *sharp decrease* in crime propensity between cohorts born during conflicts and those born just after. Finally, we explore further this question in Section 4.4. Among other validity checks, we perform a Monte Carlo (placebo) test based on cross-cohorts counterfactual reassignments of conflict exposure during childhood.

[14] We code $\text{EXPO}(k)_{n,a,t} = 1$ for cohorts who were aged k years old when civil conflict or mass killings occurred in their origin country. A cohort could be exposed at different periods of life. Cohorts that are born after the last year of conflict in their origin country are considered as born after. The last year of conflict is defined as the last year of conflict over all the years of conflict in a country.

[15] We thank Christian Dustmann and Erzo Luttmer for their comments and suggestions on this point.

4.2 Exogenous Spatial Allocation of Asylum Seekers in Switzerland

We now provide an overview of the actual process of allocation of asylum seekers across Swiss cantons. We also discuss briefly some statistical evidence supporting the view that the distribution key is based on canton population size only and is exogenous to migrants' characteristics. Many more details on the institutional/legal aspects and on the formal statistical tests are provided in Appendices C and D respectively.

Overview of the allocation process– Most asylum seekers enter Switzerland illegally (especially crossing the Italian border) and apply for asylum in one of the four national reception and procedure centers (RPC). In the RPC, asylum seekers go through interviews, where they are asked to provide identity proofs, fingerprints, and their application reasons. During the lengthy assessment process, the credible asylum seekers are granted a temporary N permit by the Swiss authorities. Given the difficulty in assessing the threat of persecution in the home country and the large number of applicants (around 25 000 per year over the 2009-2010 period), the asylum process takes substantial time. Between 2009-2010, the average duration of the process was 300-400 days.

Crucially, during this period holders of the N-permit are exogenously allocated to cantons and are not allowed to change canton. The allocation of new N-permit holders to the 26 Swiss cantons is determined by an exogenous allocation key based on the cantonal population. Once an asylum seeker has been allocated to a given canton, the canton in charge organizes the accommodation in cantonal centers or flats and takes care of the interviews and of financial matters. This allocation rule was introduced in the amendment to the Aliens Law in 1988, presumably to minimize self-segregation and ghetto effects and avoid social tensions between natives and asylum seekers.

The allocation is made by the Federal Office for Migration in Bern and its decision cannot be appealed unless under certain precise conditions (family unity reasons like minors being allocated to a different canton than their parents or if the asylum seeker or a third person are under serious threat) and the change of the canton is possible only if the two cantons approve it. According to Hofmann and Buchmann (2008), it is extremely rare that asylum seekers change canton or cantons refuse asylum seekers.

Statistical Evidence– Figure 2 in the Online Appendix displays the time series evolution of asylum seeker stocks across the 26 Swiss cantons between 1994-2010 (the main peak corresponding to the end of the Kosovo war). Visual inspection confirms parallel trends across cantons and this constitutes a first and rough piece of evidence consistent with an exogenous allocation process of migrants across cantons. More substantially, we provide formal statistical tests in Table 22 of Online Appendix D. The purpose is to tackle the question of whether there is indeed an exogenous allocation of asylum seekers following the official population-based distribution key –as we claim– or if there may be some selection on relevant dimensions. The basic approach consists in testing for the difference in means between cantons for various observable cohort characteristics (i.e. exposure to violence during childhood, age, gender). We first perform this test for each nationality of asylum seekers. However, a concern is that, for small nationality sizes, sampling variations mechanically lead to observed patterns of spatial concentration in some cantons. A first attempt to tackle this sampling issue consists in pooling cohorts from all nationalities by year. A second attempt corresponds to a Monte Carlo simulation (1000 draws) generating artificial random allocations that we compare to the observed allocation. Overall, the tests of Table 22 are supportive of our identifying assumption that the allocation of asylum seekers across cantons can be considered as exogenous with respect to their age, gender and past exposure to violence.

4.3 Baseline Results

Table 3 displays the baseline estimation results of our cohort-level crime regression (equation 1). We report only our coefficient of interest, α, that captures the impact on violent crime propensity of cohorts exposed to civil war or mass killings during childhood (1-12 years), the reference group being cohorts born after conflict. Column 1 reports the results of a pooled regression with age and gender fixed effects but without country $\times$ year fixed effects. The coefficient of interest is positive and significant at the 5 percent threshold. However, as explained in Section 4.1 this correlation is potentially driven by confounding factors that relate to pre-conflict characteristics of origin countries or by selection into migration. In Column 2, we consider a specification with the full battery of fixed effects where the identifying variations come from within-nationality / between-cohorts comparison. This is our preferred specification (baseline). The coefficient of past exposure is reduced by one third but it retains statistical significance and a positive sign. In term of magnitude, we observe that the crime propensity of cohorts exposed during childhood is on average 0.83 percentage points higher than the propensity of their co-national cohorts born after the war –a substantial effect given that the sample mean of violent crime propensity is equal to 2.04 percentage points. By means of benchmarks, gender and age have comparable consequences on crime propensity. The non-reported coefficient of the male dummy (reference group being female) is 3.03. This is not surprising, as it is widely known that most violent crimes are perpetrated by men. But the striking point is that exposure to conflict has an impact of the same order of magnitude, although smaller (about one fourth). Also age matters (coefficients are not reported here): the 16-17 years old have 6.5 percentage points, the 18-20 years old have 5.95 percentage points, 21-24 years old 4.8 percentage points and 25-29 years old 3.5 percentage points higher crime propensity than the cohort being more than 50 years old. In a nutshell, even if gender and age tend to be powerful determinants of crime, past exposure to conflict in childhood still substantially matters. In column 3 we exclude all cohorts originating from countries that have experienced no civil conflict or mass killing since 1946. The point estimate is barely changed in spite of the sample size reduction. In columns 4 and 5 we estimate the same specification as in column 3, but now separately for conflict and mass killings. In each case, the sample is again restricted to countries having experienced each specific type of violence.

Table 3: Benchmark Regression of Crime Propensities and Conflict Exposure

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Violent Crime Propensity				
Sample	Full	Full	CC & MK	CC	MK
KID [1-12]	1.244**	0.833**	0.827**		
	(0.604)	(0.363)	(0.360)		
KID [1-12] (ONLY CC)				0.809**	
				(0.360)	
KID [1-12] (ONLY MK)					1.669*
					(0.891)
Gender FE	Yes	Yes	Yes	Yes	Yes
Age Group FE	Yes	Yes	Yes	Yes	Yes
Nationality × Year FE	No	Yes	Yes	Yes	Yes
Observations	4,820	4,746	4,015	3,991	1,778
R-squared	0.125	0.564	0.587	0.587	0.477
Sample mean (Crime Prop.)	2.04	2.04	2.02	2.01	1.83

Note: OLS estimations weighted by the number of individuals in each cohort. Robust standard errors are clustered at nationality × year levels. *** p<0.01, ** p<0.05,* p<0.1. All estimations include gender fixed effects, age group fixed effects and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$. The group of reference is people born after the last years of violence. The dependent variable *Violent Crime Propensity* stands for the violent crime propensity of a cohort of nationality $(n)\times$ gender$(g)\times$ age bracket $(a)\times$ year (t). KID [1-12] is a binary measure of childhood exposure to civil conflict or mass killing (columns 1 to 3), to civil conflict only (column 4), to mass killing (column 5). In columns 3 to 5, the sample is restricted to cohorts originating from countries that have experienced civil conflict or mass killing since 1946.

Heterogeneous Effects: Gender and Type of Crime – In Table 4 we study heterogeneous effects with respect to gender and type of crime. Columns 1 and 2 replicate our baseline specification (Column 2 of Table 3) on the subsamples of male and female cohorts respectively. The results are clearly driven by men, with the coefficient for women being of smaller size and not statistically significant. Columns 3 to 5 focus on the propensity to property crime instead of violent crime as dependent variable, respectively for the full sample, for men only and for women only. The magnitude and the statistical significance of our variable of interest is strikingly lower, suggesting that exposure to conflict during childhood impacts future violent behaviors, but leaves future non-violent criminality unaffected. We see this contrasted evidence as a first indication that our causal effect captures a mechanism of perpetuation of violence –a point that we develop in more detail in Section 5. From the perspective of causal analysis we interpret the absence of effect for property crime as an indication that the correlation between past exposure and violent crime is unlikely to be spuriously driven by omitted factors (unless such factors were to affect differentially violent crimes and property crimes).

Heterogeneous Effects: Age – Table 5 is also devoted to heterogeneous effects, with a special focus on age. In Column 1, we are interested in lifecycle modulations of the impact of past exposure.

Table 4: Heterogeneous Effects – Gender and Type of Crime

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Crime Propensity				
Type of Crime	Violent	Violent	Property	Property	Property
Sample (Gender)	Men	Women	All	Men	Women
Kid [1-12]	1.093**	0.429	0.037	0.145	-0.007
	(0.479)	(0.278)	(0.635)	(0.685)	(0.361)
Gender FE	No	No	Yes	No	No
Age Group FE	Yes	Yes	Yes	Yes	Yes
Nationality × Year FE	Yes	Yes	Yes	Yes	Yes
Observations	2,660	2,008	4,746	2,660	2,008
R-squared	0.608	0.293	0.798	0.847	0.434
Sample mean (Crime Prop.)	3.24	0.47	4.27	5.82	2.24

Note: OLS estimations weighted by the number of individuals in each cohort. Robust standard errors are clustered at nationality × year levels. *** p<0.01, ** p<0.05,* p<0.1. All estimations include gender fixed effects, age group fixed effects, nationality × year fixed effects and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$. The group of reference is people born after the last years of violence. The dependent variable *Violent Crime Propensity* stands for the violent (property) crime propensity of a cohort of nationality $(n)\times$ gender $(g)\times$ age bracket $(a)\times$ year (t) in columns 1 and 2 (columns 3 to 5). Kid [1-12] is a binary measure of childhood exposure to civil conflict or mass killing.

We interact our main explanatory variable with (mutually exclusive) decade dummies coding for the current age of the cohorts. We see no significant difference for exposed and non-exposed people aged below 40, while the gap is positive and statistically significant for older cohorts. These results confirm the insights of Figure 1 where unconditional crime propensity peaks during teenage years and then decreases drastically for non-exposed cohorts while it remains at a high level for exposed cohorts. Our interpretation is that past exposure to conflict during childhood prevents the dampening effect of age on violence to take place.

In the next two columns we investigate in more details the age where exposure to conflict happens. In Column 2 we add a novel variable of *post-war* exposure. The variable Born After [0-12] takes a value of 1 if there has been a civil conflict or a mass killing in the 12 years *before* being born. If the main effect of conflict exposure is about economic deprivation or institutional collapse, we should expect the variable Born After [0-12] to be a similarly powerful predictor as our variable of interest. It turns out that this is not the case and we get back to this issue when we study the underlying mechanisms (Section 5). In fact, while our main explanatory variable of past exposure retains its magnitude and statistical significance, the coefficient of Born After [0-12] is of much smaller magnitude and is not statistically significant at conventional levels. This *sharp decrease* in crime propensity for cohorts born just after the war with respect to those exposed during their childhood is again reassuring for our causal analysis as it makes unlikely any contamination of the results by omitted variable bias. In column 3, Kid [1-12] is split in two, with a separate variable capturing the impact of war exposure during the first five years of life, and a second variable capturing war exposure during the 6th and the 12th year of life. It appears that the effect is somewhat stronger for Kid [1-5], although narrowly missing statistical significance (t-stat=1.65). This is consistent with earlier studies pointing out the crucial importance of these five earliest years of life (see e.g. Gould et al. (2011) and the literature on early child development (e.g. Heckman et al., 2013)).

Table 5: Heterogeneous Effects – Lifecycle Modulation and Age of Exposure

	(1)	(2)	(3)
Dependent Variable	Violent Crime Propensity		
Kid [1-12] × Age [16-20]	0.263		
	(1.087)		
Kid [1-12] × Age [21-29]	0.650		
	(0.422)		
Kid [1-12] × Age [30-39]	0.590		
	(0.385)		
Kid [1-12] × Age [40-49]	1.824**		
	(0.736)		
Kid [1-12] × Age [50+]	1.547**		
	(0.643)		
Kid [1-12]		0.857**	
		(0.339)	
Born After [0-12]		0.125	0.273
		(0.384)	(0.350)
Kid [1-5]			0.835
			(0.507)
Kid [6-12]			0.484
			(0.306)
Gender FE	Yes	Yes	Yes
Age Group FE	Yes	Yes	Yes
Nationality × Year FE	Yes	Yes	Yes
Observations	4,746	4,746	4,746
R-squared	0.565	0.564	0.565
Sample mean (Crime Prop.)	2.04	2.04	2.04

Note: OLS estimations weighted by the number of individuals in each cohort. Robust standard errors are clustered at nationality × year levels. *** p<0.01, ** p<0.05,* p<0.1. All estimations include gender fixed effects, age group fixed effects, nationality × year fixed effects and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$. The group of reference is people born after the last years of violence. The dependent variable *Violent Crime Propensity* stands for the violent crime propensity of a cohort of nationality $(n)\times$ gender$(g)\times$ age bracket $(a)\times$ year (t). Kid [1-12] is a binary measure of childhood exposure to civil conflict or mass killing during childhood. Born after [0-12] takes a value of 1 if there has been a civil conflict or a mass killing in the 12 years *before* being born. Kid [1-5] (Kid [6-12]) is a binary measure of childhood exposure to civil conflict or mass killing between 1 to 5 years old (6 to 12 years old).

Inspired by the recent article by Giuliano and Spilimbergo (2014) we also investigate in Table 13 whether there is an effect of conflict exposure in early adulthood (see Online Appendix). There are two main reasons why this age bracket (18-25 years of age) is less suitable in our context than in theirs: First of all, as shown in the psychological literature cited above, war trauma has particularly strong effects in the first years of life. Second, for our identification strategy it is crucial to focus on conflict victimization to rule out self-selection into violence. While one can plausibly claim that young children below the age of 12 are only victims, this is of course not the case anymore for the 18 to 25 years old. Still, as shown in Online Appendix Table 13 our results on the impact of conflict exposure in the first 12 years of age on violent crime continue to hold when controlling specifically for conflict exposure in the age bracket 18 to 25. While civil conflict exposure of the 18-25 years old does not affect future crime propensities, the exposure to mass killings in young adulthood does have an effect. Further, as shown in columns 3 and 4, conflict exposure at ages 18-25 tends to increase the propensity to property crime later in life.

Heterogeneous Effects: Intensity of Conflict – Finally, Table 6 is devoted to the heterogeneous impact of past exposure according to the intensity of the conflict in the origin country.[16] In column 1 our main explanatory variable is interacted with the inverse of the country of origin size. The idea is that the conflict threshold of UCDP/PRIO is defined in terms of the absolute number of fatalities and hence is likely to pick up more minor conflicts in large than in small countries. We measure size in terms of surface (km^2) and not population in order to mitigate any reverse causation bias from conflict intensity to population size. As expected, the coefficient of the interaction term is positive and significant. Thus, the impact of past exposure is larger for cohorts originating from small countries. In column 2 we turn to a more accurate assessment of the intensity of past exposure. We construct three mutually exclusive quantiles of conflict intensity, measured as number of battle-related deaths in a given country-year weighted by the area of the country. For an average area, low intensity corresponds to less than 4333 casualties by country-year, medium intensity to 4333-43694 and high intensity to more than 43694 casualties. The results show that exposure to high-intensity violence during childhood has a stronger impact on violent crime than exposure to medium or low intensity violence. In the same way, column (3) displays the effect of mass killings intensity. To construct the three quantiles of intensity, we rely on the country-year number of deaths index provided by Political Instability Task Force (2013),[17] weighted by country area. Here again the coefficients are ordered very clearly: the largest impact is for highly intense mass-killings, followed by events of medium intensity, while we detect no impact for mass killings of low intensity.

[16] Ideally, we would like to exploit both the geographical location of conflict within a country-year and the birth place of the individual to build a more accurate measure of individual exposure to violence. Unfortunately, we do not have information neither on the birth place of asylum seekers nor on the within country-year location of conflict worldwide (i.e. providers of micro-data such as ACLED only cover some but not all countries).

[17] This index ranges from 0 to 5 as follows: 0 - less than 300 deaths, 0.5 - 300-1000 deaths, 1.0 - 1000-2000 deaths, 1.5 - 2000-4000 deaths, 2.0 - 4000-8000 deaths, 2.5 - 8000-16000 deaths, 3.0 - 16000-32000 deaths, 3.5 - 32000-64000 deaths, 4.0 - 64000-128000, 4.5 - 128000-256000 deaths and 5 - more than 256000 deaths.

Table 6: Heterogeneous Effects – Intensity of Conflict

	(1)	(2)	(3)
Dep. Var.	Violent Crime Propensity		
Exposure to	CC and MK	CC	MK
Sample	Restricted	Restricted	Restricted
KID [1-12]	0.685*		
	(0.389)		
KID [1-12] × 1/(Size)	0.128**		
	(0.057)		
KID [1-12] : low intensity		0.211	-0.369
		(0.272)	(0.387)
KID [1-12] : medium intensity		1.534*	1.488*
		(0.844)	(0.831)
KID [1-12] : high intensity		1.856**	2.775*
		(0.926)	(1.411)
Gender FE	Yes	Yes	Yes
Age Group FE	Yes	Yes	Yes
Nationality × Year FE	Yes	Yes	Yes
Observations	3,991	3,991	1,778
R-squared	0.587	0.593	0.483
Sample mean (Crime Prop.)	2.01	2.01	1.83

Note: OLS estimations weighted by the number of individuals in each cohort. Robust standard errors are clustered at nationality × year levels. *** p<0.01, ** p<0.05,* p<0.1. All estimations include gender fixed effects, age group fixed effects, nationality × year fixed effects and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$. The group of reference is people born after the last years of violence. The dependent variable *Violent Crime Propensity* stands for the violent crime propensity of a cohort of nationality $(n)\times$ gender $(g)\times$ age bracket $(a)\times$ year (t). KID [1-12] is a binary measure of childhood exposure to civil conflict or mass killing.

4.4 Robustness Checks

In this section we show that the baseline estimate of Table 3, Column 2 is robust to a battery of sensitivity checks. All tables are relegated to Online Appendix A.

Alternative Crime Regressions – We start with testing a different level of clustering as an alternative to the baseline nationality × year level. In Table 14 we replicate the baseline Table 3 with standard errors clustered at the nationality × age level. The coefficients retain statistical significance in all the specifications where the full battery of fixed effects is included (Columns 2-4). In Table 15, we replicate our baseline estimate (column 2, Table 3), considering alternative sample sizes. We drop the cohorts with a size lower than 2, 5, 10, 15 and 20 adults, respectively. This leads to a big reduction in sample size, but the coefficient of interest remains positive and statistically significant.

In Table 16 we consider alternative econometric specifications for the crime regression (see the discussion in Section 4.1). In columns 1-3 we consider three options for dealing with zeroes and ones in our dependent variable. Column 1 restricts the OLS crime regression to the subsample of cohorts where crime propensity is strictly between 0 and 1. In spite of the major sample size reduction, the coefficient retains statistical significance (at the 10 percent level) and similar magnitude. Column 2 follows a less drastic route by keeping the full sample and estimating a Tobit model with two censoring levels, at 0 and at 1. The coefficient of interest is still significant at the 5 percent level. In column 3 we estimate a Poisson model on the full sample. This type of econometric model is well suited for count data like the cohort-level amount of violent crimes. Here again the coefficient is significant with a magnitude comparable to its OLS counterpart.[18] Columns 4 and 5 test for robustness to the removal of outliers. We retrieve from our baseline specification the estimated residuals. Then we trim the sample to remove all observations for which the residuals are further away than three standard deviations (column 4) or, even more radically, two standard deviations (column 5) from the mean residual. In both cases we obtain a statistically significant coefficient at the 5 percent, resp. 1 percent level; its magnitude is reduced with respect to its baseline counterpart. In columns 6 and 7 we go back to our baseline specification but change the weighting procedure of cohorts size by considering probability, resp. frequency weights rather than analytical weights. In both cases the estimated standard deviations are barely changed with respect to their baseline counterpart. This stability is likely due to the fact that, irrespective of the weighting scheme, all specifications estimate a cluster-robust covariance matrix at a level of aggregation higher than the cohort-level. Column 8 displays the result for the unweighted regression. The magnitude of the coefficient is comparable to its baseline value; however, it is much less precisely estimated. This confirms that small cohorts, where crime propensity is more likely to take extreme values (0 or 1), lead to mismeasurement errors and statistical noise.

Table 17 implements the aggregate logit procedure. It simply consists of an OLS crime regression where the dependent variable is now the log(odds-ratio) of Crime Propensity, namely $\ln \frac{\text{CP}}{\text{1-CP}}$. All other features are identical to the baseline specification. As explained in Section 4.1, coping with (i) zeroes and ones, and (ii) small cohorts, is problematic in such a setting. In column 1 we replace by CP = 0.001 and CP = 0.999 the observed values of CP that are equal to zero and one respectively. Though ad-hoc, this coding rule allows to force the definition of the odds-ratio for all cohorts. In columns 2 and 3 the same coding rule is used but we exclude small cohorts from the sample (respectively less than 2 individual and less than 3 individuals). Column 4 abstracts from this coding rule by simply excluding all cohorts with CP = 0 or CP = 1 from the sample. This leads to a big reduction in sample size. Finally columns 5 and 6 use probability and frequency weights, respectively, and column 7 displays the result for the unweighted regression. All in all the coefficient of interest keeps its positive sign. Its magnitude is not directly comparable to its baseline value due to the logistic transform of the dependent variable. The statistical significance is slightly reduced (below 10 percent threshold instead of 5 percent in the baseline).

[18] Note that the estimated standard deviations with the Tobit model have to be considered carefully due to the incidental parameters problem, as the length of our panel is short, $T = 4$ (Greene, 2004). See Osgood and Wayne (2000) for the use of a poisson-based regression with crime rates.

Placebo Test of conflict exposure during childhood– As mentioned in section 4.1, our identifying assumption is that past exposure to conflict is the only reason why the decline in crime rates with age is smaller for asylum seekers exposed in childhood than for asylum seekers from the same nationality and born after the war. With this respect, a reassuring pattern in our data is the observation in Table 5 of a sharp decrease in crime propensity between cohorts born during conflicts and those born just after. We now go one step further by performing a falsification exercise based on a randomization of conflict exposure during childhood. More specifically, we follow a Monte Carlo approach where we postulate a data generating process that randomly reassigns our main explanatory variable $\textsc{Kid}[1-12]$ across cohorts according to a binomial distribution based on the observed empirical frequencies of 0 and 1. All other cohort characteristics (e.g. nationality, gender, age) are left unchanged. Then, we estimate the baseline specification (Column 2 of Table 3) on this fake dataset. This procedure is generated for a large number of realizations (1,000 draws). Figure 3 reports the sampling distribution of the point estimates of the coefficient of $\textsc{Kid}[1-12]$ across the Monte Carlo draws. Visual inspection shows that this distribution is centered around zero and confirms that the likelihood of spuriously estimating a coefficient equal or above our baseline point estimate of 0.83 is very small.

Cohort $\times$ Canton Sample– The presence of small cohorts potentially leads to sampling variations in the spatial allocation of Asylum Seekers across Swiss Cantons. Hence, in spite of the exogenous allocation, cohorts born after conflict could be by chance located in cantons with different characteristics from cohorts born before (see Online Appendix D). An option for alleviating this concern is to allow for the inclusion of canton $\times$ year fixed effects in our econometric model (1). To this purpose we must disaggregate our cohort-level sample at the canton level. In this case, the dependent variable becomes $\text{CP}_{c,n,g,a,t}$, the crime propensity of cohort n, g, a, t in canton c. We replicate the Table 3 in this more fine-grained setting with the additional set of fixed effects and with the error terms clustered at both nationality $\times$ year and canton $\times$ year levels. The results are reported in Table 18. In all columns the coefficient of interest has the expected positive sign and is most of the time statistically significant. Note however that the R-squared is substantially smaller, indicating a less good fit of this disaggregated specification.

Alternative Victimization Variable– We now focus on another population group that is often victimized in conflict, namely women. We know from Table 4 that, on average, childhood exposure does not impact future violent crime propensity of women. However, it could well be that exposure to extreme events or to violence targeted specifically towards women does affect the criminality risk of women. We restrict our sample to female asylum seekers from countries of origins where there was at least one year of civil war (or mass killing) since 1945 (Table 19). We estimate a version of equation 1 where the variable of exposure to violence corresponds to $\textsc{Women}[1,+]_{n,a}$, a binary variable coded 1 if women from cohort (n, a) have experienced conflict (between birth and residence in Switzerland) and 0 otherwise. Our identification here is consequently based on the comparison between women born before the last year of conflict (civil conflict, mass killing or wartime rape, respectively) and women from the same origin country, born after the last year of conflict (civil conflict, mass killing or wartime rape, respectively), and being from the same wave of migration. On the one hand, women exposed to civil conflict or mass killing are not significantly more violent than women non-exposed (columns 1 to 3, Table 19) but on the other hand women exposed to a conflict with systematic wartime rape are more crime prone than women who are non exposed (column 4). From columns 5 to 7, we turn to a more accurate assessment of the intensity of past exposure, following the same strategy as Table 6. The results indicate that exposure to high-intensity conflict has a stronger impact on criminal behavior than exposure to medium or low intensity conflict.

5 Bilateral Crime Regressions

In this section we exploit a unique feature of our dataset on criminality in Switzerland, namely information on the nationalities of both perpetrators *and* victims. We use this source of information to build bilateral crime propensities documenting the propensity of perpetrators of a given nationality to target victims from specific nationalities.[19] Crucially, this makes possible the inclusion of cohort fixed effects, resulting in the causal inference being purely based on bilateral characteristics –an approach grounded in the gravity trade literature. As shown in detail below, our main result is that, everything else equal, the likelihood that perpetrators and victims are co-nationals is larger among asylum seekers who have been exposed to civil conflicts or mass killings during childhood.

When building bilateral crime propensities we keep on looking at asylum seekers on the perpetrator side. For potential victims we consider all nationals of a given country living in Switzerland, whatever their status (i.e. asylum seekers, migrants and natives). Our cohort-level sample is crossed with the nationality of victims such that the unit of observation is now a cohort of perpetrators of nationality (n) × gender (g) × age group (a) × year (t) targeting victims of nationality (v). We estimate the following bilateral version of our crime equation (1):

$$\begin{aligned} \text{CP}_{n,g,a,v,t} \quad = \quad & \alpha_0 \times \mathbb{I}_{n=v} + \alpha_1 \times (\text{K}\textsc{id}\ [1\text{-}12]_{n,a,t} \times \mathbb{I}_{n=v}) \\ & + \sum_{k=13}^{k=80+} \beta(k) \times (\textsc{expo}(k)_{n,a,t} \times \mathbb{I}_{n=v}) \\ & + \mathbf{FE}_{n,a,t} + \mathbf{FE}_{v,t} + \mathbf{FE}_{g} + \varepsilon_{n,g,a,v,t}, \end{aligned} \tag{2}$$

where $\text{CP}_{n,g,a,v,t}$ corresponds to bilateral propensity to violent crime and $\mathbb{I}_{n=v}$ is a binary indicator function equal to 1 if perpetrator and victim are co-national. Our focus now lies on

[19]Note that our crime data does not contain any information on the ethnic group of the perpetrator and victim of an act of crime, which means that we need to focus on the general crime propensity among co-nationals, without being able to construct an inter-ethnic crime propensity.

the interaction term KID $[1\text{-}12]_{n,a,t} \times \mathbb{I}_{n=v}$. Finding a positive coefficient α_1 means that there is an over-propensity to target co-nationals for cohorts exposed during childhood. Notice that the variables coding for past exposure at later age, $\text{EXPO}(k)_{n,a,t}$, are also interacted.

Very importantly, the bilateral nature of the data allows to go beyond the battery of fixed effects already present in our benchmark crime regression (1). In fact, besides including $\mathbf{FE}_{v,t}$ that are fixed effects in the nationality of victim × year dimension, we are now also able to include $\mathbf{FE}_{n,a,t}$ that are nationality of perpetrator × age × year fixed effects. This is possible because, for a given cohort of perpetrator (n, a, g, t), we observe within-cohort variations in bilateral crime propensities across nationality of victims. From a methodological perspective, a key element is that all the cohort-specific unobserved heterogeneity is now absorbed by the fixed effects and so causal inference relies on bilateral characteristics only. Finally, standard errors are clustered at the nationality of perpetrator × year level.

Table 7: Bilateral Crime Regressions

	(1)	(2)	(3)	(4)	(5)
Dep. var.	Violent Crime Propensity		Violent Crime Propensity - No Family		
Exposure to	CC, MK	CC, MK	CC, MK	CC	MK
$\mathbb{I}_{n=v}$	0.880***	0.880***	0.422**	0.642***	0.782***
	(0.193)	(0.193)	(0.172)	(0.225)	(0.198)
KID $[1\text{-}12]_{n,a,t}$	-0.006				
	(0.004)				
KID $[1\text{-}12]_{n,a,t} \times \mathbb{I}_{n=v}$	1.409***	1.408***	0.813***	0.636**	1.323**
	(0.358)	(0.358)	(0.257)	(0.287)	(0.602)
Nationality of perpetrator × Year FE	Yes	No	No	No	No
Nationality of perpetrator × Year × Age FE	No	Yes	Yes	Yes	Yes
Nationality of victim × Year FE	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	No	No	No	No
Gender FE	Yes	Yes	Yes	Yes	Yes
Observations	814,869	814,869	814,869	687,400	308,838
R-squared	0.027	0.030	0.017	0.019	0.025
Sample mean Bilateral Crime Prop.	0.024	0.024	0.024	0.027	0.034
Sample mean Bil. Crime Prop. (co-national only)	1.60	1.60	1.60	1.85	2.72
Sample mean Bil. Crime Prop. (others)	0.014	0.014	0.014	0.017	0.018

Note: OLS estimations. Robust standard errors are clustered at the nationality × year level. *** p<0.01, ** p<0.05,* p<0.1. Column 1 includes gender fixed effects, age group fixed effects, nationality of perpetrator × year fixed effects, nationality of victim × year fixed effects and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$, both in levels and interacted with $\mathbb{I}_{n=v}$. In columns 2 to 5, we include gender fixed effects, nationality of victim × year fixed effects, nationality of perpetrator × age × year fixed effects and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$ interacted with $\mathbb{I}_{n=v}$. The group of reference is people born after the last years of violence. The dependent variable *Violent Crime Propensity* corresponds to bilateral propensity to violent crime. $\mathbb{I}_{n=v}$ is a binary indicator function equal to 1 if perpetrator and victim are co-nationals. KID [1-12] is a binary measure of childhood exposure to civil conflict or mass killing.

The estimates of equation (2) are reported in Table 7. In column 1 we start with a slightly less demanding specification where we control only for nationality of perpetrator × year fixed effects (rather than $\mathbf{FE}_{n,a,t}$). This means that we can also include the linear term KID $[1\text{-}12]_{n,a,t}$. The coefficient of $\mathbb{I}_{n=v}$ is positive and statistically significant. Note that its magnitude is very large: the violence premium of co-nationality is equal to 0.88 percentage point, thirty times the sample mean of bilateral crime propensity (equal to 0.024 percent). The interpretation is that co-nationals are much more likely to be in a perpetrator-victim relationship. This could be due to the fact that co-nationals may interact particularly often (e.g. common language or overlapping social network), which increases the potential for disputes. Interestingly, we find that the linear term of Kid [1-12] is not statistically significant while the coefficient of its interaction term is positive and statistically significant at the 1 percent level. Hence conflict exposure during childhood drives up the violence risk later in life only in relationships with co-nationals. Here again, the effect is large: The violence premium of co-nationality increases from 0.88 for non-exposed cohorts to 2.28 percent for conflict-exposed cohorts. In column 2 we include the vector of nationality of perpetrator × age × year fixed effects and this absorbs the linear term Kid [1-12]. The magnitude and significance of our two variables of interest are unchanged. From column 3 to 5, we exclude violence against family from the construction of bilateral crime propensity. The variables of interest are still of the expected sign and highly significant for this alternative definition of violent crime (column 3). This shows that our results are not only due to war exposure driving up domestic violence, but that indeed general violent crimes against co-nationals become more frequent, in line with the theories discussed above on conflict and the depletion of social capital. Columns 4 and 5 replicate column 3, but separately for civil conflict and mass killings exposure. In both cases the variables of interest have both the expected positive sign and are statistically significant.[20]

Both the signs and the magnitudes of these estimation results can be interpreted as evidence of persistence in targeted violence. Note that (un-)observed characteristics of conflict exposed cohorts are filtered out by the nationality of perpetrator × age × year fixed effects. Henceforth the results cannot be driven by channels such as conflict-induced selection into migration or post-conflict educational dropout or pervasive developmental disorders or changes in the composition of the population. In contrast, among others, theories stressing that past conflicts damage social ties between co-nationals and that distrust and grievances acquired in early childhood persist over the lifetime are consistent with our findings. In particular, Rohner et al. (2013) and Acemoglu and Wolitzki (2014) argue that war leads to a collapse of inter-ethnic trust which in turn sows the seeds of more ethnic inter-group conflict in the future. Taking this theory literally, one should not only expect a general tendency for war-exposed individuals to commit more crimes in post-war contexts, but, on top of this, to be particularly frequently involved in committing crimes that target co-nationals. For example for the diaspora of migrants from Sri-Lanka living in Switzerland many incidents of "imported conflicts" between different ethnic political movements have been documented, in which groups that fought against each other in their homeland still have conflicted interactions many years later when living in Switzerland (Moret et al., 2007).

[20] The inclusion of dyadic fixed effects leaves our main results unchanged (Online Appendix Table 20).

6 Combating the Legacy of Conflict: The Role of Policies

In this section we study how institutions in the host country modulate the impact of past exposure to conflict on current criminality of asylum seekers. Our question of interest is whether the "right" design of institutions and policies can partly or fully alleviate the risk of increased criminality for exposed individuals. This question is of foremost policy relevance: Often anti-asylum political movements and populist parties use fear of crime as a major argument against offering protection to refugees. If the optimal integration policies are able to substantially curb the risk of crime driven by past conflict exposure, arguments against open door policies and humanitarian help would be under severe pressure.

As described above, the key players in the asylum process are the Swiss cantons. The decentralized, federalist constitution of Switzerland guarantees substantial autonomy to the cantons, which results in large cross-canton heterogeneity of integration policies. Because asylum seekers are early on allocated by the federal state to different cantons, their incentives for engaging in crime or abiding the law are consequently shaped by the various local policies that cantons put in place. Notice that due to the absence of a randomization scheme in the implementation of policies at the canton-level, our exercise of policy evaluation can barely go beyond correlations. Though limited, this preliminary evidence is, to our knowledge, new to the literature and fills a gap by documenting how public policies can tackle the recurrence of violence in the aftermath of conflict.

The reference model of crime has been proposed by Becker (1968): the decision to commit a crime is driven by the opportunity cost (legal labor market salaries) relative to returns to crime discounted by the probability of being caught by the police and the sanction imposed by the criminal justice system. Following a Beckerian logic of potential criminals trading off the gains and costs of committing a crime, we now focus on cantonal policies that are likely to shape asylum seekers' incentives. To this purpose we need to operate again with our cohort-level sample disaggregated at the canton level (see Online Appendix Table 18). We now simply proceed by including in the disaggregated version of equation (1) an interaction term between our main explanatory variable, Kid $[1\text{-}12]_{n,a,t}$, and various indicators of cantonal policies. The linear terms of the cantonal policies are absorbed by the canton×year fixed effects. Like in Online Appendix Table 18, we include gender fixed effects, age group fixed effects, canton × year fixed effects, nationality × year fixed effects and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$. In addition, we also control for the interaction of our set of indicators of cantonal policies with the set of binary variables coding for past exposure at later ages k.

Tables 8 and 9 report the estimation results and Figures 4 and 5 in the Online Appendix display some marginal effects. Note that for ease of interpretation, we normalize all continuous variables by their average level. In all specifications standard errors are two-way clustered at the canton×year and nationality×year levels. The definitions of all cantonal policies and control variables are relegated to Online Appendix F and summary statistics are displayed in Online Appendix Table 21.

Labor Market Integration– We start in Table 8 with policies related to the labor market integration of asylum seekers. A main factor affecting the opportunity cost of engaging in criminal activity is indeed the outside option of paid employment.[21] This factor is particularly salient for asylum seekers who are often severely cash constrained and many of which have indebted themselves heavily to finance the travel to Europe.

In column 1 we interact conflict exposure with two mutually exclusive labor market features. $\text{Open job access}_c = 0$ codes for cantons where asylum seekers are not allowed to rapidly search for paid employment and where there are cantonal constraints and bans from paid work. In contrast, $\text{Open job access}_c = 1$ corresponds to cantons where asylum seeker are allowed to rapidly search for paid employment. The results show that in cantons without rapid labor market access past exposure to conflict statistically significantly increases the crime propensity, while in cantons with open job access there is no significant effect of exposure on crime propensity.

Not only the *de jure* job access is important, but also the help provided for getting integrated in the job market. In column 2 we thus focus on the policy variable $\text{Promote job market}_c$ that takes a value of 1 for all cantons that offer active promotion services for labor market access, and zero otherwise. The regression results stress that conflict exposure only boosts crime propensity in the absence of active job promotion. Column 3 is dedicated to $\text{Professional training}_c$ that codes for cantons offering at least one measure promoting professional training, such as coaching, training, internships. Here also we find that past conflict exposure leads to an increase in crime propensity only in cantons where professional training is not provided. Note that the null hypothesis of the F-test, i.e. the equality of the coefficient estimates on the ($\text{Kid [1-12]} = 1 \times \text{policy}_c = 0$) and ($\text{Kid [1-12]} = 1 \times \text{policy}_c = 0$), is rejected only in column 2.

These first three columns contain measures that do not only correlate with actual employment rates, but also affect behavior through encouragement and aspirations, even for those who have not obtained an employment yet. Still, it is also interesting to consider more narrow variables capturing very directly the *de facto* employment rates. Columns 4 and 5 report the results and Figure 4 displays the marginal effects. In column 4 war exposure is interacted with $\text{Occupation rate}_{n,c,t-1}$ at the level of the nationality-canton in the previous year. For example for Afghans in the canton of Zurich in 2012 their occupation variable corresponds to the average occupation rate of Afghans in the canton of Zurich in 2011. We find that larger occupation rates tend to eliminate the crime-inducing effect of conflict exposure. This raw measure of occupation rate has the virtue of precision

[21] This issue has received considerable attention in public debates and the press (`http://www.nytimes.com/2015/09/18/business/international/migrants-refugees-jobs-germany.html?_r=0`, and `http://www.economist.com/news/leaders/21662547-bigger-welcome-mat-would-be-europes-own-interest-let-them-and-let-them-earn`). Also the academic literature on crime has found that labor market access critically affects criminal behavior (see Draca and Machin (2015) and Freeman (1999) for reviews of the literature). Fougère et al. (2009) for France and Gould et al. (2002) for the US find a positive association between youth crime and youth unemployment, while Gould et al. (2002) show that the negative effect of earnings on crimes is stronger than the effect of employment. Further, Machin and Meghir (2004) use a wage measure for the low-skilled and find that an increase in this measure corresponds to a fall in the crime rate. Bell et al. (2014) analyze the link between recessions and crimes and find that young people who leave school in the midst of recessions are significantly more likely to become criminals than those who do not. Finally, Bell et al. (2013) find that asylum seeker inflows may have a positive effect on the crime rates in the UK, while economic migrant inflows (with arguably better labor market access) have no such effect.

but it could potentially suffer from endogeneity bias, as underlying cohort characteristics could affect both the occupation rate and the crime propensity. Hence, in column 5 we focus on a slightly different measure that corresponds to the average occupation rate of asylum seekers from other nationalities in the same canton in the previous year. It is less precise but more exogenous because the nationality of interest is excluded from the sample of nationalities used to compute the average. For example for Afghans in Zurich in 2012 the value of this variable corresponds to the average occupation rates of non-Afghan asylum seekers in Zurich in 2011. The interaction term of interest has the expected negative sign though not statistically significant. However visual inspection of the marginal effects (Figure 4) confirms that larger occupation rates eliminate the crime-inducing effect of conflict exposure.

Table 8: Impact of Integration Policies: Labor Market

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Violent Crime Propensity				
POLICY$_c$:	Open job market	Promote job market	Professional training		
OCCUPATION RATE				Raw measure	Index
KID [1-12] = 1 × POLICY$_c$ = 0	0.960**	0.589*	0.660*		
	(0.470)	(0.332)	(0.394)		
KID [1-12] = 1 × POLICY$_c$ = 1	0.235	-0.311	0.177		
	(0.272)	(0.333)	(0.327)		
KID [1-12]				0.944**	0.940*
				(0.475)	(0.506)
KID [1-12]×OCCUPATION RATE				-5.146*	-5.068
				(3.050)	(3.345)
Gender FE	Yes	Yes	Yes	Yes	Yes
Age Group FE	Yes	Yes	Yes	Yes	Yes
Nationality x Year FE	Yes	Yes	Yes	Yes	Yes
Canton x Year FE	Yes	Yes	Yes	Yes	Yes
Observations	28,404	25,056	25,056	28,404	27,321
R-squared	0.215	0.222	0.222	0.215	0.217
F-test equality coefficients	2.644	4.686	1.124		
	(0.107)	(0.0332)	(0.292)		

Note: OLS estimations weighted by the number of individuals in each cohort. Robust standard errors are two-way clustered at nationality × year and canton × year levels. *** p<0.01, ** p<0.05,* p<0.1. All estimations include gender fixed effects, age group fixed effects, canton × year fixed effects, nationality × year fixed effects and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$ and the interactions with the additional controls. The group of reference is people born after the last years of violence. The dependent variable *Violent Crime Propensity* stands for the violent crime propensity of a cohort of nationality $(n)\times$ canton $(c)\times$ gender $(g)\times$ age bracket $(a)\times$ year (t). KID [1-12] is a binary measure of childhood exposure to civil conflict or mass killing. The F-test (and p-value) reports whether the estimates of (KID [1-12] = 1 × POLICY$_c$ = 0) and (KID [1-12] = 1 × POLICY$_c$ = 1) are statistically significantly different.

Social Integration and Financial Support– Labor market integration is intertwined with social integration, and one can expect social integration to reduce the crime risk directly by increasing the opportunity cost of unlawful behavior, as well as indirectly by improving labor market outcomes. We thus focus in Table 9 on measures of social integration and financial support.

In column 1 we interact conflict exposure with CIVIC AND LANGUAGE COURSES$_c$ that codes for cantons where both language and civic education courses are offered. The rationale is that knowing well the language and social norms of the host country is an important factor affecting the social and economic integration. Strikingly, the crime inducing impact of conflict exposure is only present in cantons that do not jointly offer language and civic education courses.

In column 2 we investigate how the method of management of asylum centers and level of funding affect crime incentives. The variable PRIVATE MANAGEMENT$_c$ takes a value of 1 in cantons where at least one cantonal or municipal asylum accommodation is run by a private firm, and zero otherwise.[22] One reason for cantons to outsource asylum services to private firms is to save on costs, and in the Swiss media there has been a considerable controversy on whether quality standards are guaranteed when private for-profit companies run asylum structures.[23] The coefficient of the interaction between KID [1-12] and PRIVATE MANAGEMENT$_c = 1$ is positive and significant, while it is not significant for the interaction with PRIVATE MANAGEMENT$_c = 0$, suggesting that private asylum center management goes along with a higher crime propensity, while this is not the case for non-private accommodation. Our data however do not allow us to identify whether this is due to specific management principles applied by private firms (e.g. cutting costly integration programs to maximize benefits), or whether this effect is rather due to a general willingness from the canton side to cut costs.

We turn in column 3 to a more direct measure of financial support provided by public administrations to asylum seekers. The variable SOCIAL ASSISTANCE$_{c,t}$ corresponds to the log of total public money attributed per asylum seeker per month in a given canton c and year t. More financial assistance and better funded asylum centers are expected to increase the opportunity cost of crime by making less attractive the risk of being expelled from a better material situation in case of crime conviction. Hence, we expect more financial assistance and better funded accommodation structures to deter crime. Surprisingly, neither the coefficient of the interaction term nor the marginal effects (in Figure 5) are statistically significant.

Another important aspect of integration policies relates to the outcome of the demand for political asylum: Presumably asylum seekers with a serious chance of obtaining political asylum have higher incentives to abide the law in order to maximize their asylum chances as compared to asylum

[22]The reason we are unable to code a continuous variable is that for several cantons we only know if private firms operate but the data on the number of asylum centers with and without private management is missing. Many cantons also have a huge diversity of types of accommodation, ranging from private flats to large centers. Computing some average share of privately run centers in the absence of exhaustive information on their capacity could result in substantial measurement error. For the few cantons with detailed information, the share of privately run centers was either zero or very large, which suggest that working with a dummy variable is appropriate.

[23]See, for example, a recent article in the Swiss daily newspaper *Tages-anzeiger*, `http://www.tagesanzeiger.ch/schweiz/standard/den-lohn-von-fluechtlingen-eingezogen/story/12513956`.

seekers whose asylum demand has no chance. We consider the variable ACCEPTANCE RATE$_{n,t}$ that corresponds to the percentage of recognized refugees (residence B permit) among asylum seeker demands, by nationality n in year t: Its interaction term with war exposure in column 4 has the expected negative sign, but is not statistically significant. Figure 5 displays the marginal effects.

Related to the outcome of the demand for political asylum is the existence, for a subset of nationalities, of bilateral readmission agreements between Switzerland and the country of origin. When an asylum seeker sees her application for refugee status rejected and is from a country with a readmission agreement, Switzerland is able to send her back to her country of origin by force. In contrast, people originating from countries without readmission agreement do not face this threat, as Switzerland is not able to expel them by force even if their asylum application has been rejected. We code a binary variable, READMISSION$_n$, that is equal to 1 if there exists such a bilateral readmission agreement for nationality n. Readmission agreements can be expected to have countervailing effects, as on the one hand they increase the potential consequences of breaking the law and jeopardizing the asylum application, while on the other hand they shorten the ex ante expected time horizon in Switzerland, and hence the payoff of cooperation in a repeated setting. As far as the overall effect is concerned, as shown in column 5, the coefficient of this variable has a positive sign, but is not statistically significant. However, it is possible to disentangle the two countervailing forces. Imagine someone with very low chances of being granted refugee status. In this case having a readmission agreement shortens the time horizon in Switzerland and hence reduces the incentives for cooperation. Thus it unambiguously increases the crime risk, and we should expect a positive coefficient for readmission. Now consider someone with very high chances of obtaining refugee status in Switzerland. When abiding the law, the person is likely to be able to stay in Switzerland in the long-run, while when becoming criminal she may be expelled if a readmission agreement exists. Hence, a readmission agreement in this case means that the price of being caught is higher, and hence it lowers the crime incentives. One should in this case expect a negative coefficient of readmission. To check this, we implement the triple interaction of conflict exposure with the acceptance rate and readmission agreement variables. As shown in column 6, this triple interaction term is negative and statistically significant, which suggests that readmission agreements drive up crime incentives when acceptance chances are low, but that readmission agreements lower crime when acceptance chances are high, exactly as predicted by the Beckerian logic outlined above.

Table 9: Impact of Integration Policies: Social and Financial Integration

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable		Violent Crime Propensity				
POLICY$_c$:	Civic & language courses	Private management				
KID [1-12] $= 1 \times$ POLICY$_c = 0$	0.732*	0.029				
	(0.418)	(0.325)				
KID [1-12] $= 1 \times$ POLICY$_c = 1$	0.029	0.789*				
	(0.347)	(0.434)				
KID [1-12]			0.421	0.534	0.116	0.092
			(0.348)	(0.337)	(0.286)	(0.320)
KID [1-12] $\times$ SOCIAL ASSISTANCE			0.921			
			(2.251)			
KID [1-12] $\times$ ACCEPTANCE RATE				-1.380		1.069
				(2.149)		(2.127)
KID [1-12] $\times$ READMISSION					1.088	0.288
					(0.704)	(0.893)
KID [1-12] $\times$ ACCEPTANCE RATE $\times$ READMISSION						-14.783**
						(5.976)
Gender FE	Yes	Yes	Yes	Yes	Yes	Yes
Age Group FE	Yes	Yes	Yes	Yes	Yes	Yes
Nationality x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Canton x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	25,056	28,404	28,404	28,404	28,404	28,404
R-squared	0.223	0.215	0.214	0.215	0.216	0.217
F-test equality coefficients	1.968	1.709				
	(0.164)	(0.194)				

Note: OLS estimations weighted by the number of individuals in each cohort. Robust standard errors are two-way clustered at nationality $\times$ year and canton $\times$ year levels. *** p<0.01, ** p<0.05,* p<0.1. All estimations include gender fixed effects, age group fixed effects, canton $\times$ year fixed effects, nationality $\times$ year fixed effects and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$ and the interactions with the additional controls. The group of reference is people born after the last years of violence. The dependent variable *Violent Crime Propensity* stands for the violent crime propensity of a cohort of nationality $(n)\times$ canton $(c)\times$ gender $(g)\times$ age bracket $(a)\times$ year (t). KID [1-12] is a binary measure of childhood exposure to civil conflict or mass killing. The F-test (and p-value) reports whether the estimates of (KID [1-12] $= 1 \times$ POLICY$_c = 0$) and (KID [1-12] $= 1 \times$ POLICY$_c = 1$) are statistically significantly different.

7 Economic Migrants

For the purpose of causal identification our analysis has focused so far on asylum seekers because this type of migrants are exogenously allocated across cantons. Nevertheless, it could be argued that this population has peculiar (un)observed characteristics – in term of migration incentives and background – and this could cast some doubt on the external validity of our analysis. To check if our results apply to broader contexts as well, we replicate our baseline estimates focusing on economic migrants.

The group of migrants holding B/C permits represents 22.2% of the total Swiss population in 2012 (1.8 million people out of 8 millions), which makes this a particularly powerful external validity check.[24] Economic migrants are not allocated exogenously across the cantons, they are completely free to settle wherever they want in Switzerland, i.e crime-prone individuals can self-select into crime-facilitating environment. To alleviate this concern we include canton × year fixed effects in our baseline equation (1) and this leads us to operate again with our cohort-level sample disaggregated at the canton level. Hence, the dependent variable becomes $\text{CP}_{c,n,g,a,t}$, the crime propensity of cohort n, g, a, t in canton c.

In our sample, we observe on average 1,436,172 economic migrants from 187 nationalities over the 2009-2012 period. These individual observations are aggregated by gender, age group, canton and nationality, for each year. This leaves us with 254,796 cohorts, composed on average of 22 individuals. Table 10 reports the main descriptive statistics for cohorts of economics migrants. The number of cohorts who originate from countries that have experienced at least one episode of civil conflict or mass killings since 1946 is lower than the number of cohorts of asylum seekers (61% vs 84%). The sample mean of conflict exposure during childhood, Kid [1-12], is also lower for economic migrants, 33.5% vs 48% for asylum seekers. Finally, economic migrants are also less violent: The sample mean of their violent crime propensity is equal to 0.95 percent, roughly one half of the average crime propensity of Asylum seekers. This is in line with the results of the previous Section about the crime-reducing impact of labor market access.

Baseline Results: Economic Migrants – Table 11 replicates a canton-level version of the baseline Table 3 with the sample of economic migrants. All specifications include gender, age group and canton × year fixed effects. We report only our coefficient of interest that captures the impact on violent crime propensity of cohorts exposed to civil war or mass killings during childhood (1-12 years), the reference group being cohorts born after conflict. Given the large increase in the sample size, the coefficients are very precisely estimated: Throughout the columns, the coefficient of Kid [1-12] is positive and strongly significant at the 1 percent threshold. More importantly, we see that the magnitude of the effect remains sizeable. From Column 2, the violence premium of conflict exposure during childhood amounts to 0.345 percentage points –a substantial effect given that the sample mean of violent crime propensity is equal to 0.95 percentage points. This pre-

[24] Source: FSO - Foreign Resident Population Statistics (PETRA) and Population and Households Statistics (STATPOP).

Table 10: Cohorts of Economic Migrants - Summary Statistics

variable	mean	sd	max	min
Male	51.2	49.9	100	0
Cohort Size (# individuals)	22.5	103.7	6103	1
Civil Conflict & Mass Killing	61.4	48.6	100	0
Wartime Rape	23.5	42.4	100	0
Distance to last CC or MK (years)	15.2	16.3	65	0
KID [1-12]	33.5	47.2	100	0
KID [1-12] (ONLY CC)	32.2	46.7	100	0
KID [1-12] (ONLY MK)	10.4	30.6	100	0
WOMEN[1,+] (WAR. RAPE)	23.2	42.2	100	0
$\mathrm{CP}_{n,g,a,t}$ (Violent Crime Propensity)	0.95	7.12	100	0

Note: Sample of 254,796 cohorts of economics migrants, 187 nationalities, 14 age brackets, 2009-2012. Except for cohort size and distance to last CC or MK, all figures represent percentages.

mium, although smaller in absolute terms, is comparable in relative terms to the violence premium estimated for asylum seekers: Exposed cohorts of economic migrants are 36% (= 0.345/0.95) more violent than the sample mean; while exposed cohorts of asylum seekers are 40% (= 0.833/2.04 in baseline Table 3, col. 2) more violence prone than the sample mean.

Targeted Violence: Economic Migrants – We now document the persistence of intra-national violence following the logic of sub-section 5. Table 12 replicates the bilateral crime regressions of Table 7 with the sample of economic migrants. Regarding targeted violence we focus for perpetrators on economic migrants only, while for potential victims we take into account all nationals of a given country living in Switzerland (i.e. asylum seekers, migrants and natives). Here again the large increase in the sample size enables us to get precise estimates. We see that the variables of interest have both the expected positive sign and are statistically significant. From a qualitative and quantitative perspective, the results are also similar to what we obtain with asylum seekers: Violent crimes mostly target co-nationals and this detrimental effect of co-nationality is exacerbated for cohorts exposed to civil conflicts and mass killing during their childhood. More precisely, the violence premium of co-nationality amounts to 0.163 percentage points, 4 times the sample mean of bilateral crime propensity (equal to 0.04 percent). And this premium is more than tripled for conflict-exposed cohorts (rising from 0.163 to 0.542 percent).

Overall these two external validity tables suggest that the main findings of the current paper may generalize to a broader context than just that of the group of asylum seekers.

Table 11: Economic Migrants: Crime Propensities and Conflict Exposure

	(1)	(2)	(3)	(4)	(5)
Dependent Variable			Violent Crime Propensity		
Sample	Full	Full	CC & MK	CC	MK
Kid [1-12]	0.529***	0.345***	0.312***		
	(0.091)	(0.063)	(0.053)		
Kid [1-12] (Only CC)				0.315***	
				(0.055)	
Kid [1-12] (Only MK)					0.328***
					(0.087)
Gender FE	Yes	Yes	Yes	Yes	Yes
Age Group FE	Yes	Yes	Yes	Yes	Yes
Canton × Year FE	Yes	Yes	Yes	Yes	Yes
Nationality × Year FE	No	Yes	Yes	Yes	Yes
Observations	254,796	254,787	156,455	153,776	57,985
R-squared	0.081	0.111	0.113	0.114	0.137
Sample mean Crime Prop.	0.95	0.95	1.24	1.24	1.50

Note: OLS estimations weighted by the number of individuals in each cohort. Robust standard errors are two-way clustered at nationality × year and canton × year levels. *** p<0.01, ** p<0.05,* p<0.1. All estimations include gender fixed effects, age group fixed effects, canton × year fixed effects and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$. The group of reference is people born after the last years of violence. The dependent variable *Violent Crime Propensity* stands for the violent crime propensity of a cohort of nationality $(n)\times$ gender $(g)\times$ age bracket $(a)\times$ canton $(c)\times$ year (t). Kid [1-12] is a binary measure of childhood exposure to civil conflict or mass killing (columns 1 to 3), to civil conflict only (column 4), to mass killing (column 5). In columns 3 to 5, the sample is restricted to cohorts originating from countries that have experienced civil conflict or mass killing since 1946.

Table 12: Economic Migrants: Bilateral Crime Regressions

	(1)	(2)	(3)	(4)	(5)
Dep. var.	Violent Crime Propensity		Violent Crime Propensity- No Family		
Exposure to	CC, MK	CC, MK	CC, MK	CC	MK
$\mathbb{I}_{n=v}$	0.163***	0.163***	0.082***	0.123***	0.201***
	(0.018)	(0.018)	(0.009)	(0.016)	(0.032)
Kid $[1\text{-}12]_{n,a,t}$	0.000				
	(0.001)				
Kid $[1\text{-}12]_{n,a,t} \times \mathbb{I}_{n=v}$	0.379***	0.379***	0.236***	0.193***	0.258***
	(0.054)	(0.054)	(0.037)	(0.039)	(0.081)
Nationality of perpetrator × Year FE	Yes	No	No	No	No
Nationality of perpetrator × Year × Age FE	No	Yes	Yes	Yes	Yes
Nationality of victim × Year FE	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	No	No	No	No
Gender FE	Yes	Yes	Yes	Yes	Yes
Observations	7,166,768	7,166,768	7,166,768	4,057,788	1,211,280
R-squared	0.005	0.008	0.026	0.028	0.040
Sample mean Bilateral Crime Prop.	0.04	0.04	0.03	0.004	0.005
Sample mean Bil. Crime Prop. (co-national only)	0.27	0.27	0.14	0.21	0.31
Sample mean Bil. Crime Prop. (others)	0.003	0.003	0.002	0.003	0.004

Note: OLS estimations. Robust standard errors are clustered at the nationality × year level. *** p<0.01, ** p<0.05,* p<0.1. Column 1 includes gender fixed effects, age group fixed effects, nationality of perpetrator × year fixed effects, nationality of victim × year fixed effects and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$, both in levels and interacted with $\mathbb{I}_{n=v}$. In columns 2 to 5, we include gender fixed effects, nationality of victim × year fixed effects, nationality of perpetrator × age × year fixed effects and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$ interacted with $\mathbb{I}_{n=v}$. The group of reference is people born after the last years of violence. The dependent variable *Violent Crime Propensity* corresponds to bilateral propensity to violent crime. $\mathbb{I}_{n=v}$ is a binary indicator function equal to 1 if perpetrator and victim are co-nationals. Kid [1-12] is a binary measure of childhood exposure to civil conflict or mass killing.

8 Conclusion

This paper shows that exposure to civil conflict and mass killings during childhood (1-12 years) has a robust, strong and persistent magnifying effect on the propensity to violent crime later in life. The baseline causal analysis is conducted with the population of asylum seekers in Switzerland over the 2009-2012 period but similar conclusions are reached when the analysis is extended to the population of economic migrants. Contrary to previous case studies of post-conflict reconstruction, our empirical strategy, by focusing on migrants, allows to show that this persistence in violence is not due to persistently bad institutions in post-conflict states, but that it continues to hold even when people live in an environment with fully functional institutions (Switzerland).

Do the results of the paper advocate a more restrictive immigration policy? Not by any means. Still, the legitimate need for protection of refugees needs to be traded-off against the legitimate demand for domestic security. Hence, while a generous asylum policy should in our view be maintained, our results suggest that it is equally crucial to accompany well those welcomed in the host country. By installing a well-funded asylum system that offers rapid access to local labor markets and provides future perspectives and opportunities for integration, the risk of an increase in crime perpetrated by migrants with conflict background can be well contained.

Our findings may also have implications for post-conflict reconstruction. The result on the key role of labor market integration to break vicious cycles of persistent violence may apply more generally (as recently investigated by Blattman and Ralston, 2015), and calls for a particularly strong involvement and support from donor countries in the first crucial years following conflict. While for example the Marshall Plan has surely made a key contribution to Germany's "Wirtschaftswunder" post-1945 and paved its way to stable democracy, this lesson may have been forgotten in recent time, as witnessed by the ill-fated precipitated withdrawal from post-Gaddafi Libya. More research on the role of jobs and long-run perspectives for post-conflict reconstruction should be encouraged.

References

[1] Acemoglu, Daron, and Alexander Wolitzky, 2014, "Cycles of Conflict: An Economic Model", *American Economic Review* 104: 1350-1367.

[2] Adhvaryu, Achyuta, and James Fenske, 2014, "Conflict and the Formation of Political Beliefs in Africa", HiCN Working Paper 164.

[3] Akresh, Richard, and Damien de Walque, 2010, "Armed Conflict and Schooling: Evidence from the 1994 Rwandan Genocide", mimeo, University of Illinois at Urbana-Champaign.

[4] Almas, Ingvild, Alexander Cappelen, Erik Sorensen, and Bertil Tungodden, 2010, "Fairness and the Development of Inequality Acceptance", *Science* 328: 1176-1178.

[5] Angrist, Joshua D., and Jörn-Steffen Pischke, 2009, *Mostly Harmless Econometrics: An Empiricist's Companion*, Princeton NJ, Princeton University Press.

[6] Barenbaum, Joshua, Vladislav Ruchkin, and Mary Schwab-Stone, 2004, "The Psychological Aspects of Children Exposed to War: Practice and Policy Initiatives", *Journal of Child Psychology and Psychiatry* 45: 41-62.

[7] Bauer, Michal, Christopher Blattman, Julie Chytilov, Joseph Henrich, Edward Miguel, and Tamar Mitts, 2016, "Can War Foster Cooperation?", *Journal of Economic Perspectives* 30: 249274.

[8] Bauer, Michal, Julie Chytilova, and Barbara Pertold-Gebicka, 2014, "Parental Back-ground and Other-regarding Preferences in Children", *Experimental Economics* 17: 24-46.

[9] Bauer, Michal, Nathan Fiala and Ian Levely, 2015, "Trusting Former Rebels: An Experimental Approach to Understanding Reintegration after Civil War", mimeo, CERGE-EI.

[10] Beaman, Lori, 2012, "Social Networks and the Dynamics of Labour Market Outcomes: Evidence from Refugees Resettled in the U.S.", *Review of Economic Studies* 79: 128161.

[11] Becker, Gary, 1968, "Crime and Punishment: An Economic Approach", *Journal of Political Economy* 76: 169-217.

[12] Bell, Brian, Francesco Fasani, and Stephen Machin, 2013, "Crime and Immigration: Evidence from Large Immigration Waves", *Review of Economics and Statistics* 95: 1278-1290.

[13] Bell, Brian, Anna Bindler, and Stephen Machin, 2014, "Crime Scars: Recessions and the Making of Career Criminals", CEP Discussion Paper No. 1284.

[14] Bellows, John, and Edward Miguel, 2009, "War and Local Collective Action in Sierra Leone", *Journal of Public Economics* 93: 1144-57.

[15] Besley, Timothy, and Marta Reynal-Querol, 2014, "The Legacy of Historical Conflict: Evidence from Africa", *American Political Science Review* 108: 319-336.

[16] Bianchi, Milo, Paolo Buonanno, and Paolo Pinotti, 2012, "Do Immigrants Cause Crime?", *Journal of the European Economic Association* 10: 1318-1347.

[17] Blattman, Christopher, 2009, "From Violence to Voting: War and Political Participation in Uganda", *American Political Science Review* 103: 231-47.

[18] Blattman, Christopher, and Jeannie Annan, 2010, "The Consequences of Child Soldiering", *Review of Economics and Statistics* 92: 882-898.

[19] Blattman, Christopher, and Laura Ralston, 2015, "Generating Employment in Poor and Fragile States: Evidence from Labor Market and Entrepreneurship Programs", working paper, Columbia University.

[20] Borjas, George, 1994, "The Economics of Immigration", *Journal of Economic Literature* 32: 1667-1717.

[21] Borjas, George, 2003, "The Labor Demand Curve *Is* Downward Sloping: Reexamining the Impact of Immigration on the Labor Market", *Quarterly Journal of Economics* 118: 1335-74.

[22] Butcher, Kristin, and Anne Morrison Piehl, 1998, "Cross-city Evidence on the Eelationship between Immigration and Crime", *Journal of Policy Analysis and Management* 17: 457-493.

[23] Card, David, 1990, "The Impact of the Mariel Boatlift on the Miami Labor Market", *Industrial and Labor Relations Review* 43: 245-257.

[24] Card, David, 2001, "Immigration Inflows, Native Outflows, and the Local Market Impacts of Higher Immigration", *Journal of Labor Economics* 19: 22-64.

[25] Cassar, Alessandra, Pauline Grosjean, and Sam Whitt, 2013, "Legacies of Violence: Trust and Market Development", *Journal of Economic Growth* 18: 285-318.

[26] Cohen, Dara, 2013, "Explaining Rape during Civil War: Cross-National Evidence (1980-2009)", *American Political Science Review* 107: 461-477.

[27] Collier, Paul and Anke Hoeffler, 2004, "Greed and Grievance in Civil War", *Oxford Economic Papers* 56: 563-95.

[28] Collier, Paul, Anke Hoeffler, and Dominic Rohner, 2009, "Beyond Greed and Grievance: Feasibility and Civil War", *Oxford Economic Papers* 61: 1-27.

[29] Couttenier, Mathieu, Pauline Grosjean, and Marc Sangnier, 2014, "The Wild West *is* Wild: The Homicide Resource Curse", mimeo, University of Lausanne.

[30] Damm, Anna Piil, and Christian Dustmann, 2014, "Does Growing up in a High Crime Neighborhood Affect Youth Criminal Behavior?", *The American Economic Review*, 104: 1806-1832.

[31] Département fédéral de justice et police (DFJP), 2011, "Rapport sur des mesures d'accélération dans le domaine d'asile", Confédération Suisse, available under `https://www.bfm.admin.ch/content/dam/data/migration/rechtsgrundlagen/gesetzgebung/asylg-aug/ersatz-nee/ber-beschleunig-asyl-f.pdf`.

[32] Derluyn, Ilse, Eric Broekaert, Gilberte Schuyten, and Els De Temmerman, 2004, "Post-traumatic Stress in Former Ugandan Child Soldiers", *Lancet* 363: 861-3.

[33] DeRouen, Karl, and Jacob Bercovitch, 2008, "Enduring Internal Rivalries: A New Framework for the Study of Civil War", *Journal of Peace Research* 45: 55-74.

[34] Di Tella, Rafael, and Ernesto Schargrodsky, 2004, "Do Police Reduce Crime? Estimates Using the Allocation of Police Forces after a TerroristAttack", *American Economic Review* 94: 115-133.

[35] Draca, Mirko, Stephen Machin, and Robert Witt, 2011, "Panic on the Streets of London: Police, Crime, and the July 2005 Terror Attacks", *American Economic Review* 101: 2157-2181.

[36] Draca, Mirko, and Stephen Machin, 2015, "Crime and Economic Incentives", *Annual Review of Economics* 7: 389-408.

[37] Durlauf, Steven N., Salvador Navarro, and David A. Rivers, 2010, “Understanding Aggregate Crime Regressions”, *Journal of Econometrics* 158: 306-317.

[38] Dustmann, Christian and Oliver Kirchkamp, 2002, “The Optimal Migration Duration and Activity Choice after Re-migration”, *Journal of Development Economics* 67: 351-372.

[39] Dyregrov, Atle, Leila Gupta, Rolf Gjestad, and Eugenie Mukanoheli, 2000, “Trauma Exposure and Psychological Reactions to Genocide Among Rwandan Children”, *Journal of Traumatic Stress* 13: 3-21.

[40] Edin, Per-Anders, Peter Fredriksson, and Olof Aslund, 2003, “Ethnic Enclaves And The Economic Success Of Immigrants-Evidence From A Natural Experiment”, *The Quarterly Journal of Economics* 118: 329-357.

[41] Efionayi-Mäder, Denise, 2011, “Chapitre 7: Asile” in Nicole Wichmann, Michael Hermann, Gianni DAmato, Denise Efionayi-Mder, Rosita Fibbi, Joanna Menet, and Didier Ruedin (eds.), “Les marges de maneouvre au sein du federalisme: La politique de migration dans les canton”, Bern: Comission federale pour les questions de migration.

[42] Ellison, Glenn, and Edward L. Glaeser, 1997, “Geographic Concentration in US Manufacturing Industries: A Dartboard Approach”, *Journal of Political Economy* 105: 889-927.

[43] Fajnzylber, Pablo, Daniel Lederman, and Norman Loayza, 2002, “What causes violent crime?”, *European Economic Review* 46: 1323-1357.

[44] Fearon, James, Macartan Humphreys, and Jeremy Weinstein, 2009, “Can Development Aid Contribute to Social Cohesion after Civil War? Evidence from a Field Experiment in Post-Conflict Liberia”, *American Economic Review* 99: 287-291.

[45] Fearon, James D., and David D. Laitin, 2003, “Ethnicity, Insurgency, and Civil War”, *American Political Science Review* 97: 7590.

[46] Federal Office of Migrations Asylum Statistics 2009-2012, available under `https://www.bfm.admin.ch/bfm/fr/home/publiservice/statistik/asylstatistik/jahresstatistiken.html`.

[47] Federal Statistics Office - Foreign Resident Population Statistics (PETRA) and Population and Households Statistics (STATPOP), available from `http://www.bfs.admin.ch/bfs/portal/en/index/themen/01/07/blank/key/01/02.html`.

[48] Fehr, Ernst, Helen Bernhard, and Bettina Rockenbach, 2008, “Egalitarianism in Young Children”, *Nature* 454: 1079-1083.

[49] Fehr, Ernst, Daniela Rutzler, and Matthias Sutter, 2011, “The Development of Egalitarianism, Altruism, Spite and Parochialism in Childhood and Adolescence”, IZA Discussion Paper No. 5530.

[50] Fisman, Raymond and Edward Miguel, 2007, “Corruption, Norms, and Legal Enforcement: Evidence from Diplomatic Parking Tickets”, *Journal of Political Economy* 115: 1020-1048.

[51] Fougère, Denis, Francis Kramarz, and Julien Pouget, 2009, “Youth Unemployment and Crime in France”, *Journal of the European Economic Association* 7: 909-938.

[52] Freeman, Richard B., 1999, “The Economics of Crime”, Chapter 52 in Orley Ashenfelter and David Card, *Handbook of Labor Economics* (Vol. 3), Amsterdam: Elsevier Science.

[53] Garbarino, James, and Kathleen Kostelny, 1996, "The Effects of Political Violence on Palestinian Childrens Behavior Problems: A Risk Accumulation Model", *Child Development* 67: 3345.

[54] Gilligan, Michael, Benjamin Pasquale and Cyrus Samii, 2010, "Civil War and Social Capital: Behavioral-Game Evidence from Nepal", mimeo, NYU and Columbia University.

[55] Giuliano, Paola, and Antonio Spilimbergo, 2014, "Growing Up in a Recession: Beliefs and the Macroeconomy", *Review of Economic Studies* 81: 787-817.

[56] Glaeser, Edward, and Bruce Sacerdote, 1999, "Why Is There More Crime in Cities", *Journal of Political Economy* 107: S225-S258.

[57] Glitz, Albrecht, 2012, "The Labor Market Impact of Immigration: A Quasi-Experiment Exploiting Immigrant Location Rules in Germany", *Journal of Labor Economics* 30: 175-213.

[58] Gould, Eric, Bruce A. Weinberg, and David B. Mustard, 2002, "Crime Rates and Local Labor Market Opportunities in the United States: 1979-1997", *Review of Economics and Statistics* 84: 45-61.

[59] Gould, Eric, Victor Lavy, and M. Daniele Paserman, 2011, "Sixty Years after the Magic Carpet Ride: The Long-Run Effect of the Early Childhood Environment on Social and Economic Outcomes", *Review of Economic Studies* 78: 938-973.

[60] Greene, William, 2004, "The Behavior of the Maximum Likelihood Estimator of Limited Dependent Variable Models in the Presence of Fixed Effects", *The Econometrics Journal* 7: 98-119.

[61] Grogger, Jeff and Michael Willis, 2000, "The Emergence of Crack Cocaine and the Rise in Urban Crime Rates", *Review of Economics and Statistics* 82: 519-529.

[62] Grosjean, Pauline, 2014, "A History of Violence: The Culture of Honor and Homicide in the US South", *Journal of the European Economic Association* 12: 1285-1316.

[63] Hainmueller, Jens, Dominik Hangartner, Duncan Lawrence, 2016, "When lives are put on hold: Lengthy asylum processes decrease employment among refugees", *Science Advances* 2: Issue 8.

[64] Heckman, James, Rodrigo Pinto, and Peter Savelyev, 2013, "Understanding the Mechanisms Through Which an Influential Early Childhood Program Boosted Adult Outcomes", *The American Economic Review* 103: 1-35.

[65] Hofmann, Agnes, and Kathrin Buchmann, 2008, "La Suisse Terre d'Asile: Informations sur le Droit d'Asile et sur les Personnes en Procedure d'Asile", Bern: Organisation Suisse d'Aide aux Refugies.

[66] Humphreys, Macartan, and Jeremy Weinstein, 2007, "Demobilization and Reintegration", *Journal of Conflict Resolution* 51: 531-67.

[67] Kelly, Morgan, 2000, "Inequality and Crime", *Review of Economics and Statistics* 82: 530-539.

[68] Kuterovac-Jagodic, Gordana, 2003, "Post-traumatic Stress Symptoms in Croatian Children Exposed to War: A Prospective Study", *Journal of Clinical Psychology* 59: 9-25.

[69] Leon, Gianmarco, 2012, "Civil Conflict and Human Capital Accumulation: Long Term Consequences of Political Violence in Perú", *Journal of Human Resources* 47: 991-1023.

[70] Levitt, Steven, 1997, "Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime", *American Economic Review* 87: 270-290.

[71] Machin, Stephen, and Costas Meghir, 2004, "Crime and economic incentives", *Journal of Human Resources* 39: 958-979.

[72] Miguel, Edward, Sebastian Saiegh, and Shanker Satyanath, 2011, "Civil War Exposure and Violence." *Economics and Politics* 23: 59-73.

[73] Moret, Joelle, Denise Efionayi, and Fabienne Stants, 2007, "Die srilankische Diaspora in der Schweiz", Bern: Federal Office for Migration.

[74] Öster, Anna, and Jonas Agell, 2007, "Crime and Unemployment in Turbulent Times", *Journal of the European Economic Association* 5: 752-775.

[75] Osgood, Wayne D., 2000, "Poisson-Based Regression Analysis of Aggregate Crime Rates", *Journal of Quantitative Criminology* 16: 21-43.

[76] Political Instability Task Force, 2013, "Genocides", dataset, available under `http://globalpolicy.gmu.edu/political-instability-task-force-home/`.

[77] Quinn, J. Michael, David Mason and Mehmet Gurses, 2007, "Sustaining the Peace: Determinants of Civil War Recurrence", *International Interactions* 33: 167-193.

[78] Rohner, Dominic, Mathias Thoenig, and Fabrizio Zilibotti, 2013, "War Signals: A Theory of Trade, Trust and Conflict",*Review of Economic Studies* 80: 1114-1147.

[79] Rohner, Dominic, Mathias Thoenig, and Fabrizio Zilibotti, 2013b,"Seeds of Distrust: Conflict in Uganda", *Journal of Economic Growth* 18: 217-252.

[80] Shemyakina, Olga, 2011, "The Effect of Armed Conflict on Accumulation of Schooling: Results from Tajikistan", *Journal of Development Economics* 95: 186-200.

[81] Swee, Eik Leong, 2008, "On War and Schooling Attainment: The Case of Bosnia and Herzegovina", mimeo, University of Toronto.

[82] Swiss Asylum Law (LAsi) since 26 june 1998, modified on 1 July 2013, available under `http://www.admin.ch/opc/fr/classified-compilation/19995092/201307010000/142.31.pdf`.

[83] UCDP/PRIO, 2013, "UCDP/PRIO Armed Conflict Dataset v.4-2013", dataset, available under `http://www.pcr.uu.se/research/ucdp/datasets/ucdp_prio_armed_conflict_dataset/`.

[84] United Nations Convention Relating to the Status of Refugees adopted in 1951, United Nations High Comission for Refugees, available under `http://www.unhcr.org/3b66c2aa10.pdf`.

[85] Voors, Maarten, Eleonora Nillesen, Philip Verwimp, Erwin Bulte, Robert Lensink and Daan van Soest, 2012, "Violent Conflict and Behavior: A Field Experiment in Burundi", *American Economic Review* 102: 941-64.

[86] Walter, Barbara, 2004, "Does Conflict Beget Conflict? Explaining Recurring Civil War", *Journal of Peace Research* 41: 371-88.

[87] Whitt, Sam, and Rick Wilson, 2007, "The Dictator Game, Fairness and Ethnicity in Postwar Bosnia", *American Journal of Political Science* 51: 655-68.

The Violent Legacy of Victimization Post-Conflict Evidence on Asylum Seekers, Crimes and Public Policy in Switzerland

– Online Appendix –

February 14, 2017

Mathieu Couttenier[1] Veronica Preotu[2]
Dominic Rohner[3] Mathias Thoenig[4]

[1]University of Geneva. E-mail: mathieu.couttenier@unige.ch.
[2]University of Geneva. E-mail: veronica.preotu@unige.ch.
[3]Department of Economics, University of Lausanne and CEPR. E-mail: dominic.rohner@unil.ch.
[4]Department of Economics, University of Lausanne and CEPR. E-mail: mathias.thoenig@unil.ch.

A Tables

Table 13: Exposure when 18-25 years old

	(1)	(2)	(3)	(4)
Dep. var.	Violent Crime Propensity		Property Crime Propensity	
Sample	Full	CC & MK	Full	CC & MK
Kid [1-12]	0.608*		0.025	
	(0.336)		(0.628)	
Exposure [18-25]	0.036		0.716	
	(0.433)		(0.451)	
Kid [1-12] (Only CC)		1.235**		0.938
		(0.524)		(0.688)
Kid [1-12] (Only MK)		1.282		0.771*
		(0.905)		(0.394)
Exposure [18-25] (Only CC)		-0.419		-0.013
		(0.605)		(0.579)
Exposure [18-25] (Only MK)		1.009**		1.230**
		(0.448)		(0.609)
Observations	4,820	1,786	4,820	1,786
R-squared	0.561	0.489	0.797	0.807

Note: OLS estimations weighted by the number of individuals in each cohort. Robust standard errors are clustered at nationality $\times$ year levels. *** p<0.01, ** p<0.05,* p<0.1. All estimations include gender fixed effects, age group fixed effects and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, 16, 17\}$ and $k \in \{26, 27..., 80+\}$. The group of reference is people born after the last years of violence. The dependent variable *Violent Crime Propensity* stands for the violent crime propensity of a cohort of nationality $(n)\times$ gender$(g)\times$ age bracket $(a)\times$ year (t).

Table 14: Alternative level of clustering

	(1)	(2)	(3)	(4)
Dependent Variable	Violent Crime Propensity			
Sample	Full	Full	CC	MK
Kid [1-12]	1.244	0.833**		
	(0.986)	(0.397)		
Kid [1-12] (Only CC)			0.809*	
			(0.413)	
Kid [1-12] (Only MK)				1.669**
				(0.721)
Gender FE	Yes	Yes	Yes	Yes
Age Group FE	Yes	Yes	Yes	Yes
Nationality × Year FE	No	Yes	Yes	Yes
Observations	4,820	4,746	3,991	1,778
R-squared	0.125	0.564	0.587	0.477
Sample mean	2.04	2.04	2.01	1.83

Note: OLS estimations weighted by the number of individuals in each cohort. Robust standard errors are clustered at nationality × age group levels. *** p<0.01, ** p<0.05,* p<0.1. All estimations include gender fixed effects, age group fixed effects and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$. The group of reference is people born after the last years of violence. The dependent variable *Violent Crime Propensity* stands for the violent crime propensity of a cohort of nationality $(n)\times$ gender $(g)\times$ age bracket $(a)\times$ year (t). Kid [1-12] is a binary measure of childhood exposure to civil conflict or mass killing (columns 1 and 2), to civil conflict only (column 3), to mass killing (column 4). For columns 3 to 5, the sample is restricted to cohorts from countries having experienced each specific type of violence.

Table 15: Alternative sample size

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Violent Crime Propensity				
Cohort size	>2	>5	>10	>15	>20
Kid [1-12]	0.809**	0.840**	0.858**	0.762*	0.769*
	(0.376)	(0.386)	(0.385)	(0.392)	(0.434)
Observations	2,753	1,946	1,426	1,168	985
R-squared	0.596	0.616	0.660	0.676	0.688

Note: OLS estimations weighted by the number of individuals in each cohort. Robust standard errors are clustered at nationality × year levels. *** p<0.01, ** p<0.05,* p<0.1. All estimations include gender fixed effects, age group fixed effects and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$. The group of reference is people born after the last years of violence. The dependent variable *Violent Crime Propensity* stands for the violent crime propensity of a cohort of nationality $(n)\times$ gender$(g)\times$ age bracket $(a)\times$ year (t). Kid [1-12] is a binary measure of childhood exposure to civil conflict or mass killing.

Table 16: Alternative Crime Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var.		Violent Crime Propensity						
Sample	Exclude CP=0 and CP=1	Tobit two censoring	Poisson	Excluding 3-sigma outliers	Excluding 2-sigma outliers	Probability Weight	Frequency Weight	Unweighted
Kid [1-12]	1.049*	1.911**	0.188***	0.547**	0.561***	0.833**	0.833**	0.646
	(0.554)	(0.792)	(0.072)	(0.217)	(0.201)	(0.363)	(0.360)	(0.416)
Observations	770	4,820	104,932	4,681	4,630	4,746	104,837	4,746
R-squared	0.666			0.717	0.745	0.564	0.564	0.201

Note: OLS estimations weighted by the number of individuals in each cohort. The Poisson estimation uses frequency weights. Robust standard errors are clustered at nationality $\times$ year levels. *** p<0.01, ** p<0.05,* p<0.1. All estimations include gender fixed effects, age group fixed effects, nationality $\times$ year fixed effects and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$. The group of reference is people born after the last years of violence. The dependent variable *Violent Crime Propensity* stands for the violent crime propensity of a cohort of nationality $(n)\times$ gender $(g)\times$ age bracket $(a)\times$ year (t). Kid [1-12] is a binary measure of childhood exposure to civil conflict or mass killing.

Table 17: Alternative Crime Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. Var.		(log-) odds ratio of Violent Crime Propensity					
Sample		cohort size > 1	cohort size > 2	Exclude CP=0 and CP=1	Probability Weight	Frequency Weight	Unweighted
Kid [1-12]	0.166*	0.161*	0.153*	0.118	0.166*	0.166*	0.165*
	(0.086)	(0.089)	(0.091)	(0.092)	(0.086)	(0.085)	(0.084)
Observations	4,746	3,349	2,753	770	4,746	104,837	4,746
R-squared	0.667	0.673	0.676	0.805	0.667	0.667	0.318

Note: OLS estimations weighted by the number of individuals in each cohort. Robust standard errors are clustered at nationality $\times$ year levels. *** p<0.01, ** p<0.05,* p<0.1. All estimations include gender fixed effects, age group fixed effects, nationality $\times$ year fixed effects and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$. The group of reference is people born after the last years of violence. The dependent variable *Violent Crime Propensity* stands for the violent crime propensity of a cohort of nationality $(n)\times$ gender $(g)\times$ age bracket $(a)\times$ year (t). Kid [1-12] is a binary measure of childhood exposure to civil conflict or mass killing.

Table 18: Crime Propensities and Conflict Exposure: Cohort × Canton Sample

	(1)	(2)	(3)	(4)
Dependent Variable	Violent Crime Propensity			
Sample	Full	Full	CC	MK
Kid [1-12]	0.928*	0.419*		
	(0.471)	(0.246)		
Kid [1-12] (Only CC)			0.432*	
			(0.259)	
Kid [1-12] (Only MK)				0.595
				(0.361)
Observations	28,462	28,400	26,469	14,870
R-squared	0.073	0.213	0.223	0.166

Note: OLS estimations weighted by the number of individuals in each cohort. Robust standard errors are two-way clustered at nationality × year and canton × year levels. *** p<0.01, ** p<0.05,* p<0.1. All estimations include gender fixed effects, age group fixed effects, canton × year fixed effects and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$. From column 2 onwards we further include nationality × year fixed effects. The group of reference is people born after the last years of violence. The dependent variable *Violent Crime Propensity* stands for the violent crime propensity of a cohort of nationality $(n)\times$ gender $(g)\times$ age bracket $(a)\times$ year (t) in a canton (c). Kid [1-12] is a binary measure of childhood exposure to civil conflict or mass killing.

Table 19: The Effect of Conflict Experience on Women's Crime Propensity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. Var.	Violent Crime Propensity						
Exposure to	CC & MK	CC	MK	Wartime rape	CC	MK	Wartime rape
Women$[1,+]$	0.307	0.415	0.487	0.488**			
	(0.654)	(0.674)	(0.425)	(0.233)			
Women$[1,+]$: low intensity					0.345	-0.312	0.755***
					(0.285)	(0.332)	(0.232)
Women$[1,+]$: medium intensity					-0.071	0.489	0.744*
					(0.295)	(0.416)	(0.442)
Women$[1,+]$: high intensity					0.629*	0.739	1.739**
					(0.325)	(0.446)	(0.701)
Observations	2,008	1,717	843	1,717	1,717	843	1,717
R-squared	0.290	0.286	0.294	0.256	0.292	0.300	0.257

Note: OLS estimations weighted by the number of individuals in each cohort. Robust standard errors are clustered at nationality × year levels. *** p<0.01, ** p<0.05,* p<0.1. All estimations include gender fixed effects, age group fixed effects, nationality × year fixed effects. The dependent variable *Violent Crime Propensity* stands for the violent crime propensity of a cohort of nationality $(n)\times$ gender $(g)\times$ age bracket $(a)\times$ year (t). The explanatory variable, Women$[1,+]$ is a dummy equal to one if at least one individual in the observation cell was exposed to conflict (civil conflict, mass killing episode or wartime rape episode). The sample is restricted only to women and the group of reference is people born after the last year of conflict (civil conflict, mass killing episode or wartime rape episode).

Table 20: Bilateral Crime Regressions: Dyadic fixed effects

	(1)	(2)	(3)	(4)
Dep. var.	Violent Crime Propensity	Violent Crime Propensity - No Family		
Exposure to	CC, MK	CC, MK	CC	MK
Kid [1-12]$_{n,a,t} \times \mathbb{I}_{n=v}$	1.646***	0.926***	0.948***	1.900**
	(0.399)	(0.307)	(0.304)	(0.739)
Nationality of perpetrator × Nationality of victim FE	Yes	Yes	Yes	Yes
Nationality of perpetrator × Year × Age FE	Yes	Yes	Yes	Yes
Nationality of victim × Year FE	Yes	Yes	Yes	Yes
Gender FE	Yes	Yes	Yes	Yes
Observations	812,831	812,831	686,382	308,808
R-squared	0.085	0.062	0.058	0.051

Note: OLS estimations. Robust standard errors are clustered at the nationality × year level. *** p<0.01, ** p<0.05,* p<0.1. We include nationality of perpetrator × nationality of victim fixed effects, nationality of victim × year fixed effects, nationality of perpetrator × age × year fixed effects, gender fixed effects and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$ interacted with $\mathbb{I}_{n=v}$. The group of reference is people born after the last years of violence. The dependent variable *Violent Crime Propensity* corresponds to bilateral propensity to violent crime. $\mathbb{I}_{n=v}$ is a binary indicator function equal to 1 if perpetrator and victim are co-nationals. Kid [1-12] is a binary measure of childhood exposure to civil conflict or mass killing.

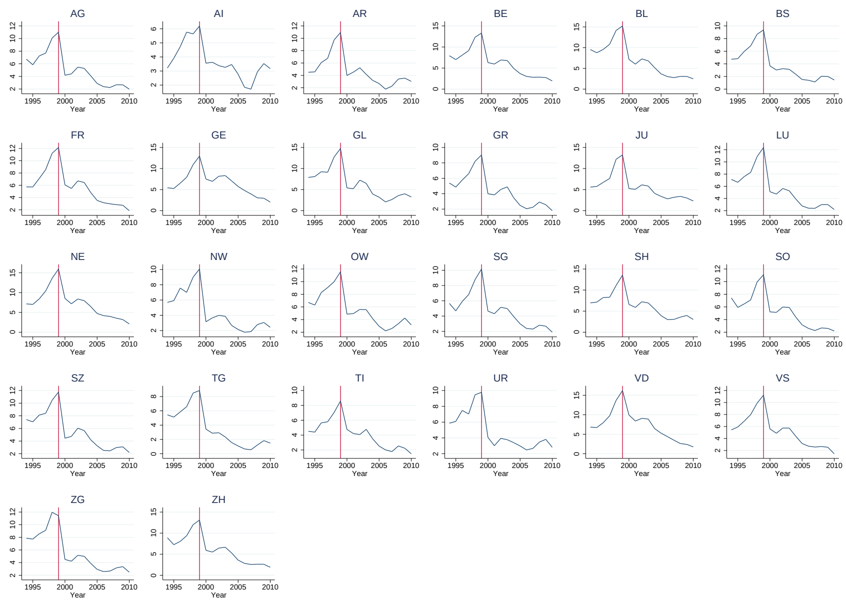
Table 21: Integration Policies - Summary Statistics

Variable	Mean	Std.Dev.	Max	Min
Open job access$_c$	.6538461	.4851645	1	0
Promote job market$_c$	.1363636	.3512501	1	0
Professional training$_c$	.3636364	.492366	1	0
Occupation rate$_{nct}$	.1022142	.063855	.3513073	0
Occupation rate: Index$_{nct}$	.1128592	.067392	.406135	0
Civic & language courses$_c$	.4090909	.5032363	1	0
Private management$_c$	.2692308	.4523443	1	0
Social assistance$_{ct}$	6.929337	0.1319578	7.324757	6.607203
Acceptance rate$_n$	.0767988	.105302	.537329	0
Readmission$_n$	.2313311	.4232513	1	0

Note: Sample of 26 cantons for the time-invariant variables varying at the canton level (c). Sample of 26 cantons over 4 years (2009-2012) for the variables varying by canton and over time (c,t). Sample of 134 countries for the variables varying at the nationality level (n).

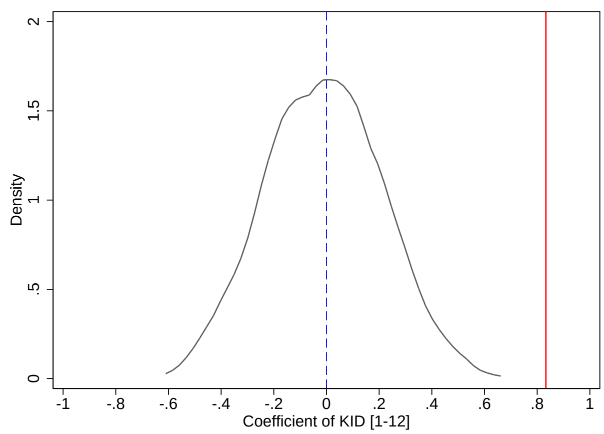
B Figures

Figure 2: Evolution of Adult Asylum Seeker Stocks by Cantons



Note: The graph plots the share of adult asylum seekers in total population (per thousands) by cantons over the period 1994-2010.

Figure 3: Monte Carlo Simulations



Note: Figure 3 displays the re-estimates of column 2 of Table 3 with a random draw of our variable KID$[1-12]$ (1000 draws).

Figure 4: Marginal Effect: Occupation Rate

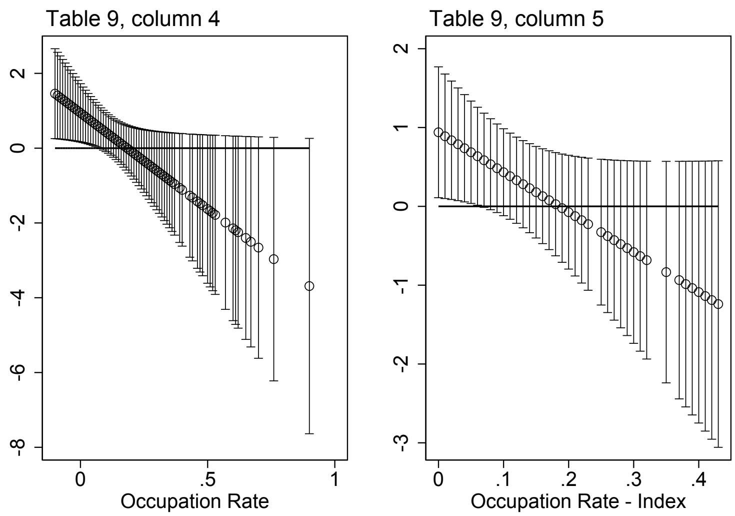
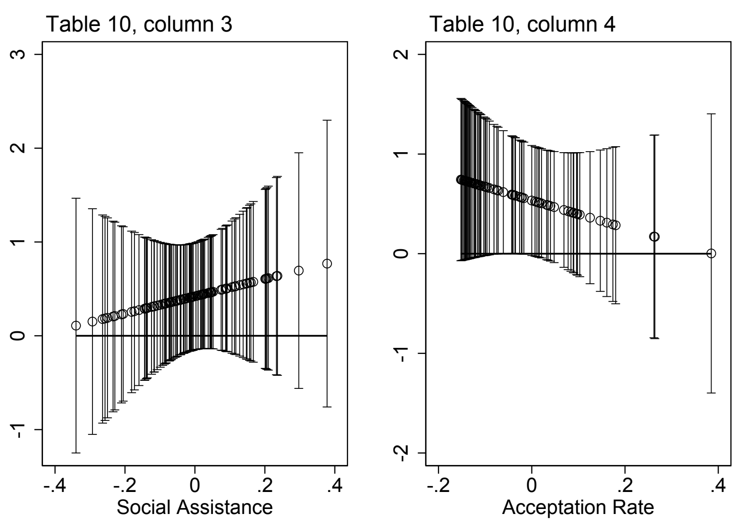


Figure 5: Marginal Effect: Financial and Social Integration



C Appendix on Political Asylum in Switzerland

C.1 The Demand for Asylum in Switzerland

Switzerland is a federal state with 26 cantons (i.e. the main sub-national entities) and a population of about 8 million people. This country has a strong humanitarian tradition (starting in the 16th century with providing large-scale refuge to the Huguenots fleeing France) and has traditionally hosted many foreigners. While according to the Swiss Federal Statistical Office in 2012 about 23.3% of the population are foreign nationals, the number of asylum seekers –which are defined as individuals who have applied and are waiting for being approved the refugee status– is considerably smaller. In particular, in our data in 2012 there were slightly above 32'000 asylum seekers (with the N permit), corresponding to about 0.4% of the Swiss population. The biggest cohorts for our sample period came from Eritrea, Sri Lanka, Nigeria, Afghanistan, Somalia, Tunisia, Serbia, Turkey, Iraq and Syria (see Table 1).

According to the United Nations Convention Relating to the Status of Refugees adopted in 1951 and signed by Switzerland, a refugee is a person who "owing to well-founded fear of being persecuted for reasons of race, religion, nationality, membership of a particular social group or political opinion, is outside the country of his nationality and is unable or, owing to such fear, is unwilling to avail himself of the protection of that country; or who, not having a nationality and being outside the country of his former habitual residence as a result of such events, is unable or, owing to such fear, is unwilling to return to it" (Article 1, A2). Thus, while of course many migrants are motivated by the (legitimate) goal of escaping extreme poverty, to obtain refugee status a person needs to be able to demonstrate persecution for political reasons.

C.2 The Procedure of Assessing Asylum Requests

Most asylum seekers enter Switzerland illegally (especially crossing the Italian border) and apply for asylum in one of the four national reception and procedure centers (RPC) [5]. Only few arrive by plane in one of the two airport centers (Geneva and Zurich) or through asylum applications filed at Swiss embassies abroad.[6]

In the RPC, asylum seekers go through a first interview, where they are asked to provide identity proofs, fingerprints, and their application reasons. If there persist doubts about the identity and the application reasons, language tests or lie detection techniques are used. After on average 100-120 days since the demand was made in the RPC [7], authorities declare "non-credible" ("non-entrée en matière" - NEM) around 50% of the 20000-25000 yearly treated demands, according to the Federal Office of Migrations Statistics (2009-2012). Typical examples of demands judged "non-credible" are from nationals who either originate from safe countries, or who do not collaborate with the

[5] There are four reception centers close to the Swiss borders: Basel, Chiasso, Kreuzlingen, and Vallorbe.

[6] On 28 September 2012, the Swiss Parliament abolished the possibility of applying for asylum from abroad. However, the possibility to file a visa request with a Swiss diplomatic representation is still open.

[7] The asylum process duration measures follow the estimations for 2008-2010 published in *Rapport sur des mesures d'accélération dans le domaine d'asile*, Département fédéral de justice et police (2011).

authorities, who apply for asylum a second time after having already been rejected earlier or whose demand has to be treated by another state according to the Dublin Agreement [8]. When a demand is judged "non-credible" and, thus, rejected, the asylum seeker either voluntary leaves the country or is detained and expelled by force (when possible).

The other half of asylum seekers whose demands are judged credible receive the N permit [9] (i.e. a temporary "green card" for being allowed to stay in Switzerland during the duration of the in-depth assessment of their asylum request). Given that assessing the threat of persecution in the home country is hard, the asylum process naturally takes substantial time. Between 2008-2010, the average duration of the process for credible asylum seekers (from the moment the demand was made in one of the RPC until the first decision), was 300-400 days, with complex cases taking several years.

Crucially, during this period holders of the N-permit are exogenously allocated to cantons and are not allowed to change canton. The allocation of new N-permit holders to the 26 Swiss cantons are determined precisely by a random allocation key based on the cantonal population. This exogenous and random allocation of N-permit holders is crucial for the identification strategy of our current paper, as it rules out self-selection of particular types of asylum seekers to particular cantonal environments. Once an asylum seeker has been allocated to a given canton, the canton in charge organizes the accommodation in cantonal centers or flats and takes care of the interviews and of financial matters.[10].

The accommodation procedure for asylum seekers takes place in two steps within the canton. (Efionayi-Mäder, 2011). In a first step, they are hosted in collective centers where their basic needs are taken care of. In a second step, they are offered either private apartments or collective accommodation in special centers. In about half of the cantons, families with children are offered private accommodation. The diversity of accommodation possibilities across cantons is explained by the housing availabilities, size of the canton, responsible organizations etc. We acknowledge that while the exogenous allocation holds across cantons it may not hold inside cantons due to housing availabilities. In other words, collective accommodation centers may sometimes be placed in remote neighborhoods.

[8]The Dublin Association Agreement of 2008 between Switzerland and the EU ensures that a request for asylum submitted by an asylum-seeker is only examined by one state within the Schengen/Dublin Area (which includes most EU member states in continental Europe, plus Switzerland, Norway and Iceland). The Dublin criteria establish which country is responsible for dealing with a given asylum application, and aims to prevent asylum seekers from being referred from one country to another. If the asylum demand was rejected by this responsible state, then the asylum seeker cannot apply for asylum in another member state.

[9]In the individual crimes database, asylum seekers are reported with N-permit and non-credible asylum seekers are reported with NE. We only consider those with N-permit, thus the more credible ones. It might still be possible that non-credible asylum seekers are allocated to cantons if the decision takes more than 90 days.

[10]Cantons decide on the funds offered for social assistance and the Confederation supplies the funds needed. According to Efionayi-Mäder (2011), the Confederation allocated, on average, 55,64 CHF per day per asylum seeker.

C.3 The Chances of Being Granted Refugee Status

At the end of the assessment period and during which the asylum seeker has permit-N status, 15% of the treated demands receive a positive answer which consists in being awarded permanent refugee status (B-permit). This allows refugees to stay in Switzerland in the long-run and have the same rights as a usual registered immigrant (like e.g. migrants from EU countries), including the freedom to change the residence canton, social assistance and full working rights. The acceptance rates vary with the influx of asylum seekers which depends on circumstances in the countries of origin.

In addition, around 20% of treated demands are awarded temporary protection (F-permit). Provisionally admitted foreign nationals are persons who have been ordered to return from Switzerland to their native countries but in whose cases the enforcement of this order has proved inadmissible (violation of international law), unreasonable (concrete endangerment of the foreign national) or impossible (for technical reasons of enforcement, such as missing readmission treaties). F-permit holders cannot change canton and benefit from the same social assistance as N-permit holders (for up to 7 years), even if some cantons offer them more generous social assistance.[11]

The remaining 15% of treated demands are rejected and can be appealed at the Federal Administrative Court which makes the final decision. In this case the process might take on average another 560 days during which the asylum seeker sees his N-permit prolonged. If the final decision is negative, some proportion leaves voluntarily, while there are also a small number of forced expulsions, and a part of rejected asylum seekers who go into hiding and become illegal immigrants. The timeline and outcomes of the asylum process are summarized in Figure 6 below.

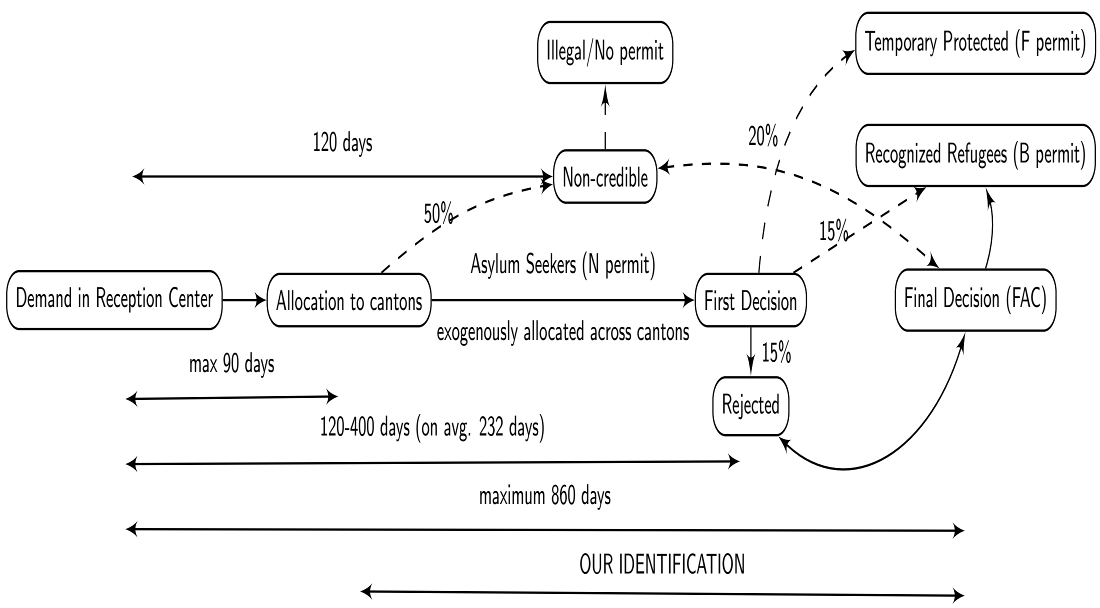
There are various factors that affect the chances of an asylum seeker to obtain a B-permit. First of all, the acceptance rates vary widely depending on the country of origin. While residents from home countries with ongoing civil wars and large-scale political persecution have very high chances of their demand being successful, people from countries that are poor but without systematic human rights abuses have much smaller chances. For example, residents from Eritrea have a 41.3% chance of being offered the B-permit, whereas residents from Algeria have only a chance of 3.6%.

While the chances of obtaining a B-permit mostly depend on the current political situation in the home country, the chances for an F-permit also depend on whether Switzerland has been able to conclude a treaty of readmission with the home country of an asylum seeker. If such a treaty does not exist, it is nearly impossible for Swiss authorities to force an individual to leave the country.

Last, but not least, there are also incentives to behave in a low-abiding way, as criminal behavior can trigger rejection of the asylum request. In particular, a new constitution article introduced in 2010 (Art. 121, al. 3-6, available at `https://www.admin.ch/ch/e/rs/1/101.en.pdf`) stipulates that asylum seekers who are convicted of serious crimes like for example murder, rape, robbery, drug dealing, fraudulent abuse of social aid and assistance see their asylum demands automatically rejected and are expelled.

[11] Since 2008 the Confederation has decided that integration programs for temporary protected persons are compulsory. Moreover, since 2006, F permit holders have the same working rights as the other Swiss residents.

Figure 6: Timeline and Outcomes of the Swiss Asylum Process



Notes: The timeline follows the Swiss Asylum Law (LAsi) firstly adopted on 26 june 1998 and lastly modified on 1 July 2013. The estimations for the different asylum process duration measures are for 2008-2010 and follow the Report on accelerating measures in the asylum process, Federal Department for Justice and Police (2011). The refusal and acceptance rates are taken from the Swiss Federal Office of Migrations Statistics Reports for 2009-2012. Overall, the information is sustained by discussions with professionals in the asylum process.

Successful integration efforts can also be rewarded through another channel. In special cases (such as an asylum procedure lasting longer than 5 years or for several personal hardship reasons), cantonal authorities can grant a residence (B) permit to asylum seekers under the condition of good integration into the Swiss society[12]. The most important integration criteria refers to the respect of laws: no police record, and no acts of default of goods and prosecution. The second important criteria is the proof of financial independence (no need for social assistance). First, such a special hardship demand needs to be accepted by the canton and then it has to be approved at the federal level. While the cantonal decision cannot usually be repealed, the federal decision can be repealed.

[12] According to the Ordinance for the Integration of Foreigners (2008), integration criteria are defined as follows: respect of federal values, public order and security, willingness to have an economic activity and acquire training, knowledge of at least one Swiss language and Swiss living style. Duration of stay, family and health conditions, and the possibility of reintegration into the country of origin are also taken into account.

D Appendix on formal tests of the process of allocation

We now provide more formal statistical tests for the exogenous spatial allocation of asylum seekers across cantons. The purpose of this is to tackle the question of whether there is indeed an exogenous allocation of asylum seekers following the official population-based distribution key –as we claim– or if there may be some selection on relevant dimensions. For example, such selective allocation would occur if, say, the urban (supposedly more crime-prone) cantons of Zurich or Geneva were more likely to host young males fleeing a conflict zone while rural and quiet cantons were to host other asylum seekers.

In Table 22 we display a first approach where, for various observable cohort characteristics, we test for the difference in means between cantons. To this purpose we consider a more fine-grained version of our sample where, in year t, each cohort of nationality n, gender g, and age group a is splitted across cantons c. In Column (1) we regress, for each nationality taken separately, each observable on a battery of canton fixed effects. In case of endogenous spatial sorting in some specific cantons, the fixed effects associated with those cantons should be statistically significantly correlated with the cohort characteristics, and the F-test should be rejected for this nationality. The reported figure corresponds to the share of nationalities for which the F-test is not rejected at the 5 percent level. The first observable that we consider corresponds to our main explanatory variable, e.g. the exposure to violence during childhood. We see that in 82% of cases our test does not detect any endogenous spatial sorting based on this observable. Results are similar for the alternative measures of exposure or for other first order determinants of crime, namely gender and age (by brackets).

Our above approach to testing exogenous spatial allocation has the advantage of simplicity but might not be ideally suited to our empirical context where the spatial units under consideration are small. Indeed, the median nationality is composed of only 42 asylum seekers –sample average being 765– that have to be allocated across 26 cantons. In this type of context, as firstly pointed out by Ellison and Glaeser (1997), a parametric test of spatial allocation/concentration (that assumes independent location choices) might be ill-defined to test the null hypothesis of exogenous allocation. Indeed, in presence of an exogenous allocation process that does not take into account the past exposure to violence of asylum seekers, we should find a uniform distribution of past exposure across the cantons only asymptotically, i.e. *only if the size of each nationality is large enough.* Otherwise, for small nationality sizes, sampling variations will lead to observed patterns of spatial concentration in some cantons.

Column (2) of Table 22 is a first attempt to tackle this sampling issue. The idea is to pool cohorts from all nationalities by year before performing the F-test. The pool of asylum seekers to be allocated is now much larger (on average 24,864 individuals per year) and this makes our F-test more likely to meet the asymptotic requirement. For each observable, the reported statistic corresponds to the number of years (out of 4) for which the F-test is not rejected. As expected, the F-test performs now better.

In Column (3) we propose an alternative approach to circumvent the sampling issue in the test of exogenous allocation. We perform a Monte Carlo simulation (1000 draws) generating artificial random allocations that we later compare to the observed allocation. For each nationality, we pool the population of asylum seekers and reallocate it randomly, without replacement, across the different cantons, maintaining unchanged the actual size of each canton. We get a simulated random distribution of individuals with a given observable characteristic across cantons. The F-tests of Column (1) are then replicated on this generated sample for each observable. We report the mean and standard deviation across 1000 Monte Carlo draws of the share of nationalities for which the F-test is not rejected at the 5 percent level. The results obtained with the simulated data based on random allocation are comparable to the results obtained with the observed data.

Overall, the tests of Table 22 are supportive of our identifying assumption that the allocation of asylum seekers across cantons can be considered as exogenous with respect to their age, gender and past exposure to violence. As discussed above, a concern is that the size of cohorts is small such that the spatial allocation of asylum seekers from a given nationality may be subject to sampling variations: By chance, in spite of the exogenous allocation, cohorts born after could be located in cantons with different characteristics from cohorts born before. In an important robustness check (Section 4.4) we show that our results are not driven by sampling errors. To this purpose we estimate the baseline crime regression (equation 1) on the fine-grained sample of cohorts split across cantons that allows the inclusion of (canton × year) fixed effects in order to absorb unobserved sampling errors.

Table 22: F-Tests of Exogenous Allocation

Characteristics	Share of nationalities H_0 is not rejected (1)	Nb of years (out of 4) H_0 is not rejected (2)	Share of nationalities H_0 is not rejected (3)
Kid [1-12]	0.82	4	0.81 (0.05)
Kid [1-12] (only CC)	0.82	4	0.81 (0.05)
Kid [1-12] (only MK)	0.87	4	0.68 (0.09)
Male	0.75	3	0.82 (0.04)
Age [16-17]	0.97	4	0.72 (0.04)
Age [18-20]	0.91	4	0.69 (0.04)
Age [21-24]	0.92	4	0.72 (0.04)
Age [25-29]	0.83	4	0.74 (0.04)
Age [30-34]	0.81	4	0.72 (0.04)
Age [35-39]	0.88	4	0.69 (0.04)
Sample	Obs. data	Obs. data	Monte Carlo

Note: The table displays the results of F-tests for different observable characteristics of asylum seeker cohorts. Each test consists of regressing $characteristic_{n,g,a,t,c} = \sum_{c=1}^{c=26} \beta_c FE_c + \varepsilon_{n,g,a,t,c}$ under H_0: $\beta_c = 0$ for all cantons c. Column (1) reports the share of nationalities for which H_0 cannot be rejected at 5% from nationality-specific F-tests. Column (2) reports the number of years (out of 4) for which H_0 cannot be rejected at 5% when all nationalities are pooled by year. Column (3) replicates Column (1) on simulated data where individuals are randomly allocated across cantons. The numbers reported correspond to the mean and standard deviation across 1000 Monte Carlo draws of the share of nationalities for which H_0 cannot be rejected at 5%.

E Appendix on the Identification Strategy

In this Appendix we discuss in more details four elements of our identification strategy.

Spatial sorting in Switzerland – A first challenge relates to the fact that crime-prone individuals tend to self-select into a crime-facilitating environment. For example, individuals exposed to conflict in their origin country are used to live in areas with high economic deprivation and violence; by contrast, individuals from peaceful background, once in Switzerland, could strategically avoid criminal hotspots or poorest neighborhoods with few labor market opportunities. This example illustrates a case where past exposure to conflict correlates with an unobserved cohort characteristic (i.e. preferences in terms of living area) that impacts crime-proneness in Switzerland.

Our empirical strategy is able to rule out this spatial sorting issue by restricting our core estimates to asylum seekers, a subsample of migrants who are exogenously allocated across Switzerland (see Section 4.2). Notice that this exogenous allocation has a second virtue related to the fact that cantons are very heterogeneous in term of pro-asylum policies which may affect the elasticity of violence propensity to past conflict exposure. The exogenous allocation makes sure that exposed individuals cannot select location according to cantonal policies.

Pre-conflict characteristics of origin countries – Our empirical analysis intends to capture the *consequences* of past conflict exposure on crime propensity. We consequently include nationality fixed effects (captured by $\mathbf{FE}_{n,t}$), in order to filter out slow-moving characteristics of the origin country that could correlate with frequent war outbreaks and crime-promoting characteristics (weak institutions, low social capital and dismal inter-ethnic trust, etc.).

Selection into migration – The push and pull factors determining migration decisions are likely to be affected by conflicts. Presumably, peacetime is associated to economic migration while humanitarian migrants are over-represented in post-conflict periods. In turn, this could affect post-migration crime incentives in the destination country. The inclusion of gender and age bracket fixed effects, $\mathbf{FE}_g$ and $\mathbf{FE}_a$, aims to control for major socio-demographic co-determinants of violent behaviors and the decision to emigrate. Further, at least as important is the inclusion of the nationality×years fixed effects ($\mathbf{FE}_{n,t}$) which absorb time-series variations in origin-specific push factors.[13] Note that we have no information on the educational level of asylum seekers. Therefore the estimated excess criminality of exposed cohorts could be partly linked to unobserved heterogeneity in human capital. We believe however that economic deprivation and educational disruption are important drivers of the causal

[13] It would be even better to control for nationality×years of emigration fixed effects. Unfortunately, our data does not contain this information. However, given that the Swiss asylum procedures are on average relatively short (i.e. the average processing time of the procedure of asylum request is around 300-400 days), the current year t is on average close to the year of emigration, and hence nationality×years fixed effects are a good proxy for nationality×years of emigration fixed effects.

impact of past exposure to conflict on violent criminality. Note that in Section 5 we control for education and human capital thanks to the inclusion of cohort-specific fixed effects (in bilateral crime regressions).

Perpetrators and victims – Related to the previous point, it could be that after a conflict perpetrators are over-represented among migration waves. Hence, high crime proneness in Switzerland may not only be due to participation to the war, but to prewar individual disposition. To alleviate this concern we exclude the potential perpetrators by focusing on the sub-sample of victims exclusively, i.e. i/ individuals who were children during the war compared to those born afterwards; ii/women born before the war compared to those born afterwards.[14]

[14]Of course, individuals may also become involved in a further war later in life, but this is picked up in the regressions by the fact that we include as controls a set of binary variables coding for past exposure at the later ages.

F Additional Data Definitions and Sources

In Section 6 of the paper, we use several measures of integration policies and observables at the canton-level. We describe hereafter how we build these variables:

Open job access – The Swiss federal law stipulates that every asylum seeker in Switzerland is banned from paid work for the first three months in Switzerland (article 43 of the asylum law, see also `http://www.jugendweb.asyl.admin.ch/php/get_pdf.php?id=165`). This ban can under some conditions be renewed by the canton for a further three months. After this, asylum seekers are in principle allowed to engage in paid work, subject to holding a valid work permit, which is again subject to a canton's decision. In sum, after the first three months of general work ban, the Swiss cantons have the power to authorize or further restrict asylum seeker requests for accepting a paid employment. Our *Open Job Access* is a dummy of whether in a given canton an asylum seeker is allowed to immediately and freely search for paid employment (conditional on respecting any federal conditions), or whether any cantonal forms of constraints and bans from paid work apply in 2012. *Source:* Konferenz der Sozialdirektoren, `http://sodk.ch/fileadmin/user_upload/Fachbereiche/Migration/2012.08.27_Schreiben_SODK_an_SPK-S_Asylgesetzrevision_Webversion_d.pdf` and personal communication with canton officials.

Promote job market – The variable PROMOTE JOB MARKET$_c$ takes a value of 1 in all cantons that offer active promotion services for labor market access, and 0 otherwise. *Source:* Survey on "Migration and Federalism", collected by the "Forum suisse pour l'etude des migrations et de la population", question 17, subquestion on "Promotion de l'integration dans le 1er ou 2e marché du travail".

Professional training – The variable PROFESSIONAL TRAINING$_c$ takes a value of 1 in all cantons that offer at least one type of professional training, such as coaching, training, internships, and 0 otherwise. *Source:* Survey on "Migration and Federalism", collected by the "Forum suisse pour l'etude des migrations et de la population", question 18.

Occupation rate – The occupation rate as defined in Table 8, Column 4 corresponds to the occupation rate at the level of the nationality-canton in the previous year, while in Column 5 of the same table the definition of the occupation rate corresponds to the average occupation rate of asylum seekers from different countries in a given canton in the previous year. *Source:* OFS.

Civic and language courses – The variable CIVIC AND LANGUAGE COURSES$_c$ takes a value of 1 if both civic education and language courses are offered, and 0 otherwise. *Source:* Survey on "Migration and Federalism", collected by the "Forum suisse pour l'etude des migrations et de la population", question 17 (are counted as language courses both "Cours de langue pour usage quotidien" and "Cours de langues de niveau approfondi", and are counted as civic education courses both "Cours de culture generale" and "Education civique et cours similaires").

Private management – The variable PRIVATE MANAGEMENT$_c$ is a dummy taking a value of 1 when in a given canton at least one asylum center on the municipal or cantonal level is run by a private firm, and taking a value of 0 otherwise. The variable NO PRIVATE$_c$ takes a value of 1 (0) if PRIVATE$_c$ is 0 (1). *Source:* www.abs-ag.ch, www.ors.ch, private communication with canton officials.

Social Assistance – The variable ASSISTANCE$_{c,t}$ originates from FSO Asylum Social Aid Statistics (eAsyl) and corresponds to estimates of the log of the total money attributed per asylum seeker per month in a given canton and year. *Source:* FSO-Asylum Social Aid Statistics (eAsyl).

Acceptance Rate – The variable ACCEPTANCE$_{n,t}$ stands for the percentage of recognized refugees (residence B permit) among asylum seeker demands, by nationality and year. *Source:* Our own calculations based on the raw data on all migrants received from the Federal Office of Migrations (FOM).

Readmission Agreement – The binary variable READMISSION$_n$ takes a value of 1 if there exists a bilateral readmission agreement between Switzerland and the country of origin n. *Source:* Coded using the official list of readmission treaties from the FOM - `www.bfm.admin.ch/content/bfm/fr/home/themen/internationales/internationale_vertraege/ref_rueckuebernahme.html`.