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

– Online Appendix –

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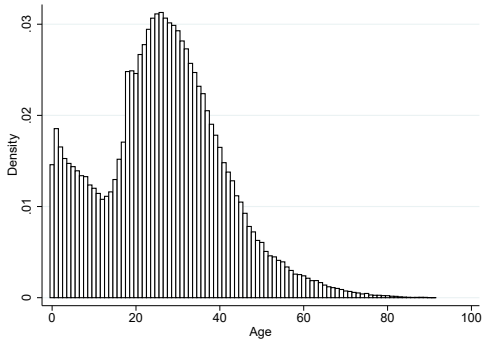
A1 Additional Summary Statistics

Below we display additional summary statistics on our sample of asylum seekers. Figure A1.1 shows the distribution of ages for all asylum seeker cohorts, while Figure A1.2 displays for the fifteen most represented countries their share in the total of asylum seekers from 2009 to 2016.

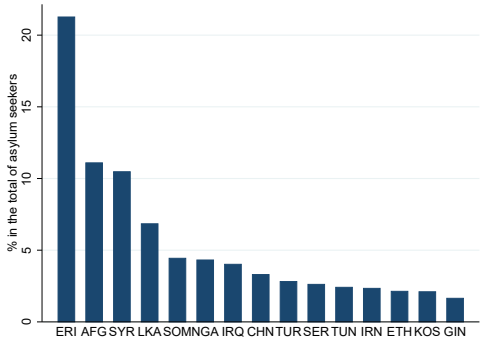
Next, we list the average of yearly number of individuals, history of civil conflicts and mass killings, years since last civil conflict and mass killings, as well as the childhood exposure to these events for each nationality. These summary statistics are based on the sample of adult male cohorts used for column (1) in Panel A of Table 5.1. We mark with X the average of yearly number of individuals from a given nationality if it is lower than 10 for confidentiality reasons. Civil conflicts are defined as episodes leading to at least 25 battle-related fatalities at the country × year level, and come from UCDP (2017). Mass killing episodes are defined 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". The data on mass killings at the country × year level comes from the Political Instability Task Force (2016).

Figure A1.1: Distribution of Asylum Seekers by Age



Notes: Based on all cohorts of asylum seekers in Switzerland during 2009-2016.

Figure A1.2: Most Represented Countries in the Total of Asylum Seekers



Notes: Based on all cohorts of asylum seekers in Switzerland during 2009-2016.

Table A1.1: Summary Statistics by Nationality
- numbers replaced by X for confidentiality compliance -

Nationality	Yearly average nb. of individuals	History of CC or MK	Nb. of years since last CC	Nb. of years since last MK	Kid[1-12] (CC or MK)
Afghanistan	2084	1	0	24	0.58
Albania	27	0	.	.	0
Algeria	394	1	0	54	0.56
Angola	31	1	7	14	0.82
Armenia	61	0	.	.	0
Australia	X	0	.	.	0
Azerbaijan	16	1	0		0.40
Bahrain	X	0	.	.	0
Bangladesh	59	1	0	.	0.87
Belarus	62	0	.	.	0
Benin	18	0	.	.	0
Bhutan	X	0	.	.	0
Bolivia	X	1	49	.	0.50
Bosnia and Herzegovina	70	1	21	21	0.38
Botswana	X	0	.	.	0
Brasil	X	0	.	.	0
Bulgaria	X	0	.	.	0
Burkina Faso	19	1	29	.	0.43
Burundi	X	1	1	23	0.85
Cameroon	49	1	0	37	0.59
Cape Verde	X	0	.	.	0
Central African Republic	X	1	3	0	0.44
Chad	32	1	1	.	0.99
Chile	X	1	43	40	0
China	623	1	8	41	0.48
Columbia	13	1	0	.	0.95
Congo	X	1	0	.	0.25
Croatia	X	1	21	.	0.43
Cuba	X	1	55	.	0.08
Czech Republic	X	0	.	.	0
Democratic Republic of Congo	105	1	2	37	0.83
Djibouti	X	1	17	.	0.65
Ecuador	X	0	.	.	0
Equatorial Guinea	X	1	.	37	0.08
Egypt	83	1	0	.	0.41
Eritrea	3577	1	13	.	0.20
Ethiopia	302	1	0	37	0.96
Gabon	X	1	52	.	0
Gambia	347	1	35	.	0.37

Table A1.1: Summary Statistics by Nationality
- numbers replaced by X for confidentiality compliance -

Nationality	Yearly average nb. of individuals	History of CC or MK	Nb. of years since last CC	Nb. of years since last MK	Kid[1-12] (CC or MK)
Georgia	338	1	8	.	0.42
Ghana	120	1	33	.	0.48
Guatemala	X	1	21	26	1.00
Guinea	464	1	15	.	0.25
Haiti	X	1	12	.	0.75
Hungary	X	0	.	.	0
India	14	1	0	.	1.00
Iran	441	1	5	24	0.92
Iraq	779	1	0	0	0.95
Israel	X	1	2	.	1.00
Ivory Coast	125	1	5	.	0.19
Jamaica	X	0	.	.	0
Jordan	X	1	0	.	0
Kazakhstan	X	0	.	.	0
Kenya	12	1	0	.	0.39
Republic of Korea	X	0	.	.	0
Dem. People's Rep. of Korea	X	0	.	.	0
Kosovo	242	1	17	17	0.41
Kuwait	X	0	.	.	0
Kyrgyzstan	X	0	.	.	0
Lebanon	23	1	1	.	0.71
Liberia	42	1	13	.	0.95
Libya	101	1	0	.	0
Macedonia	100	1	15	.	0.16
Malawi	X	0	.	.	0
Mali	120	1	0	.	0.68
Mauritania	37	1	5	.	0.19
Mexico	X	1	20	.	0.69
Moldova	13	1	24	.	0.51
Mongolia	46	0	.	.	0
Morocco	395	1	27	.	0.76
Myanmar	X	1	0	38	1.00
Nepal	38	1	10	.	0.36
Niger	44	1	0	.	0.74
Nigeria	1261	1	0	46	0.36
Pakistan	154	1	0	39	0.87
Peru	X	1	6	.	1.00
Poland	X	0	.	.	0
Portugal	X	0	.	.	0

Table A1.1: Summary Statistics by Nationality
- numbers replaced by X for confidentiality compliance -

Nationality	Yearly average nb. of individuals	History of CC or MK	Nb. of years since last CC	Nb. of years since last MK	Kid[1-12] (CC or MK)
Romania	X	1	27	.	0.24
Russia	129	1	0	.	0.47
Rwanda	X	1	0	22	0.66
Saudi Arabia	X	1	37	.	0.33
Senegal	145	1	5	.	0.61
Serbia	284	1	17	17	0.36
Sierra Leone	59	1	15	.	0.75
Slovakia	X	0	.	.	0
Somalia	852	1	0	25	0.67
South Africa	X	1	28	.	0.86
Spain	X	1	25	.	0.75
Sri Lanka	1547	1	7	7	0.67
Sudan	157	1	0	5	1.00
Syria	1463	1	0	34	0.50
Tajikistan	X	1	5	.	0.44
Tanzania	X	0	.	.	0
Togo	77	1	30	.	0.41
Tunisia	787	1	36	.	0.31
Turkey	548	1	0	.	0.55
Turkmenistan	X	0	.	.	0
Uganda	21	1	0	30	1.00
Ukraine	40	1	0	.	0
United States of America	X	1	0	.	0
Uzbekistan	X	1	12	.	0.35
Venezuela	X	1	24	.	0.83
Vietnam	X	1	.	41	0.62
Yemen	42	1	0	.	0.91
Zambia	X	0	.	.	0
Zimbabwe	11	1	37	.	0.23

A2 Political Asylum in Switzerland

The Demand for Asylum in Switzerland – Switzerland has 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 2016 about 25% 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, for our sample period of 2009-2016, the yearly average of asylum seekers (detaining a N-permit) was around 34'300 individuals, corresponding to about 0.4% of the Swiss population. The biggest cohorts came from Eritrea, Afghanistan, Sri Lanka, Syria, Nigeria, Somalia, Tunisia, Iraq, China and Turkey (see Table 1 in the main text and Figure A1.2).

According to the United Nations Refugees Convention (1951), which was 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.

The Procedure of Assessing Asylum Requests – Most asylum seekers enter Switzerland through one of the borders near which the reception and procedure centers (RPC) are located.[1] Only few arrive by plane in one of the two airport centers (Geneva and Zurich) or through asylum applications filed at Swiss embassies abroad.[2] In the RPC, asylum seekers go through a first interview, where they are asked to provide identity proofs, fingerprints, and their application reasons. If doubts persist about the identity and the application reasons, language tests or lie detection techniques are used. After these initial procedures during which the asylum seekers stay in the RPC, authorities declare "non-credible" ("non-entrée en matière") a large portion of the demands. Typical examples of demands judged "non-credible" are from nationals who either originate from safe countries, or who do not collaborate with the 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.[3] 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 asylum seekers whose demands are judged credible receive the N-permit[4] (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

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

[2] 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.

[3] 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.

[4] 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.

naturally takes substantial time. Moreover, if the number of applications increases the process becomes even longer, given the constant capacities of the authorities. The length of the asylum process (from the moment the demand was made in one of the RPC until the first decision) was on average 163 days in 2012, increased to 401 days in 2014 and decreased to 249 days in 2016.[5]

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 cantons.

Once an asylum seeker has been allocated to a given canton, the canton in charge organizes the interviews and the accommodation in cantonal centers or individual flats, as well as provides financial assistance.[6] The diversity of accommodation possibilities across cantons is explained by the housing availabilities, size of the canton, responsible organizations, etc. Note 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.

The Chances of Being Granted Refugee Status – Between 2009-2016, according to our estimations, on average about 10% 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 other registered immigrants (like e.g. migrants from EU countries), including the freedom to change the residence canton, the access to 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), according to the State Secretariat of Migration annual reports. 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 in principle from the same social assistance as N-permit holders (for up to 7 years), but some cantons offer them more generous social assistance.[7]

The remaining treated demands are rejected and can be appealed at the Federal Administrative Court which makes the final decision. In this case, during the process, the asylum seeker sees her/his N-permit prolonged. If the final decision is negative, some proportion leaves voluntarily, while there are also some 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 A2.3 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 current human rights abuses have much smaller chances (see Table A4.5 in section A4). For example, residents from Eritrea have a 32.3%

[5]These numbers are taken from the State Secretariat of Migration Annual Reports (2016, 2012 and 2008).

[6]Between 2008 and 2015, cantons received on average 1051 CHF per month per asylum seeker from the Confederation (i.e. the Swiss central government). The sum of money each asylum seeker receives varies across cantons and across time in accordance with the housing and consumer prices. The Asylum Law (1998, Art. 82, Alin. 3) stipulates that this social aid should be provided as much as possible in the form of accommodation, food, medical insurance, but not in cash and the total amount per asylum seeker should be lower than for the other registered immigrants.

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

chance of being offered the B-permit, whereas residents from Algeria have only a chance of 2%. While the chances of obtaining a B-permit mostly depend on the current political situation in the home country, the chances for a 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.

There are incentives to behave in a law-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 must see their asylum demands automatically rejected and are expelled.

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.[8] 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 special hardship demands need to be accepted by the canton and then they have to be approved at the federal level. While the cantonal decision cannot usually be repealed, the federal decision can be repealed.

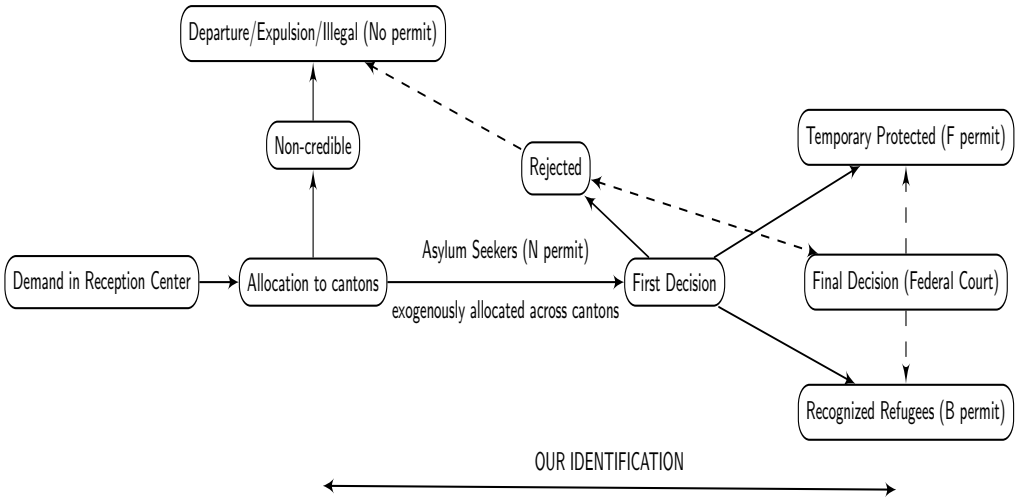
Asylum Policy and Legislative Framework in Switzerland – The legal competence of asylum is at the federal level, so cantons must implement the Asylum Law (1998). An exception is if the federal law delegates certain competencies to the cantons. Regarding the authorization to engage in gainful employment, Art. 43, alin. 1 of the Asylum Law stipulates that for the first three months after filing an asylum application, asylum seekers may not engage in any gainful employment. In consequence, for this period of time, cantons cannot allow access to the labor market to N-permit holders. After this three months period the cantons have some leeway in deciding on labor market access, which is what we exploit in Section 7 of the paper.

In addition, the Asylum Law specifies that the authorization for access to the labor market is given in accordance to the conditions specified in the Foreign National Act (2005), which is the federal immigration law. Art. 21, alin. 1 of the Foreign National Act (2005) specifies the order of priority for a vacancy to be filled: if no Swiss national, C-permit or B-permit foreigner are found to suit the job, then nationals from countries with whom Switzerland signed an agreement for the free circulation of persons (especially EU member states) are allowed to be employed, and, if lastly no such person is found, then nationals from other countries are allowed to be employed. Also, following the Foreign National Act, the cantonal migration authorities have a wide discretionary power in allowing access to the labor market.

In practice, no canton allows access to the labor market for asylum seekers in the first three months after they filled their demand, some cantons extend this period to six months and some cantons limit the access to certain sectors in need of labor force (e.g. agriculture, construction, hotel industry and medical care, following Hofmann, Buchmann and Trummer, 2014), following the order of priority by nationalities as described above. Moreover, apart from the fact that most asylum seekers with a job are typically employed in low paid sectors, in addition to the usual taxes, social and medical insurance, asylum seekers pay 10% of their salary (up to 10 years or up to a total of 15'000 Swiss Francs) for administrative efforts of assessing their asylum demands. Unsurprisingly, the occupation rate of asylum seekers remains very low (around 6%

[8] According to the Law for the Integration of Foreigners (2007), 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.

Figure A2.3: Timeline and Outcomes of the Swiss Asylum Process



Notes: The timeline follows the Swiss Asylum Law (1998) which was last modified on 1 October 2016. This description of the process is a simplified version of the timeline presented in https://www.fluechtlingshilfe.ch/assets/hilfe/asylverfahren-kurz-erklaert/bfm-asylschema-d.pdf.

according to Table A10.31 in section A10) and most of them rely on social aid.

The modification of the Foreign National Act (2005) from 2008 and the Law for the Integration of Foreigners (2007) mentioned for the first time the encouragement of the integration on the labor market of foreign nationals who are lawful residents in Switzerland for a long term (Art. 4, alin. 2). Asylum seekers are not considered to be lawful residents as long as they are not recognized as refugees (B-permit holders) or granted a temporal admission (F-permit holders).

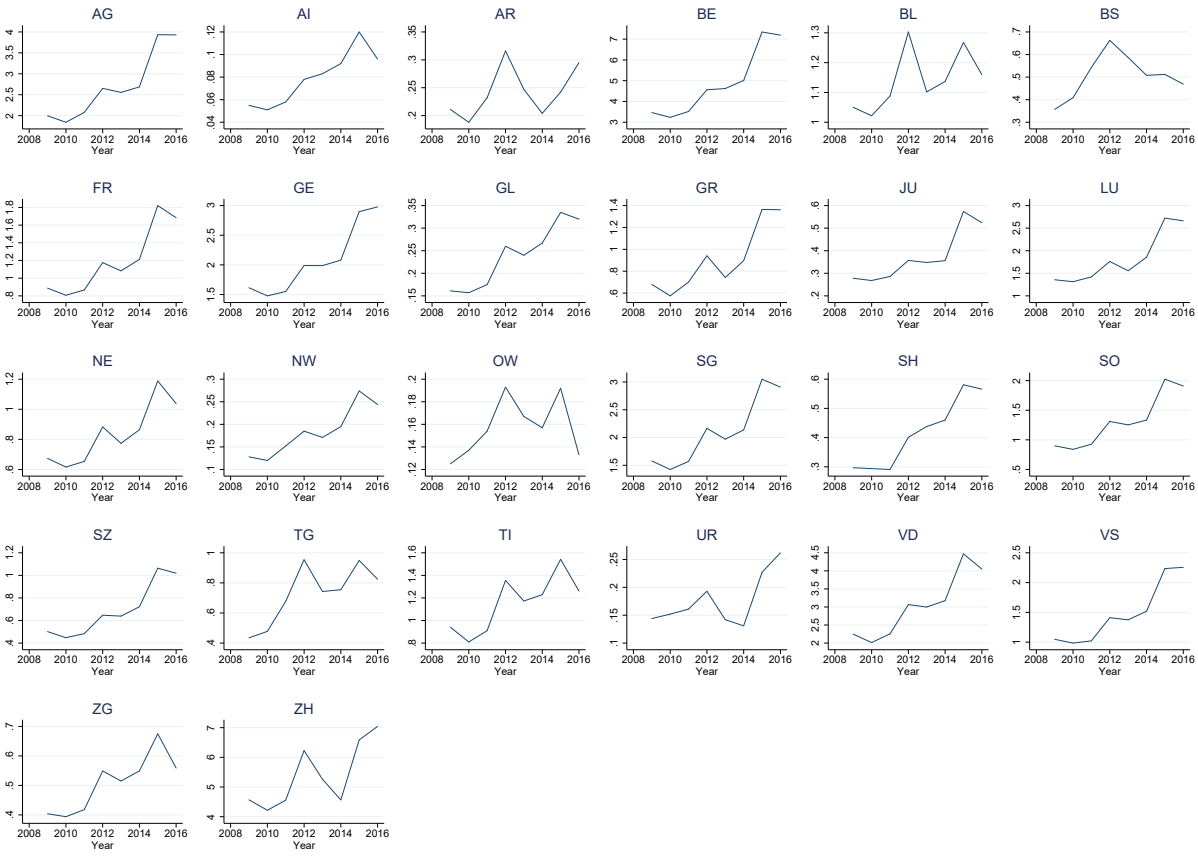
The federal program "Working Giving a Chance" was introduced in 2012 to encourage integration of immigrants in the labor market, with a specific focus on B-permit and F-permit holders. Again, there has been less emphasis at the federal level on boosting access to the labor market for asylum seekers, with most initiatives having been taken rather at the cantonal level. In Section 7 of the paper we precisely exploit the variation in cantonal policies for studying what factors may be able to attenuate or boost the impact of war experience on crime propensity.

A3 Tests of Cantonal Allocation

The purpose of this section is to complement the analysis of the main text of whether there is indeed an exogenous allocation of asylum seekers following the official population-based distribution key –as the Federal administration claims– 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 were more likely to host young males fleeing a conflict zone while rural cantons were to host other asylum seekers.

Figure A3.4 displays the time series evolution of asylum seekers stocks across the 26 Swiss cantons between 2009-2016.

Figure A3.4: Evolution of Adult Asylum Seeker Stocks by Cantons



Note: The graph plots the numbers of adult asylum seekers (in thousands) by cantons between 2009-2016.

A3.1 F-Tests of Exogenous Allocation

Below we implement a more flexible (and demanding) approach than the one of the main text by testing for the difference in means between cantons for a large array of cohort-level characteristics. We estimate the following regression equation separately for each nationality n of asylum seekers:

$$\text{COHORT CHARACTERISTIC}_{n,a,t,c} = \sum_{c=1}^{c=26} \beta_c \times \mathbb{I}_{n,a,t,c} + \varepsilon_{a,t}, \tag{A3.1}$$

where the estimation sample of adult cohorts is disaggregated at the canton-level, β_c are canton-specific coefficients and $\mathbb{I}_{n,a,t,c}$ is a binary variable equal to 1 if cohort (n, a, t) is allocated to canton c. The dependent variable is a cohort-specific characteristic corresponding to: childhood exposure to civil conflict or mass killing (KID [1-12]); childhood exposure to civil conflict (KID [1-12] (ONLY CC)); childhood exposure to mass killing (KID [1-12] (ONLY MK)); number of asylum seekers in the cohort (labeled *Cohort size*); male dummy equal to 1 if the cohort is composed of males (labeled *Male*);share of asylum seekers who arrive with family members (labeled *% Have Family*); share of asylum seekers working in services or office (labeled *% White Collar*); number of different ethnic groups in the cohort (labeled *# Ethnic Groups*); age brackets of the cohort.[9]

For each nationality of asylum seekers n, we assess the number of cantons for which the F-test of the null hypothesis $\hat{\beta}_c = \mu_n$ is rejected, where μ_n is the nationality-level average of the cohort characteristic under consideration (i.e. $\text{COHORT CHARACTERISTIC}_{n,a,t,c}$). In case of exogenous spatial placement in a given canton, the canton-specific coefficient β_c should not be different from the nationality-level average, and the F-test should *not* be rejected for this canton. In other words, we test whether there is an under/over representation of an observable characteristic in some cantons with respect to the nationality-level average. Table A3.2 displays the results. Each row represents the share of nationalities for which the F-test is rejected at the 10 percent cutoff in at most 0, 1, 5 and 10 cantons. For instance, for 64,62% of the nationalities, no canton-specific coefficient is different from the nationality-level average of KID [1-12] and for almost all nationalities (i.e. 95.38%), less than 10 canton-specific coefficients differ from the average. We observe some departure from the asymptotic case –corresponding to 100% of nationalities with rejection in zero canton. The reason is that, in our sample of asylum seekers, the size of cohorts is usually small; hence 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 series of robustness checks (Online Appendix Section A5.4) we show that our results are not driven by sampling errors. Overall, the results are supportive of our identifying assumption that the allocation of asylum seekers across cantons can be considered as exogenous with respect to their childhood exposure to conflict, age, family composition, education level, and gender.

A3.2 Post-Conflict Exposure and Selection into Migration

A first complement to our balancing test analysis in the main text is to study possible systematic differences between exposed/non-exposed cohorts in a multivariate way. Table A3.3 displays the estimation results of a specification where KID [1-12] is regressed on all characteristics and a test of joint significance is then conducted. Column (7) that encompasses all the available observed cohort characteristics indicates the absence of joint significance.

A second complement consists in investigating further dimensions of possible selection into migration. With this respect, inter-ethnic differences in the propensity to migrate could play an important role, i.e. individuals who were exposed to conflict in childhood could systematically originate from different ethnic groups compared to the non-exposed. Unfortunately, our ability to address this question is limited, as the information on ethnic affiliation in the administrative data is both sparse and coarse: 15% of the observations

[9]Information on the sector of occupation is especially incomplete. Considering the information we have, we define white collar asylum seekers as those working in services or office, science and arts or architecture and technology.

Table A3.2: F-Tests of Exogenous Allocation: Percentage of Nationality

Number of cantons	0	1	5	10
KID [1-12]	64.62	69.23	86.92	95.38
KID [1-12] (ONLY CC)	63.85	68.46	87.69	95.38
KID [1-12] (ONLY MK)	83.08	85.38	92.31	97.69
Cohort Size	66.15	69.23	74.62	83.85
Male	53.08	61.54	86.15	96.15
% Have Family	52.31	60.77	80.77	89.23
% White Collar	82.5	85	97.5	100
# Ethnic Groups	68.42	74.74	78.95	84.21
Age [18-29]	46.92	53.08	86.15	96.15
Age [30-39]	46.15	60	92.31	99.23
Age [40-49]	55.38	70.77	87.69	96.15
Age [50-59]	65.38	77.69	93.08	97.69
Age [59+[	78.46	89.23	96.92	98.46

Note: This table is based on the estimation results of equation (A3.1) on the sample of adult cohorts of asylum seekers disaggregated at the canton-level. The null hypothesis is $\hat{\beta_c} = \mu_n$ where μ_n is the nationality-level average of the cohort characteristic under consideration. Each row represents the share of nationalities for which the F-test of this null hypothesis is rejected at the 10 percent cutoff in at most 0, 1, 5 and 10 cantons.

Table A3.3: Balancing Tests: Multivariate Analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable				KID [1-12]			
Cohort Size	0.000	0.000	0.000	0.000	0.000	0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Below Median Age		-0.014	-0.014	-0.006	-0.005	0.020	0.000
		(0.063)	(0.063)	(0.064)	(0.064)	(0.075)	(0.157)
% Male			0.025	0.031	0.028	0.031	-0.025
			(0.028)	(0.028)	(0.028)	(0.034)	(0.097)
% Have Family				0.026	0.023	0.019	0.166
				(0.026)	(0.026)	(0.030)	(0.124)
Enclave Size/Cohort Size					0.000	0.000	0.000
					(0.001)	(0.001)	(0.001)
Nb. Ethnic Groups						0.009	0.019
						(0.017)	(0.012)
% White collars							-0.058
							(0.059)
Observations	21,320	21,320	21,320	20,662	20,662	17,131	3,217
R-squared	0.486	0.486	0.486	0.490	0.490	0.501	0.529
F-test	0.0439	0.0294	0.264	0.457	0.417	0.315	0.644
Prob > F	0.834	0.971	0.851	0.767	0.836	0.927	0.717

Note: OLS estimations based on the sample of adult cohorts of asylum seekers originating from countries that have experienced civil conflict or mass killing since 1946. Robust standard errors are clustered at the nationality level. The dependent variable stands for the binary measure of childhood exposure to civil conflict or mass killings. All estimations include age fixed effects, nationality × year fixed effects and gender fixed effects.

are missing, and only 151 different ethnic affiliations are reported by the Swiss administration for a total of 131 different nationalities. This is clearly underreported, as we know from reliable information sources that

there are more than 850 ethnic affiliations only in Africa (Murdock, 1959) and around 1250 ethnic groups worldwide (Weidmann, Rød and Cederman, 2010). With this caveat in mind, we now look at whether cohorts exposed during childhood systematically originate from specific ethnic groups. To this purpose, we aggregate our data on asylum seekers by nationality n, ethnic group e and age a, for each year t, and estimate the following equation for each nationality n:

$$\text{KID [1-12]}_{e,a,t} = \sum_{e=1}^{e=151} \beta_c \times \mathbb{I}_{e,a,t} + \varepsilon_{e,a,t}, \tag{A3.2}$$

where $\text{KID [1-12]}_{n,e,a,t}$ is coded 1 if the cohort of nationality n, ethnic group e and age a, in year t was exposed to violence during childhood, and 0 otherwise.[10] $\mathbb{I}_{n,e,a,t}$ is coded 1 if the cohort is from the ethnic group e. We start by carrying out a balancing test by performing for each nationality a joint test of significance of the set of ethnic group dummies. For 80% of the nationalities, the dummies of ethnic group are not jointly significant and the KID [1-12] dummy is balanced along the ethnicity dimension. Note that in Table 5 we show that the inclusion of ethnic group fixed effects does not affect our baseline estimates: We conclude that our results are unlikely to be driven by the 20% of nationalities for which KID [1-12] is unbalanced across ethnic groups.

[10] Note that we cannot use this level of aggregation in our baseline estimates, as we do not have information on the ethnic group of the perpetrators of violence.

A4 Likelihood To Be Granted Asylum

In this section we test whether there is any difference in the incentives to apply for asylum in Switzerland between those exposed to violence during childhood and those born after, using as a proxy the relative chances of being granted the residence permit. In other words, we estimate the following regression equation:

$$\begin{aligned}\text{ACCEPTANCE RATE}_{n,a,t} =& \alpha_1 \times \text{KID [1-12]}_{n,a,t} + \alpha_2 \times \text{KID [1-12]}_{n,a,t-1} + \\ &+ \sum_{k=13}^{k=80+} \beta(k) \times \text{EXPO}(k)_{n,a,t} + \mathbf{FE}_a + \mathbf{FE}_{n,t} + \varepsilon_{n,a,t}\end{aligned} \tag{A4.3}$$

This regression equation is similar to the baseline regression equation (1), but the dependent variable is now the acceptance rate or the cohort-specific likelihood to be granted the residence permit. We construct this variable by making use of the information on the reasons for which the N-permit of asylum seekers comes to an end. This allows us to construct a narrow definition of acceptance rate for which the end reason is the grant of residence permit to only recognized refugees, or a broader definition including other reasons for obtaining the residence permit.[11] The variables of interest are KID[1-12] but also its lag. Results are displayed below in Table A4.4, with Panel A focusing on the narrow definition and Panel B on the broader definition. We find that both with and without nationality × year fixed effects, and with and without lags, exposure to violence during childhood (KID[1-12]) does not affect the chances of obtaining asylum.

The previous exercise investigates a within-nationality predictor of successful asylum applications. We now look at between-nationality predictors. So we investigate whether the likelihood of being granted refugee status at the nationality × year level could be explained by country of origin characteristics. More precisely, we estimate the following regression equation:

$$\text{ACCEPTANCE RATE}_{n,t} = \beta \text{COUNTRY CHARACTERISTICS}_{n,t-1} + \mathbf{FE}_n + \mathbf{FE}_t + \varepsilon_{n,t} \tag{A4.4}$$

where $\mathbf{FE}_t$ are year fixed-effects and $\mathbf{FE}_n$ are nationality fixed-effects. The variables of interest are a set of lagged country characteristics: a measure for whether the country n experienced civil conflict or mass killings in year t, institutional quality (Polity IV) and economic development (GDP per capita). Results are displayed in Table A4.5. Again in Panel A we use the narrow definition of the asylum application being accepted while in Panel B we use the broader definition. Columns (1) to (3) display the results conditional on year fixed effects. Current violence in the country of origin increases the likelihood to be granted asylum, while the level of democracy decreases it. Economic development does not have any impact. From columns (4) to (6) we further control for nationality fixed effects. With no surprise most of the identifying variations are absorbed by the fixed effects and none of these variables seem to matter anymore. Overall those findings confirm that, thanks to the inclusion of the nationality × year fixed effects, our baseline equation (1) is immune to omitted variable bias related to prospects of success/failure in the asylum application.

[11] Other reasons for being granted the residence permit include: hardship cases due to economic or personal reasons (but not for persecution reasons as it is the case for recognized refugees) or situations where the asylum seeker has stayed in Switzerland for more than five years and proven his economic and social integration in the Swiss society.

Table A4.4: Cohort-specific Likelihood to Be Granted Residence Permit and Exposure to Violence

	(1)	(2)	(3)	(4)
Dependent Variable	Acceptance Rate			
Panel A	Residence permit only by recognized refugees			
KID [1-12]	0.007	0.003	0.000	-0.005
	(0.022)	(0.011)	(0.009)	(0.013)
L.KID [1-12] (CC OR MK)		0.004		-0.002
		(0.024)		(0.013)
Age FE	Yes	Yes	Yes	Yes
Nationality × Year FE	No	No	Yes	Yes
Observations	10,014	7,332	9,944	7,300
R-squared	0.063	0.082	0.448	0.496
Sample Mean (Acc. Rate)	0.095	0.10	0.096	0.10
	(5)	(6)	(7)	(8)
Dependent Variable	Acceptance Rate			
Panel B	Residence permit also for other reasons			
KID [1-12]	0.009	0.004	0.002	-0.005
	(0.022)	(0.012)	(0.009)	(0.013)
L.KID [1-12] (CC OR MK)		0.004		-0.004
		(0.024)		(0.013)
Age FE	Yes	Yes	Yes	Yes
Nationality × Year FE	No	No	Yes	Yes
Observations	10,014	7,332	9,944	7,300
R-squared	0.062	0.082	0.436	0.491
Sample Mean (Acc. Rate)	0.099	0.10	0.099	0.10

Note: OLS estimations based on the sample of adult male cohorts of asylum seekers originating from countries that have experienced civil conflict or mass killing since 1946. Robust standard errors are clustered at the nationality level. All estimations include a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$. Panel A focuses on the narrow definition of acceptance rate (i.e. residence permit granted to only recognized refugees) and Panel B on the broader definition (i.e. including residence permit obtained also for other reasons).

Table A4.5: Explaining the Nationality-Year Likelihood to Be Granted a Residence Permit by Country of Origin Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Acceptance Rate					
Panel A	Residence permit only by recognized refugees					
L.Civil Conflict or Mass Killing	0.070	0.065	0.081	0.006	0.008	0.014
	(0.026)	(0.028)	(0.033)	(0.018)	(0.018)	(0.019)
L.Polity IV		-0.006	-0.005		-0.003	0.002
		(0.002)	(0.002)		(0.004)	(0.005)
L.GDP/cap			-0.000			0.000
			(0.000)			(0.000)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Nationality Fixed Effects	No	No	No	Yes	Yes	Yes
Observations	805	778	646	792	765	633
R-squared	0.037	0.081	0.082	0.460	0.460	0.467
Sample Mean (Acc. Rate)	0.081	0.083	0.074	0.082	0.083	0.075
	(7)	(8)	(9)	(10)	(11)	(12)
Dependent Variable	Acceptance Rate					
Panel B	Residence permit also for other reasons					
L.Civil Conflict or Mass Killing	0.070	0.065	0.081	0.011	0.013	0.021
	(0.026)	(0.028)	(0.033)	(0.019)	(0.019)	(0.020)
L.Polity IV		-0.006	-0.005		-0.003	0.001
		(0.002)	(0.002)		(0.004)	(0.005)
L.GDP/cap			-0.000			0.000
			(0.000)			(0.000)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Nationality Fixed Effects	No	No	No	Yes	Yes	Yes
Observations	805	778	646	792	765	633
R-squared	0.033	0.074	0.074	0.455	0.455	0.459
Sample Mean (Acc. Rate)	0.088	0.089	0.082	0.088	0.089	0.083

Note: OLS estimations based on the sample of adult male cohorts of asylum seekers originating from countries that have experienced civil conflict or mass killing since 1946. Robust standard errors are clustered at the nationality level. The measure for whether a given country n experienced civil conflict or mass killings in year t is constructed from UCDP (2017) and Political Instability Task Force (2016). The measure of democracy comes from Polity IV index (2017). GDP per capita is the real GDP per capita at constant prices, from Penn World Table (2018).

A5 Sensitivity Analysis

In this section we show that the baseline estimate of Table 5, Panel A, column (2), is robust to a battery of sensitivity checks. In particular we present robustness checks with respect to potential outliers, perform a Placebo test for the estimates of conflict exposure during childhood, and show the results for alternative levels of clustering, alternative sample sizes and alternative estimation methods. Further, we display the estimates based on the sample of cohorts disaggregated at the canton-level and document the robustness of our estimates when controlling for additional cohort-level cohorts.

A5.1 Outlier Analysis

Below we assess the robustness of the main results to potential outliers –our main concern being that a small number of countries could drive our results. Firstly, we start with replicating our baseline specification excluding specific set of countries. Figure A5.5 reports estimates of column (2) of Panel A in Table 5 when excluding countries accounting for first the largest share of asylum seekers in Switzerland, then excluding cumulatively the two largest and so on until excluding the 50 largest countries in terms of asylum seekers shares. The estimated coefficient remains positive and significant in almost all cases. In addition, Figure A5.6 reports estimates when each country is excluded one by one. Estimates remain robust which suggests that the baseline results do not rely on a specific country.

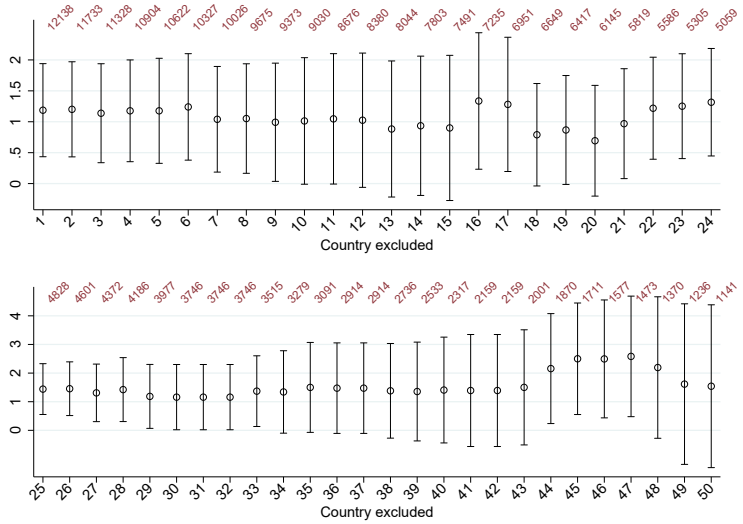
Secondly, we further investigate the robustness of our baseline results to the exclusion of outliers (Table A5.6). In columns (1) to (3) we exclude observations that are 3, 2 and 1 standard deviation away from the residual mean. Then, we exclude observations with high leverage, i.e. when the individual leverage is superior to $2k/N$ (with k being the number of predictors and N the number of observations) (column 4). Finally, we exclude observations that shift the estimate at least to one standard error (dfbeta) (column 5), and observations that shift the estimate at least to $4/N$ (Cook's distance) (column 6). The estimated coefficient remains robust across all these specifications.

Table A5.6: Outlier Analysis

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Violent Crime Propensity					
Sample	Excl. 3-sigma outliers	Excl. 2-sigma outliers	Excl. 1-sigma outliers	Leverage measure	Influence measure (dfbeta)	Influence measure (Cook's distance)
KID [1-12]	0.866	0.734	0.623	1.074	1.085	0.782
	(0.363)	(0.358)	(0.311)	(0.480)	(0.454)	(0.346)
Age FE	Yes	Yes	Yes	Yes	Yes	Yes
Nationality × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,413	12,340	12,186	11,778	12,564	12,260
R-squared	0.346	0.367	0.403	0.193	0.200	0.378
Sample Mean (Crime Prop.)	2.18	1.94	1.61	3.21	3.09	1.84

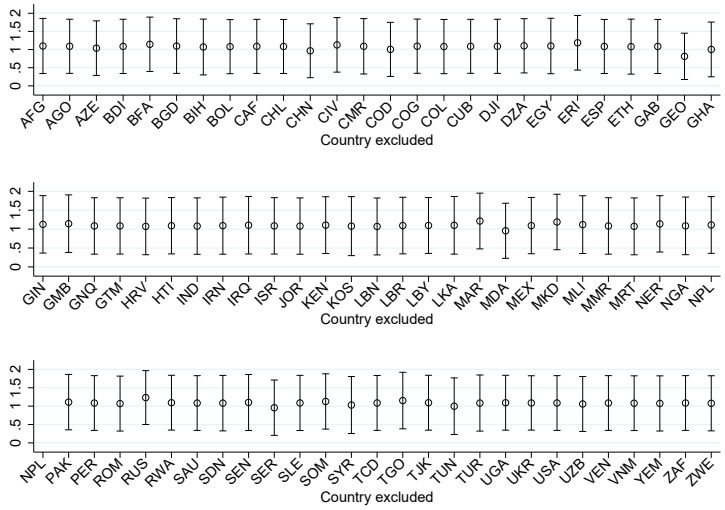
Note: OLS estimations based on the sample of adult male cohorts of asylum seekers originating from countries that have experienced civil conflict or mass killing since 1946. Robust standard errors are clustered at the nationality level. The dependent variable stands for the violent crime propensity of a cohort of nationality (n) × age (a) × year (t). KID [1-12] is a binary measure of childhood exposure to civil conflict or mass killings. All estimations include age 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 consists of the individuals born after the last years of violence. In column (4) we exclude observations with high leverage, i.e when the individual leverage is superior to $2k/N$ (k is the number of predictors and N is the number of observations). In column (5) we exclude observations that shift the estimate at least to one standard error (dfbeta). In column (6) we exclude observations that shift the estimate at least to $4/N$ (Cook's distance).

Figure A5.5: Exclusion of Countries One to Many



Note: The estimations are based on the sample of cohorts of male asylum seekers aged at least 18 years old and originating from countries that have experienced civil conflict or mass killing since 1946. The estimated coefficients and confidence intervals at 90% are reported. Each estimated coefficient and confidence interval emanate from a single estimation. The number above each observation is the number of observations in the underlying regression, while the number below is its rank.

Figure A5.6: Exclusion of Countries One by One

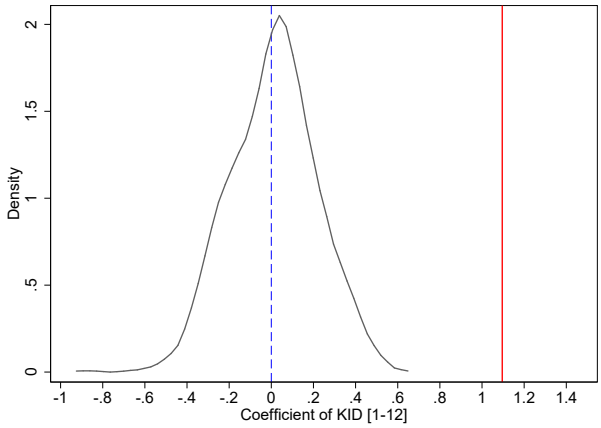


Note: The estimations are based on the sample of adult male cohorts of asylum seekers, originating from countries that have experienced civil conflict or mass killing since 1946. The figure reports the estimated coefficients and confidence intervals at 90%.

A5.2 Placebo Test of Conflict Exposure during Childhood

As mentioned in section 4, 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. In the goal of assessing the risk of having found spurious results, in this section we perform 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 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 (Table 5, Panel A, column 2) on this "fake" dataset. This procedure is applied for a large number of realizations (1,000 draws). Figure A5.7 displays the sampling distribution of the point estimates of the coefficient of KID$[1-12]$ from column (2) of Panel A in Table 5 across Monte Carlo draws. This distribution proves to be centered around zero and confirms that the likelihood of spuriously estimating a coefficient equal to or above our baseline point estimate of 1.085 is very small.

Figure A5.7: Monte Carlo Simulations



Note: The figure displays the re-estimates of column (2) of Panel A in Table 5 with a random draw of our variable KID$[1-12]$ (1000 draws), using cohorts of adult male asylum seekers originating from countries that have experienced civil conflict or mass killing since 1946.

A5.3 Alternative Crime Regressions

In this section we estimate alternative crime regressions. First, we test different levels of clustering of the robust standard errors as an alternative to the nationality level retained in the baseline. Table A5.7 replicates the baseline estimates (column 2 of Panel A in Table 5), clustering robust standard errors at the *nationality*×*year* and, separately, at the *nationality*×*age* levels. We observe no inflation of the standard errors with respect to their baseline counterparts and the coefficient of interest retains statistical significance at conventional thresholds in all the specifications. These results confirm that, with 77 nationalities, the number of clusters in the baseline specifications is a priori sufficiently large for avoiding estimation bias potentially linked to a small number of clusters.

Secondly, in Table A5.8 we consider alternative econometric specifications for the crime regression. In columns (1) to (8) we consider an aggregate logit. It simply consists of an OLS crime regression where the dependent variable is now the log(odds-ratio) of violent crime propensity, namely $\ln \frac{\text{CP}}{\text{1-CP}}$. All other features are identical to the baseline specification. Coping with (i) extreme values, and (ii) small cohorts, is problematic in such a setting. In column (1) we replace by CP = 0.001 and CP = 99.9 the extreme values of CP that are equal to 0 and 100 respectively. Though ad-hoc, this coding rule allows to force the definition of the odds-ratio for all cohorts. In the next columns, the rule is maintained but we impose additional restrictions for dealing with extreme values in CP. First, we replicate our previous specification but restrict the sample to cohorts larger than, respectively, 1, 2, 5, and 10 adults (columns 2 to 5). By focusing on larger cohorts, this mechanically reduces the in-sample share of observations with extreme values. Column (6) restricts the estimation to the subsample of cohorts where crime propensity is strictly under 100. Column (7) follows a more drastic route by keeping observations with non-zero crime propensity. The sample size reduction is massive. Column (8) combines the two previous restrictions. Finally in column (9) we follow a completely different approach by estimating a logit crime regression at the individual level. Disaggregating our dataset at this level of analysis is possible because the set of covariates consists in observable characteristics (e.g. KID$[1-12]$, EXPO(k), age, nationality, gender, canton of residence) that are common to all the cohort members. The outcome variable is now CRIME, a binary variable, equal to 1 if an individual has perpetrated a crime, and zero otherwise. In reality this outcome variable differs across individuals within cohort and we don't observe it. We circumvent this difficulty by arbitrarily coding CRIME = 1 for a share CP of cohort members (i.e. the observed crime propensity at the cohort-level) and CRIME = 0 for all the others. This random allocation is inconsequential given that all cohort members are observationally equivalent in term of attributes; thus, there is no need to rely on Monte-Carlo simulations. In all columns, the coefficient of interest remains pretty stable. The similarity of the point estimates based on log-odds ratio estimator (column 1) and logit estimator (column 9) is remarkable given how different the underlying econometric model is; this reassuringly suggests that the coding rule of extreme values in column (1) does not lead to an estimation bias. Finally, in Table A5.9, we replicate columns (1) to (8) with the log of violent crime propensity ($\ln$ CP) as dependent variable (in place of odds ratio) and a similar handling of the extreme values. Such a specification is pretty standard in the crime literature. Point estimates and statistical precision are barely affected.

Table A5.7: Alternative Level of Clustering

	(1)	(2)	(3)	(4)
Dependent Variable		Violent Crime Propensity		
Sample	Full	CC or MK	CC	MK
KID [1-12]	1.096	1.085		
cluster robust S.E. – *nationality×year*	(0.321)	(0.318)		
cluster robust S.E. – *nationality×age*	(0.300)	(0.299)		
KID [1-12] (ONLY CC)			1.220	
cluster robust S.E. – *nationality×year*			(0.325)	
cluster robust S.E. – *nationality×age*			(0.303)	
KID [1-12] (ONLY MK)				1.167
cluster robust S.E. – *nationality×year*				(0.430)
cluster robust S.E. – *nationality×age*				(0.407)
Age FE	Yes	Yes	Yes	Yes
Nationality × Year FE	Yes	Yes	Yes	Yes
Observations	13,720	12,564	12,514	5,737
R-squared	0.193	0.200	0.200	0.196
Sample Mean (Crime Prop.)	3.09	3.09	3.10	2.59

Note: OLS estimations based on the sample of adult male cohorts of asylum seekers. Robust standard errors are clustered at the *nationality×year* and, separately, at the *nationality×age* level. The dependent variable stands for the violent crime propensity of a cohort of nationality (n) × age (a) × year (t). KID [1-12] is a binary measure of childhood exposure to civil conflict or mass killings (columns 1 to 2), to civil conflict only (column 3), or to mass killings only (column 4). In columns 2 to 4, the sample is restricted to cohorts originating from countries that have experienced civil conflict or mass killings since 1946. All estimations include age 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 consists of the individuals born after the last years of violence. Abbreviations CC and MK refer to Civil Conflict and Mass Killing.

Table A5.8: Alternative Crime Regressions - I

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent Variable		Log (Odds Ratio of Violent Crime Propensity)							Logit
Sample	Full	Only cohorts > 1	Only cohorts > 2	Only cohorts > 5	Only cohorts > 10	Exclude CP = 100	Exclude CP = 0	Exclude CP = 100 & CP = 0	with expand
KID [1-12]	0.145	0.177	0.175	0.203	0.091	0.116	0.207	0.098	0.155
	(0.064)	(0.068)	(0.080)	(0.082)	(0.078)	(0.051)	(0.120)	(0.090)	(0.048)
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Nationality × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,564	8,431	6,691	4,318	2,773	12,485	2,111	2,056	142,666
R-squared	0.201	0.332	0.414	0.514	0.620	0.365	0.643	0.747	-

Note: Estimations based on the sample of adult male cohorts of asylum seekers originating from countries that have experienced civil conflict or mass killing since 1946. Robust standard errors are clustered at the nationality level. The dependent variable in columns 1 to 8 is log(odds-ratio) of violent crime propensity, namely $\ln(\frac{CP}{1\text{-}CP})$, replacing by CP = 0.001 if CP = 0 and by CP = 99.9 if CP = 100. In column 9 we estimate a logit regression at the individual level with the dependent variable being a binary variable, equal to 1 if an individual has perpetrated a crime, and zero otherwise. We code this binary variable as equal to 1 for a share of cohort members equal to CP, the observed violent crime propensity at the cohort-level (and 0 for all the others). KID [1-12] is a binary measure of childhood exposure to civil conflict or mass killings. All estimations include age 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 consists of the individuals born after the last years of violence.

Table A5.9: Alternative Crime Regressions - II

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	Log (Violent Crime Propensity)							
Sample	Full	Only cohorts > 1	Only cohorts > 2	Only cohorts > 5	Only cohorts > 10	Exclude CP = 100	Exclude CP = 0	Exclude CP = 100 & CP = 0
KID [1-12]	0.110	0.148	0.159	0.183	0.078	0.104	0.114	0.095
	(0.046)	(0.058)	(0.070)	(0.074)	(0.069)	(0.046)	(0.078)	(0.080)
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Nationality × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,564	8,431	6,691	4,318	2,773	12,485	2,111	2,056
R-squared	0.337	0.389	0.425	0.507	0.611	0.370	0.776	0.769

Note: Estimations based on the sample of adult male cohorts of asylum seekers originating from countries that have experienced civil conflict or mass killing since 1946. Robust standard errors are clustered at the nationality level. The dependent variable is the log of the violent crime propensity ln(CP). KID [1-12] is a binary measure of childhood exposure to civil conflict or mass killings. All estimations include age 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 consists of the individuals born after the last years of violence.

A5.4 Sampling Variations

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. Note that this concern is not first-order in our data given the positive results of the balancing tests. We still implement four alternative strategies to deal with this question.

First, in Table A5.10, we replicate our baseline estimates from column (2) of Panel A in Table 5, but keeping only the cohorts larger than 1, 2, 5 and 10 individuals, respectively. This approach has the advantage of simplicity but leads to informational loss.

Second, we replicate the baseline Table 5, but aggregating by 14 age brackets, which results in larger cells. The results are displayed in Table A5.11 (see the table notes on more details on the age brackets). Reassuringly, the results are similar for this alternative specification.

Further, in Table A5.12, we replicate the baseline specification after expanding the set of controls with the cohort-specific characteristics used in the balancing tests in Section 4.2. Note that we do not include those controls in our baseline specifications in order to avoid any concerns with potentially endogenous regressors. The set of observables consists in the % of asylum seekers who arrive with family members (column 1), the % of males in the cohort (column 2), the size of the enclave (i.e. economic migrants holding a B or C permit) relative to the size of the asylum seekers cohort (column 3), the number of ethnic groups (column 4); and % white collar workers (column 5).[12] In all specifications the coefficient of interest is barely changed with respect to its baseline counterpart. These results are reassuring as they confirm the conclusions drawn from the balancing tests, namely that there is no evidence of selection of KID[1-12] across observable characteristics. Moreover, applying the methodology of Oster (2017) to assess the effect of omitted cohort-specific variables, we find that our results – if anything – underestimate the true effect.

In addition, we allow for the inclusion of canton × year fixed effects in our econometric model (equation 1). To this purpose we must disaggregate our cohort-level sample at the canton level. In this case, the dependent variable becomes $\text{CP}_{c,n,a,t}$, the crime propensity of cohort n, a, t in canton c. We replicate the entire baseline Table 5, Panel A in this more fine-grained setting with the additional set of fixed effects and with the error terms two-way clustered at both nationality and canton × year levels. The regression equation becomes thus:

$$\text{CP}_{c,n,a,t} = \alpha \times \text{KID}\ [1\text{-}12]_{n,a,t} + \sum_{k=13}^{k=80+} \beta(k) \times \text{EXPO}(k)_{n,a,t} + \mathbf{FE}_a + \mathbf{FE}_{n,t} + \mathbf{FE}_{c,t} + \varepsilon_{c,n,a,t} \qquad \text{(A5.5)}$$

Results are presented in Table A5.13. The coefficient of interest remains positive but statistically significant in only one of the four columns. Note however that the R-squared is substantially smaller, indicating a less good fit of this disaggregated specification.

Finally, we also study how accounting for ethnicity could affect our main regression results. In particular, Table A5.14 below replicates our baseline Table 5 for men with the inclusion of a set of ethnic group fixed effects (e.g., if a cohort contains only members of, say, some ethnic group A and of some other ethnic group B, then both the fixed effects for groups A and B take a value of 1, while the fixed effects for all other ethnic groups become 0). Interestingly, the inclusion of ethnic group fixed effects does not affect the magnitude of our baseline estimate.

[12] Information on the individual occupation of asylum seekers is especially incomplete. Given the information at disposal, we code as white collar workers those asylum seekers who work in services or office, science and arts or architecture and technology.

Table A5.10: Alternative Sample Size

	(1)	(2)	(3)	(4)
Dependent Variable	Violent Crime Propensity			
Sample size	> 1	> 2	> 5	> 10
KID [1-12]	1.347	1.271	1.565	0.910
	(0.518)	(0.697)	(0.674)	(0.711)
Age FE	Yes	Yes	Yes	Yes
Nationality × Year FE	Yes	Yes	Yes	Yes
Observations	8,431	6,691	4,318	2,773
R-squared	0.298	0.382	0.509	0.621
Sample Mean (Crime Prop.)	3.82	4.18	4.89	5.09

Note: OLS estimations based on the sample of adult male cohorts of asylum seekers originating from countries that have experienced civil conflict or mass killing since 1946. Robust standard errors are clustered at the nationality level. The dependent variable stands for the violent crime propensity of a cohort of nationality (n) × age (a) × year (t). KID [1-12] is a binary measure of childhood exposure to civil conflict or mass killings. All estimations include age 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 consists of the individuals born after the last years of violence.

Table A5.11: Aggregation by Age Brackets

	(1)	(2)	(3)	(4)
Dependent Variable	Violent Crime Propensity			
Sample	Full	CC & MK	CC	MK
KID [1-12]	1.136	1.156		
	(0.512)	(0.510)		
KID [1-12] (ONLY CC)			1.592	
			(0.511)	
KID [1-12] (ONLY MK)				0.602
				(0.863)
Age FE	Yes	Yes	Yes	Yes
Nationality × Year FE	Yes	Yes	Yes	Yes
Observations	4,659	4,110	4,070	1,698
R-squared	0.338	0.351	0.352	0.379
Sample Mean (Crime Prop.)	2.49	2.49	2.51	2.11

Note: OLS estimations based on the sample of adult cohorts of asylum seekers. Robust standard errors are clustered at the nationality levels. The dependent variable stands for the violent crime propensity of a cohort of nationality (n) × age bracket (a) × year (t). The 14 brackets 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+]. KID [1-12] is a binary measure of childhood exposure to civil conflict or mass killings (columns 1 to 2), to civil conflict only (column 3), or to mass killings only (column 4). In columns (2) to (4), the sample is restricted to cohorts originating from countries that have experienced civil conflict or mass killings since 1946. Panel A is confined to the male adult cohorts and Panel B to the female adult cohorts of asylum seekers. All estimations include age fixed effects, nationality × year fixed effects and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$. Therefore, the group of reference consists of the individuals born after the last years of violence. Abbreviations CC and MK refer to Civil Conflict and Mass Killing.

Table A5.12: Controlling for Cohort Specific Characteristics

	(1)	(2)	(3)	(4)	(5)
Dependent Variable			Violent Crime Propensity		
COHORT CHARACTERISTIC	% Have Family	Sex Ratio	# Enclave Size/ Cohort Size	# Ethnic Groups	% White Collar
KID [1-12]	0.957	1.101	1.070	1.194	1.536
	(0.460)	(0.455)	(0.449)	(0.500)	(0.595)
COHORT CHARACTERISTIC	-0.054	-1.017	-0.006	-0.099	-1.290
	(0.481)	(0.513)	(0.001)	(0.096)	(0.469)
Age FE	Yes	Yes	Yes	Yes	Yes
Nationality × Year FE	Yes	Yes	Yes	Yes	Yes
Observations	12,234	12,568	12,568	10,486	2,476
R-squared	0.199	0.200	0.201	0.220	0.353
Sample Mean (Crime Prop.)	2.97	3.09	3.09	3.49	2.72

Note: OLS estimations based on the sample of adult male cohorts of asylum seekers originating from countries that have experienced civil conflict or mass killing since 1946. Robust standard errors are clustered at the nationality level. The dependent variable stands for the violent crime propensity of a cohort of nationality (n) × age (a) × year (t). KID [1-12] is a binary measure of childhood exposure to civil conflict or mass killings. All estimations include age fixed effects, nationality × year fixed effects and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$. Therefore, the group of reference consists of the individuals born after the last years of violence.

Table A5.13: The Impact of Childhood Conflict Exposure on Crime Propensity: Cohort × Canton level

	(1)	(2)	(3)	(4)
Dependent Variable		Violent Crime Propensity		
Sample	Full	CC or MK	CC	MK
KID [1-12]	1.030	1.028		
	(0.675)	(0.684)		
KID [1-12] (ONLY CC)			0.991	
			(0.713)	
KID [1-12] (ONLY MK)				1.415
				(0.758)
Age FE	Yes	Yes	Yes	Yes
Nationality × Year FE	Yes	Yes	Yes	Yes
Canton × Year FE	Yes	Yes	Yes	Yes
Observations	64,241	62,350	62,296	35,568
R-squared	0.095	0.095	0.095	0.075
Sample Mean (Crime Prop.)	4.32	4.32	4.32	3.33

Note: OLS estimations based on the sample of adult male cohorts of asylum seekers. Robust standard errors are two-way clustered at the nationality and canton × year levels. The dependent variable stands for the violent crime propensity of a cohort of nationality (n) × age (a) × year (t) in canton (c). KID [1-12] is a binary measure of childhood exposure to civil conflict or mass killings (columns 1 to 2), to civil conflict only (column 3), or to mass killings only (column 4). In columns 2 to 4, the sample is restricted to cohorts originating from countries that have experienced civil conflict or mass killings since 1946. All estimations include age fixed effects, nationality × year fixed effects, canton × year fixed effects and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$. Therefore, the group of reference consists of the individuals born after the last years of violence. Abbreviations CC and MK refer to Civil Conflict and Mass Killing.

Table A5.14: Violent Crime Propensity and Conflict Exposure: Controlling for Ethnic Groups

	(1)	(2)	(3)	(4)
Dependent Variable	Violent Crime Propensity			
Sample	Full	CC & MK	CC	MK
KID [1-12]	1.170	1.137		
	(0.499)	(0.495)		
KID [1-12] (ONLY CC)			1.229	
			(0.517)	
KID [1-12] (ONLY MK)				1.163
				(0.562)
Ethnic Group FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Nationality × Year FE	Yes	Yes	Yes	Yes
Observations	11,500	10,642	10,629	5,121
R-squared	0.221	0.234	0.234	0.260
Sample Mean (Crime Prop.)	3.48	3.45	3.46	2.75

Note: OLS estimations based on the sample of men adult cohorts of asylum seekers. Robust standard errors are clustered at the nationality level. The dependent variable stands for the violent crime propensity of a cohort of nationality (n) × age (a) × year (t). KID [1-12] is a binary measure of childhood exposure to civil conflict or mass killings (columns 1 to 2), to civil conflict only (column 3), or to mass killings only (column 4). In columns (2) to (4), the sample is restricted to cohorts originating from countries that have experienced civil conflict or mass killings since 1946. All estimations include age fixed effects, nationality × year fixed effects, a set of binary variables coding for past exposure, but at later ages $k \in \{13, 14, 15, ..., 80+\}$ and a set of ethnic group fixed effects. Therefore, the reference group consists of those individuals born after the last years of violence. Abbreviations CC and MK refer respectively to Civil Conflict and Mass Killing.

A6 Additional Results on Age Effects

In this section we report additional results on age effects (Section 5.2 of the paper).

A6.1 Impressionable Years

Inspired by Giuliano and Spilimbergo (2014), we first investigate whether there is a specific impact on violent criminal behaviors of conflict exposure during the "impressionable years", namely the age period in early adulthood between 18 to 25. There are two main reasons why this age bracket is less suitable in our context than in theirs: First of all, as shown in the psychological literature cited above, violent behaviors tend to emerge in early childhood and 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. Table A6.15 replicates our baseline Table 5, Panel A (i.e. adult male asylum seekers) when controlling for exposure between 18 and 25 years old, accounting at the same time for exposure between 13 and 17 years and exposure after 26 years old. We find a positive, but imprecisely estimated, coefficient of conflict exposure during the impressionable years on future criminal behaviors. Quite reassuringly, our main finding 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.

Table A6.15: Exposure During 18-25 Years Old: Men

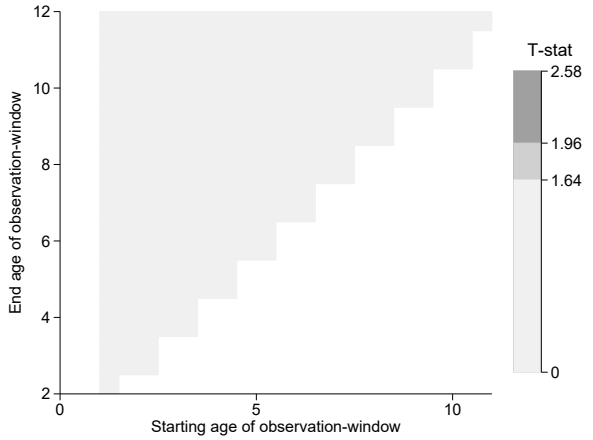
	(1)	(2)	(3)	(4)
Dependent Variable	Violent Crime Propensity			
Sample	Full	CC or MK	CC	MK
KID [1-12]	0.893	0.896	1.001	1.105
	(0.463)	(0.464)	(0.517)	(0.444)
EXPOSED [13-17]	0.019	0.017	-0.037	0.744
	(0.480)	(0.485)	(0.496)	(0.425)
EXPOSED [18-25]	0.446	0.413	0.389	1.167
	(0.536)	(0.535)	(0.554)	(0.689)
EXPOSED [26+]	0.087	-0.044	-0.029	1.243
	(0.534)	(0.497)	(0.511)	(1.039)
Age FE	Yes	Yes	Yes	Yes
Nationality × Year FE	Yes	Yes	Yes	Yes
Observations	13,720	12,564	12,514	5,737
R-squared	0.188	0.194	0.194	0.187
Sample Mean (Crime Prop.)	3.09	3.09	3.10	2.59

Note: OLS estimations based on the sample of adult male cohorts of asylum seekers. Robust standard errors are clustered at the nationality levels. The dependent variable stands for the violent crime propensity of a cohort of nationality (n) × age (a) × year (t). KID [1-12] is a binary measure of childhood exposure to civil conflict or mass killings (columns 1 to 2), to civil conflict only (column 3), or to mass killings only (column 4). In columns (2) to (4), the sample is restricted to cohorts originating from countries that have experienced civil conflict or mass killings since 1946. All estimations include age fixed effects and nationality × year fixed effects. Therefore, the group of reference consists of the individuals born after the last years of violence. Abbreviations CC and MK refer to Civil Conflict and Mass Killing.

A6.2 Aftermath of Violence

In Table 14 of the main text we investigate in column (2) a potential impact on future crime propensity of being born in the 12 years following a war, detecting no effect. In the goal of assessing the sensitivity of this result to the time window selected, Figure A6.8 displays the estimates for various windows of cohorts born after the last year of conflict. Whatever the time span of the window, we continue to detect no effect.

Figure A6.8: Aftermath of Violence: All Potential Exposure Windows



A6.3 Victimization

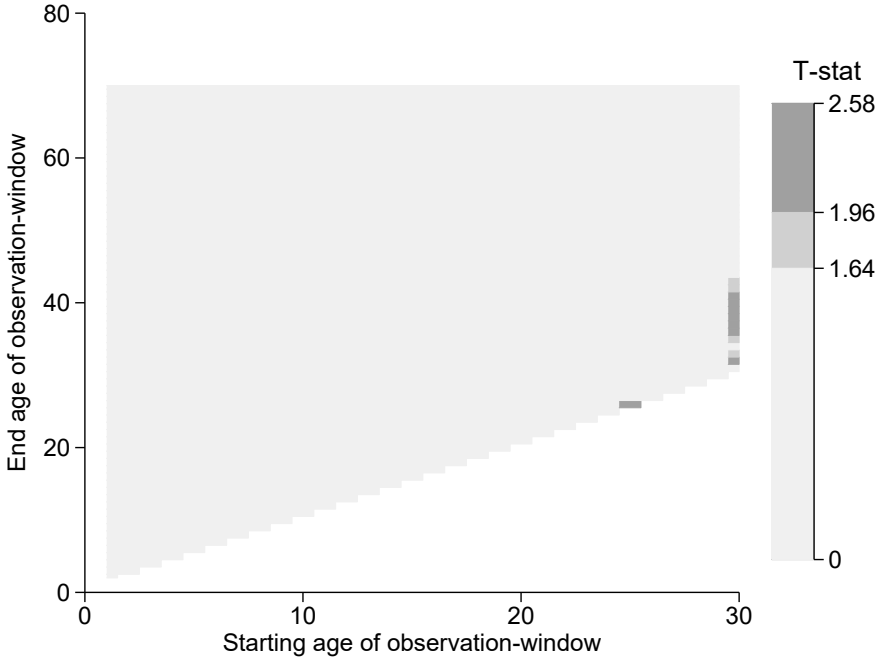
We revisit the question of victimization of male asylum seekers using exposure to violence during one's lifetime before arriving in Switzerland in Table A6.16. In addition we adopt the same agnostic perspective as in the main text (Figure 3), but for the victimization rate as dependent variable. The results are displayed in Figure A6.9. We keep on detecting no effect of conflict exposure on future victimization of men.

Table A6.16: Victimization Rate: Men

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Victimization Rate (Violent Crimes)					
		within family		within family		within family
Sample	CC or MK	CC or MK	CC	CC	MK	MK
Exposed (CC or MK)	-1.142	0.132				
	(1.645)	(0.133)				
Exposed (CC only)			-1.188	0.134		
			(1.662)	(0.135)		
Exposed (MK only)					-0.175	-0.072
					(0.661)	(0.136)
Age FE	Yes	Yes	Yes	Yes	Yes	Yes
Nationality × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,564	12,564	12,514	12,514	5,737	5,737
R-squared	0.079	0.033	0.079	0.033	0.074	0.036
Sample Mean (Vict. Rate)	3.59	0.12	3.60	0.12	3.93	0.16

Note: OLS estimations based on the sample of adult male cohorts of asylum seekers. Robust standard errors are clustered at the nationality levels. The dependent variable stands for the victimization rate of a cohort of nationality (n) × age (a) × year (t) for all violent crimes (columns 1, 3 and 5) and for violent crimes committed within one's family (columns 2, 4 and 6). Exposed is a binary measure of exposure coded 1 if the cohort was exposed to violence at any time from birth to their arrival in Switzerland and 0 otherwise, where violence refers to civil conflict or mass killings (columns 1 and 2), to civil conflict only (columns 3 and 4), or to mass killings only (columns 5 to 6). The sample is restricted to cohorts originating from countries who have experienced civil conflicts or mass killings episodes since 1946 (columns 1 and 2), only civil conflicts (columns 3 and 4) and only mass killings (columns 5 and 6). All estimations include age fixed effects and nationality × year fixed effects. The group of reference consists of the individuals born after the last years of violence. Abbreviations CC and MK refer to Civil Conflict and Mass Killing.

Figure A6.9: The Effect of Violence Exposure on Victimization Rate Using All Potential Exposure Windows: Men



Note: The figure reports estimates of equation (1), but having as variable of interest KID[s-e]), where $s \in [1, 30]$ is the lower bound and $e \in [s, 70]$ is the upper bound of the exposure window. Each pixel corresponds hence to the t-stat of the estimate of KID [S-E] for a particular combination of s and e in a separate regression. Darker colors represents higher t-stats so higher significance. The estimates are based on the sample of adult male cohorts of asylum seekers originating from countries that have experienced civil conflict or mass killing since 1946.

A6.4 Later Ages of Conflict Exposure: Women

In this Section we study the effect of later ages exposure for women, both from the perspective of their criminal behavior and their victimization. We know from the baseline results in Section 5.1 that, on average, early age exposure does not impact the future crime propensity and victimization rate of female asylums seekers. However, it could well be that exposure to conflict-induced violence is in fact more severe for women at later ages than for young girls. Indeed, in many wars and genocides, women are specifically targeted by the fighting groups (i.e. by wartime rapes).

First, we replicate Table A6.15 but for women. Results are displayed in Table A6.17. As we can notice, there is no significant effect of violence exposure on women's violent crime propensity for any of the exposure windows. Second, we estimate a version of equation (1) where the variable of exposure to violence corresponds to EXPOSED, a binary variable coded 1 if women from cohort (n, a) have experienced conflict (between birth and residence in Switzerland) and 0 otherwise (Table A6.18). The sample is restricted to female asylum seekers from countries of origins where there has been at least one year of civil war (or mass killing) since 1945. 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[13], 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. This specification reflects the fact that women are in wars in most cases on the victim side, implying that coding as war exposed women exposed even during fighting age is unlikely to threaten the identification strategy. Results show that women exposed to civil conflict or mass killings are not significantly more violent than women non-exposed but that women exposed to a conflict with systematic wartime rape are more crime prone than women who are non exposed. Moreover, exposure to high-intensity wartime rape episodes has a stronger impact on criminal behavior than exposure to such episodes of medium or low intensity.

We also revisit the question of victimization of female asylum seekers with this set of measures of later age exposure (Table A6.19). In that respect we are particularly interested in studying domestic abuse and rape of conflict-exposed women. In contrast to childhood exposure, we detect a positive effect of later age exposure to civil conflict and mass killing on the likelihood of being sexually abused domestically. The pattern is more pronounced when we focus on exposure to wartime systematic rapes. Two different mechanisms contribute potentially to this victimization result for women. First, as emphasized in the literature, early age victimization increases future vulnerability and victimization later in life. Second, assortative mating plays a role because conflict-exposed women may tend to mate with co-nationals of the same age who are likely to be exposed as well and so to be more crime-prone. Knowing which mechanism dominates is important from a policy perspective as they lead to prescriptions that differ drastically. Unfortunately data availability limits our ability to disentangle the two mechanisms as we have imperfect information on the family composition of asylum seekers. However, we know from column (3) in Table 6 that male asylum seekers who have been exposed to a conflict are not more prone to sexual assaults. This evidence suggests that assortative mating is unlikely to explain the victimization result observed among conflict-exposed female asylum seekers.

[13]For exposure to wartime rapes, we use the data of Cohen (2013), who 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 for the period 1981-2009. For our alternative measure of exposure, EXPOSED, we see that around 61% of female cohorts have experienced a conflict where wartime rapes were pervasive. Also, 30 percent of female cohorts flee a country that was plagued by wartime rape in 2009.

Table A6.17: Exposure When 18-25 Years Old: Women

	(1)	(2)	(3)	(4)
Dependent Variable	Violent Crime Propensity			
Sample	Full	CC & MK	CC	MK
Exposure to	CC & MK	CC & MK	CC	MK
KID [1-12]	0.029	0.039	-0.038	0.202
	(0.117)	(0.118)	(0.121)	(0.186)
EXPOSED [13-17]	-0.030	-0.031	-0.004	-0.032
	(0.113)	(0.108)	(0.129)	(0.121)
EXPOSED [18-25]	0.080	0.069	0.055	-0.029
	(0.122)	(0.119)	(0.125)	(0.181)
EXPOSED [26+]	-0.046	-0.141	-0.145	-0.199
	(0.220)	(0.231)	(0.233)	(0.192)
Age FE	Yes	Yes	Yes	Yes
Nationality × Year FE	Yes	Yes	Yes	Yes
Observations	9,503	8,658	8,640	5,082
R-squared	0.088	0.050	0.050	0.058
Sample Mean (Crime Prop.)	3.09	3.09	3.10	2.59

Note: OLS estimations based on the sample of adult female cohorts of asylum seekers. Robust standard errors are clustered at the nationality levels. The dependent variable stands for the violent crime propensity of a cohort of nationality (n) × age (a) × year (t). KID [1-12] is a binary measure of childhood exposure to civil conflict or mass killings (columns 1 to 2), to civil conflict only (column 3), or to mass killings only (column 4). In columns (2) to (4), the sample is restricted to cohorts originating from countries that have experienced civil conflict or mass killings since 1946. All estimations include age fixed effects and nationality × year fixed effects. Therefore, the group of reference consists of the individuals born after the last years of violence. Abbreviations CC and MK refer to Civil Conflict and Mass Killing.

Table A6.18: Exposure to Violence by Intensity: Women

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable	Violent Crime Propensity						
Sample and Exposure to	CC and MK	CC	CC	MK	MK	Rape Wars	Rape Wars
Exposed	0.076	0.078		0.409		0.436	
	(0.137)	(0.140)		(0.310)		(0.120)	
Low Intensity			-0.075		0.093		0.407
			(0.128)		(0.245)		(0.112)
Medium Intensity			0.146		0.811		0.279
			(0.169)		(0.570)		(0.145)
High Intensity			-0.208		0.539		0.786
			(0.189)		(0.199)		(0.209)
Observations	8,658	8,640	8,640	5,082	5,082	6,007	6,007
R-squared	0.050	0.050	0.050	0.058	0.059	0.053	0.053
Sample Mean (Crime Prop.)	0.27	0.27	0.27	0.32	0.32	0.29	0.29

Note: OLS estimations based on the sample of adult female cohorts of asylum seekers. Robust standard errors are clustered at the nationality levels. The dependent variable stands for the violent crime propensity of a cohort of nationality (n) × age (a) × year (t). The sample and the definition of Kid [1-12] are restricted to civil conflict or mass killings (column 1), to civil conflict only (columns 2 and 3), to mass killings only (columns 4 and 5) or to conflicts with the systematic use of wartime rapes (columns 6 and 7). All estimations include age fixed effects and nationality × year fixed effects. The group of reference consists of the individuals born after the last years of violence. Exposed is a binary measure of exposure coded 1 if the cohort was exposed to violence at any time from birth to their arrival in Switzerland and 0 otherwise. For exposure to wartime rapes, we use the data of Cohen (2013) for the period 1981-2009, which provides also the three levels of intensity. The definitions of the three levels of intensity for civil conflict and mass killing follow the intensity levels displayed in Table 15 of the Appendix.

Table A6.19: Victimization Rate: Women

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	Victimization Rate (Violent Crimes)							
Type of offences	Violent	Violent within family	Sexual	Sexual within family	Violent	Violent within family	Sexual	Sexual within family
Sample	CC or MK	CC or MK	CC or MK	CC or MK	CC	CC	CC	CC
Exposed (CC or MK)	0.733	0.697	-0.031	0.143				
	(0.844)	(0.588)	(0.496)	(0.083)				
Exposed (CC only)					0.716	0.695	-0.044	0.143
					(0.860)	(0.601)	(0.506)	(0.084)
Observations	8,658	8,658	8,658	8,658	8,640	8,640	8,640	8,640
R-squared	0.080	0.063	0.056	0.034	0.080	0.063	0.056	0.034
Sample Mean (Vict. Rate)	2.70	0.85	0.33	0.12	2.71	0.85	0.33	0.12

	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Dependent Variable	Victimization Rate (Violent Crimes)							
Type of offences	Violent	Violent Within family	Sexual	Sexual Within family	Violent	Violent Within family	Sexual	Sexual Within family
Sample	MK	MK	MK	MK	RW	RW	RW	RW
Exposed (MK only)	1.412	0.553	-0.004	-0.254				
	(0.709)	(0.368)	(0.233)	(0.236)				
Exposed (RW only)					1.843	0.482	0.524	0.243
					(0.576)	(0.288)	(0.221)	(0.125)
Observations	5,082	5,082	5,082	5,082	6,007	6,007	6,007	6,007
R-squared	0.074	0.058	0.043	0.035	0.080	0.058	0.061	0.037
Sample Mean (Vict. Rate)	3.16	0.99	0.35	0.14	3.03	0.94	0.41	0.14

Note: OLS estimations based on the sample of adult female cohorts of asylum seekers. Robust standard errors are clustered at the nationality levels. The dependent variable stands for the victimization rate of a cohort of nationality (n) × age (a) × year (t) for all violent, resp. sexual crimes (uneven columns) and for violent, resp. sexual crimes committed within one's family (even columns). Exposed is a binary measure of exposure coded 1 if the cohort was exposed to violence at any time from birth to their arrival in Switzerland and 0 otherwise. The sample is restricted to cohorts originating from countries who have experienced civil conflicts or mass killing episodes since 1946 (columns 1 to 4), only civil conflicts (columns 5 to 8), only mass killings (columns 9 to 12) and only conflicts with wartime rape (columns 13 to 16). All estimations include age fixed effects and nationality × year fixed effects. The group of reference consists of the individuals born after the last years of violence. Abbreviations CC, MK and RW refer to Civil Conflict, Mass Killing and Rape War.

A7 Contextual Factors During Childhood - Child soldiering

In Table A7.20, we show that child soldiering is not a major contextual factor driving our results. Indeed individuals who have been child soldiers were direct perpetrators of violence and may consequently reproduce this violence in adulthood. Obviously, data availability is a challenge here, as asylum seekers do not report to the Swiss authorities whether they have been themselves child soldiers. We address the question by using two different informational sources for the sake of cross-checking the results. In column (1) we replicate the baseline specification after excluding from the sample all countries that experienced a conflict where child soldiering has been reported by official UN sources (UN, 2016). In column (2) we are able to build a cohort-level measure of exposure to child soldiering before the age of 12 thanks to the database of Tynes and Early (2015). They focus on 198 conflicts identified in the Peace Research Institute Oslo (PRIO) dataset from 1987-2007. We replicate the baseline specification after controlling not only for this measure but also for exposure to child soldiering later in life (i.e. above 12). In both columns, the coefficient of KID [1-12] remains stable and precisely estimated.

Table A7.20: Accounting for Cases of Child Soldiers

	(1)	(2)
Dependent Variable	Violent Crime Propensity	
KID [1-12]	0.968	1.379
	(0.559)	(0.521)
KID [1-12] : Child Soldiering		-0.650
		(0.680)
Age FE	Yes	Yes
Nationality × Year FE	Yes	Yes
Observations	9,012	8,353
R-squared	0.208	0.270
Sample Mean (Crime Prop.)	3.71	3.86

Note: OLS estimations based on the sample of adult cohorts of asylum seekers originating from countries that have experienced civil conflict or mass killings since 1946. Robust standard errors are clustered at the nationality levels. The dependent variable stands for the violent crime propensity of a cohort of nationality (n) × age (a) × year (t). KID [1-12] is a binary measure of childhood exposure to civil conflict or mass killings. All estimations include age fixed effects, nationality × year fixed effects and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$. Therefore, the group of reference consists of the individuals born after the last years of violence. In column (1) we drop countries for which UN (2016) reports cases of child soldiers. In column (2) we compute KID[1-12]: Child Soldering which is equal to 1 if the cohort was exposed to child soldiering during childhood (between 1 and 12 years old), using the database of Tynes and Early (2015). We also control for exposure to child soldering later in life.

A8 Heterogeneous Effects

In this section we provide additional results on heterogeneous effects by gauging how the average effect of conflict exposure on criminal behaviors may be modified by a series of country-specific or cohort-specific characteristics. No remarkable pattern emerges from the analysis.

Country of Origin Characteristics – We investigate how characteristics of asylum seekers country of origin modulate our main effect in Table A8.21 .[14] We start from our baseline regression of column (2) of Table 5, Panel A, but interact KID [1-12] with three country characteristics, namely current war (column 1), measure of democracy (column 2) and GDP per capita (column 3).[15] We find that none of these three country characteristics statistically significantly influences the magnitude of the impact of war exposure during childhood on crime propensity later in life.

Ethnic Cleavages in the Home Country – We also look at the effect of ethnic cleavages in the country of origin. In Table A8.22 we make use of information on the nature of the conflict (territorial / ethnic-based or not) and the salience of ethnic cleavages (using standard measures of ethnic diversity). We draw on the data from *GROWup* (Girardin et al., 2015) and distinguish between conflicts where (i) the incompatibility was about territory or government and (ii) an ethnic component was or was not involved. The data is from *GROWup* (Girardin et al., 2015), takes as starting point the UCDP (2017) and links it with information on the ethnic group location. In column (1) we start by replicating our baseline regression of column (3) of Panel A in Table 5 but with conflict data provided by *GROWup*. The results are very similar as in the baseline. In column (2) we exploit a distinction between conflicts where there is an incompatibility concerning territory (KID $[1\text{-}12]_{\text{Territory}}$) and other conflicts with incompatibility concerning government (KID $[1\text{-}12]_{\text{Government}}$).[16] The two coefficients of interest are not statistically significantly different from each other. In column (3) we slice conflicts in even smaller categories, distinguishing also between conflicts where ethnicity played a major role versus other conflicts where ethnicity was not a major dimension. This leaves us with four categories of conflict events. The slicing in thinner categories results in an increase in standard errors, implying that none of the four variables of interest is statistically significantly. While the coefficients of KID $[1\text{-}12]_{\text{Territory \& Ethnic}}$ and KID $[1\text{-}12]_{\text{Territory \& Non-ethnic}}$ are not statistically significantly different from each other, it is found that the coefficient of KID $[1\text{-}12]_{\text{Government \& Non-ethnic}}$ is larger than of KID $[1\text{-}12]_{\text{Government \& Ethnic}}$. Overall, we do not detect strong heterogeneity between types of conflict. We cannot rule out that this non-result could be due to the coarse nature of the typology of conflict categories.

As a next step, we investigate how ethnic heterogeneity for a given nationality may modulate our main effect. While ethnic diversity in the origin country matters for explaining why conflict broke out and the context of migration, also the ethnic composition of the diasporas moving to Switzerland may have an impact on criminal behaviors. Importantly, the level of ethnic diversity of the origin country and of the sample of migrants in Switzerland may differ substantially, if for example a given ethnic group had to flee the home

[14] In addition to the three country characteristics studied here we have further ones that are influenced directly by the Swiss asylum policy, i.e. nationality-specific acceptance rates of asylum requests and the existence of readmission treaties. We shall study the impact of these variables in Section 7.

[15] The variable CURRENT CC OR MK is a dummy equal to 1 if there is a civil conflict or mass killing episode in the country of origin n in the current year t, and 0 otherwise, and it comes from the same data source as conflict exposure during childhood that we use throughout the paper, namely UCDP (2017). The Polity IV index represents the institutional quality in the country of origin n in year t, ranging from -10 (complete autocracy) to 10 (complete democracy), and is provided by Polity IV (2017). GDP/cap is the real GDP per capita at constant prices in the country of origin n in year t and is from Penn World Table (2018).

[16] Incompatibility concerning the status of a territory includes for instance the change of the state in control of a certain territory, secession or autonomy. Incompatibility concerning type of political system includes for instance the replacement of the central government, or the change of its composition.

country while other ethnic groups were able to stay. In order to properly address this issue, we would need to have high-quality data on the ethnic group adherence of asylum seekers in Switzerland. As discussed above, we have some information on the ethnicity of migrants in Switzerland, but the data is extremely coarse, with only 142 distinct ethnicities being included and the information is in many cases missing (it is available in only 15 percent of cases).[17] Further, data on the ethnicity of crime perpetrators is completely missing. Thus, due to data limitations we are not able to carry out the whole analysis at the ethnicity level (nor to include ethnicity fixed effects), and cannot go beyond investigating heterogeneous effects with respect to the ethnic composition of the home country, drawing on existing data from Reynal-Querol (2002) on ethnic fractionalization and polarization (the formal definitions of these concepts are in Montalvo and Reynal-Querol, 2005). In Table A8.23 we interact the country-level measures of ethnic fractionalization and polarization with our variable KID [1-12]. The coefficients of the interaction terms in columns (1) and (2) turn out to be negative and statistically significant. This may at first seem surprising, as higher diversity has commonly been linked to higher conflict potential (see e.g. Montalvo and Reynal-Querol, 2005). What we however have to keep in mind, as mentioned above, is that in more polarized countries the composition of refugees may be different from the overall composition in the home country. Take for example Rwanda, a country with a large ethnic polarization score. After the 1994 genocide especially the militarily defeated perpetrators from the Hutu ethnic group had to flee the country in masses, and hence while the country as a whole was very ethnically polarized, this was not necessarily the case of the migrant sub-population. Hence, the findings of Table A8.23 are hard to interpret, and should be taken with a grain of salt.

Gender Ratio – As shown above in Section 3, there is a large gender imbalance within asylum seekers in general. In columns (1) to (3) in Table A8.24 of the Appendix, we estimate whether this gender imbalance may exacerbate or mitigate the effect of childhood exposure. In particular, KID [1-12] is interacted with the percentage of women at the cohort-level (column 1), the percentage of women at the country-age level (column 2) and the percentage of women at the country level (column 3). In all three cases, the interaction terms are not statistically significant. In column (4) we interact KID [1-12] with the share of asylum seekers who arrive with family members. We do not detect a statistically significant coefficient of the interaction term. Overall, these results suggest that gender composition is not a major modulating factor of the impact of childhood war exposure.

[17] For comparison, in the classic Soviet "Atlas Narodov Mira" there are 1248 groups in total (Weidmann, Rød and Cederman, 2010).

Table A8.21: Interactions with Characteristics at the Country of Origin and Year Level

	(1)	(2)	(3)
Dependent Variable	Violent Crime Propensity		
CHARACTERISTICS	Current CC or MK	Polity IV	GDP/cap
KID [1-12]	1.429	0.903	0.597
	(0.666)	(0.447)	(0.680)
KID [1-12] × CHARACTERISTICS	-1.136	0.132	0.098
	(0.805)	(0.098)	(0.083)
Age FE	Yes	Yes	Yes
Nationality × Year FE	Yes	Yes	Yes
Observations	12,564	12,335	10,911
R-squared	0.204	0.208	0.214
Sample Mean (Crime Prop.)	3.09	3.09	3.09

Note: OLS estimations based on the sample of adult male cohorts of asylum seekers originating from countries that have experienced civil conflict or mass killings since 1946. Robust standard errors are clustered at the nationality levels. The dependent variable stands for the violent crime propensity of a cohort of nationality (n) × age (a) × year (t). KID [1-12] is a binary measure of childhood exposure to civil conflict or mass killings. All estimations include age fixed effects, nationality × year fixed effects and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$. Therefore, the group of reference consists of the individuals born after the last years of violence. The current CC or MK is a dummy equal to 1 if there is a civil conflict or mass killing episode in the country of origin n in the current year t and comes from UCDP (2017). The Polity IV index represents the institutional quality in the country of origin n in year t and comes from Polity IV (2017); GDP per capita is the real GDP per capita at constant prices in the country of origin n in year t and is from Penn World Table (2018). Abbreviations CC and MK refer to Civil Conflict and Mass Killing.

Table A8.22: Heterogeneous Effects by Types of Conflict

Dependent Variable	(1) Violent Crime	(2) Propensity	(3)
KID [1-12]$_{\text{Cederman}}$	1.172		
	(0.484)		
KID [1-12]$_{\text{Territory}}$		1.138	
		(0.687)	
KID [1-12]$_{\text{Government}}$		0.951	
		(0.580)	
KID [1-12]$_{\text{Territory \& Ethnic}}$			0.936
			(0.724)
KID [1-12]$_{\text{Territory \& Non-ethnic}}$			1.649
			(1.258)
KID [1-12]$_{\text{Government \& Ethnic}}$			-0.711
			(0.498)
KID [1-12]$_{\text{Government \& Non-ethnic}}$			0.836
			(0.654)
Observations	12,514	12,514	12,514
R-squared	0.200	0.206	0.203
Sample Mean (Crime Prop.)	3.11	3.11	3.11
P-value T-test ($\beta_{\text{Territory}} = \beta_{\text{Government}}$)		0.813	
P-value T-test ($\beta_{\text{Territory \& Ethnic}} = \beta_{\text{Territory \& Non-ethnic}}$)			0.656
P-value T-test ($\beta_{\text{Government \& Ethnic}} = \beta_{\text{Government \& Non-ethnic}}$)			0.066

Note: OLS estimations based on the sample of adult male cohorts of asylum seekers originating from countries that have experienced civil conflict since 1946. Robust standard errors are clustered at the nationality levels. The dependent variable stands for the violent crime propensity of a cohort of nationality (n) × age (a) × year (t). KID [1-12]$_{\text{Cederman}}$ is a binary measure of childhood exposure to civil conflict according to the database on conflict *GROWup*, KID [1-12]$_{\text{Territory}}$ (Ethnic or Non-Ethnic) if the conflict is about an incompatibility concerning the territory (ethnic or non-ethnic), KID [1-12]$_{\text{Government}}$ (Ethnic or Non-Ethnic) if the conflict regards an incompatibility concerning the government (ethnic or non-ethnic). The distinction ethnic versus non-ethnic refers to whether ethnicity played a major role or not. All estimations include age fixed effects, nationality × year fixed effects and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$. Therefore, the group of reference consists of the individuals born after the last years of violence.

Table A8.23: Interactions with Ethnic Fractionalization and Polarization in the Home Country

Dependent Variable	(1) Violent Crime	(2) Propensity
Ethnic Measure	Fractionalization	Polarization
KID [1-12]	2.410	3.312
	(0.716)	(0.908)
KID [1-12] × CHARACTERISTICS	-3.288	-4.966
	(1.131)	(1.524)
Age FE	Yes	Yes
Nationality × Year FE	Yes	Yes
Observations	8,701	8,701
R-squared	0.233	0.233
Sample Mean (Crime Prop.)	2.46	2.46

Note: OLS estimations based on the sample of adult male cohorts of asylum seekers originating from countries that have experienced civil conflict or mass killings since 1946. Robust standard errors are clustered at the nationality levels. The dependent variable stands for the violent crime propensity of a cohort of nationality (n) × age (a) × year (t). KID [1-12] is a binary measure of childhood exposure to civil conflict or mass killings. All estimations include age fixed effects, nationality × year fixed effects and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$. Therefore, the group of reference consists of the individuals born after the last years of violence. Ethnic fractionalization and polarization variables at the country level are taken from Reynal-Querol (2002).

Table A8.24: Interactions with Characteristics at the Cohort Level

Dependent Variable	(1)	(2)	(3)	(4)
	Violent Crime Propensity			
CHARACTERISTICS	% Women cohort level	% Women country-age level	% Women country level	% Individuals with adult family
KID [1-12]	1.230	1.751	0.736	0.880
	(0.482)	(0.736)	(0.953)	(0.564)
KID [1-12] × CHARACTERISTICS	-0.704	-2.633	1.217	0.130
	(1.456)	(2.169)	(3.016)	(0.765)
Age FE	Yes	Yes	Yes	Yes
Nationality × Year FE	Yes	Yes	Yes	Yes
Observations	12,564	12,564	12,564	12,230
R-squared	0.204	0.205	0.205	0.201
Sample Mean (Crime Prop.)	3.09	3.09	3.09	2.97

Note: OLS estimations based on the sample of adult male cohorts of asylum seekers originating from countries that have experienced civil conflict or mass killings since 1946. Robust standard errors are clustered at the nationality levels. The dependent variable stands for the violent crime propensity of a cohort of nationality (n) × age (a) × year (t). KID [1-12] is a binary measure of childhood exposure to civil conflict or mass killings. All estimations include age fixed effects, nationality × year fixed effects and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$. Therefore, the group of reference consists of the individuals born after the last years of violence. % Women at the cohort level is the percentage of women at the $n \times t \times a$ level, % Women at the country-age level is the percentage of women at the $n \times a$ level and % Women at the country level is the percentage of women at the n level. % Individuals with adult family is the percentage of individuals in the cohort who arrive in Switzerland with relatives aged at least 18 years old.

A9 Additional Results on Bilateral Crime Regressions

In this section are presented additional results and robustness checks on the bilateral crime regressions. Table A9.25 replicates Table 8 excluding intra-family or domestic crimes. The results remain unchanged. Table A9.26 displays the estimates for property crimes. Again, we do not detect any effect of childhood conflict exposure on property crime propensity. Table A9.27 displays the estimates when the sample of victims is restricted to asylum seekers (columns 1 to 3) and to Swiss natives and economic migrants (columns 4 to 6). While we find somewhat smaller magnitudes for the regressions focusing on victims being Swiss natives and economic migrants, it is striking that the results are strong and statistically significant in both cases. Table A9.28 displays estimates when the perpetrator sample is restricted to women and Table A9.29 shows that the estimates are robust to the inclusion of bilateral fixed effects.

Finally, Table A9.30 focuses on miliary inter-state disputes. The idea is to test whether targeted violence is also observed in the case of extra-territorial conflicts. For this purpose, we introduce a new variable, $\text{MID } [1\text{-}12]_{n,a,t}$, an indicator function equal to 1 when a cohort of potential perpetrators has been exposed to a MID during childhood (between age 1 and 12). We also build $\mathbb{MID}_{n,v}$, which is an indicator function taking a value of 1 when perpetrator and victim are from a country pair (n, v) having experienced a MID in the past. In column (1) we introduce $\mathbb{MID}_{n=v}$ along with the dummy indicating whether the perpetrator and the victim are co-nationals, i.e. $\mathbb{I}_{n=v}$. The latter is found to have a much larger effect on violent bilateral crime propensity than the former. In column (2) we add the interaction between $\mathbb{MID}_{n=v}$ and $\text{Mid } [1\text{-}12]_{n,a,t}$, which is the childhood exposure to MID, as well as the interaction between $\text{Kid } [1\text{-}12]_{n,a,t} \times \mathbb{I}_{n=v}$, which is our variable of interest. Only the latter interaction turns out to be significant. The magnitude of our main coefficient of interest ($\text{kid } [1\text{-}12]_{n,a,t} \times \mathbb{I}_{n=v}$) is unchanged when controlling for MID. The results on targeted violence in the case of MID are hard to interpret. Quantitatively the effect is sizable: the likelihood of targeting a national from a former enemy country doubles for perpetrators exposed to a MID during their childhood (from 0.030 to $0.071 = 0.03 + 0.041$, using coefficients from column 2). However, the coefficients are imprecisely estimated in spite of the large sample size. A likely reason is that the probability of encountering a national from a former enemy country is low among asylum seekers (e.g. an Iraqi asylum seeker encountering a US citizen in Switzerland) whereas this probability is much larger for co-nationals (an Iraqi asylum seeker meeting an Iraqi). Another likely reason is that, after 1946, civilian populations have often been less exposed to direct violence during an inter-state war than during a civil conflict or genocide: Not only are interstate wars much more rare, but they are typically also associated with a smaller death toll, even in the case of high intensity interstate wars (level 5 of MID) such as e.g. the Falklands War and "Soccer War" killing about 1000, respectively 5000 people.[18] In column (3) the nationality of victim $\times$ nationality of perpetrator fixed effects are included. This implies that $\mathbb{MID}_{n=v}$ and $\mathbb{I}_{n=v}$ drop from the specification, as perfectly collinear with these additional fixed effects. Overall, it is found that childhood exposure to civil conflict and mass killings has a quantitatively very sizeable and statistically significant impact on future crime propensity, while we do not detect a statistically significant effect of MID exposure during childhood.

[18] See http://www.systemicpeace.org/warlist/warlist.htm.

Table A9.25: Bilateral Crime Regressions: Excluding Intra-family Crimes

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Violent Bilateral Crime Propensity					
Exposure to	CC or MK	CC	MK	CC or MK	CC	MK
$\mathbb{I}_{n=v}$	0.843	0.815	1.227	0.880	0.851	1.279
	(0.218)	(0.216)	(0.197)	(0.267)	(0.265)	(0.286)
KID [1-12]$_{n,a,t} \times \mathbb{I}_{n=v}$	0.804	0.887	0.603	0.792	0.877	0.593
	(0.401)	(0.407)	(0.475)	(0.426)	(0.432)	(0.547)
$\mathbb{I}_{v=\text{CH}}$				1.706	1.660	2.403
				(0.185)	(0.182)	(0.234)
KID [1-12]$_{n,a,t} \times \mathbb{I}_{v=\text{CH}}$				0.576	0.692	-0.438
				(0.246)	(0.250)	(0.358)
Nationality of perpetrator × Year × Age FE	Yes	Yes	Yes	Yes	Yes	Yes
Nationality of victim × Year FE	Yes	Yes	Yes	No	No	No
Observations	2,380,666	2,370,854	1,084,442	2,380,666	2,370,854	1,084,442
R-squared	0.027	0.027	0.030	0.025	0.026	0.027
Sample Mean (Bilateral Crime Prop.)	0.04	0.04	0.05	0.04	0.04	0.05

Note: OLS estimations based on the sample of male adult cohorts of asylum seekers originating from countries that have experienced civil conflict or mass killing since 1946. Robust standard errors are two-way clustered at the nationality of perpetrator and nationality of victim levels. The dependent variable *Violent Bilateral Crime Propensity* corresponds to bilateral propensity to violent crime. $\mathbb{I}_{n=v}$ is a binary indicator function equal to 1 if the perpetrator and the victim are co-nationals. $\mathbb{I}_{v=\text{CH}}$ is a binary indicator function equal to 1 if victim is Swiss. KID [1-12]$_{n,a,t}$ is a binary measure of childhood exposure to civil conflict or mass killing. All estimations include a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$ interacted with $\mathbb{I}_{n=v}$. Therefore, the group of reference consists of the individuals born after the last years of violence. Abbreviations CC and MK refer to Civil Conflict and Mass Killing.

Table A9.26: Bilateral Crime Regressions: Property Crimes

	(1)	(2)	(3)
Dependent variable	Property Bilateral Crime Propensity		
Exposure to	CC or MK	CC	MK
$\mathbb{I}_{n=v}$	0.126	0.123	0.155
	(0.055)	(0.054)	(0.060)
KID [1-12]$_{n,a,t} \times \mathbb{I}_{n=v}$	0.068	0.077	0.169
	(0.072)	(0.072)	(0.105)
Nationality of perpetrator × Year × Age FE	Yes	Yes	Yes
Nationality of victim × Year FE	Yes	Yes	Yes
Observations	2,380,666	2,370,854	1,084,442
R-squared	0.058	0.059	0.052
Sample Mean (Bilateral Crime Prop.)	0.05	0.05	0.04

Note: OLS estimations based on the sample of male adult cohorts of asylum seekers originating from countries that have experienced civil conflict or mass killing since 1946. Robust standard errors are two-way clustered at the nationality of perpetrator and nationality of victim levels. The dependent variable *Property Bilateral Crime Propensity* corresponds to bilateral propensity to property crime. $\mathbb{I}_{n=v}$ is a binary indicator function equal to 1 if the perpetrator and the victim are co-nationals. KID [1-12]$_{n,a,t}$ is a binary measure of childhood exposure to civil conflict or mass killing. All estimations include a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$ interacted with $\mathbb{I}_{n=v}$. Therefore, the group of reference consists of the individuals born after the last years of violence. Abbreviations CC and MK refer to Civil Conflict and Mass Killing.

Table A9.27: Victims: Asylums Seekers Versus Swiss Natives and Economic Migrants

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Violent Bilateral Crime Propensity					
Victim Sample	Only asylum seekers			Swiss natives and economic migrants		
Exposure to	CC or MK	CC	MK	CC or MK	CC	MK
$\mathbb{I}_{n=v}$	0.926	0.907	1.208	0.297	0.290	0.538
	(0.224)	(0.221)	(0.201)	(0.076)	(0.075)	(0.095)
KID [1-12]$_{n,a,t} \times \mathbb{I}_{n=v}$	0.847	0.915	1.281	0.614	0.649	0.743
	(0.401)	(0.407)	(0.531)	(0.181)	(0.185)	(0.334)
Nationality of perpetrator × Year × Age FE	Yes	Yes	Yes	Yes	Yes	Yes
Nationality of victim × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,380,666	2,370,854	1,084,442	2,380,666	2,370,854	1,084,442
R-squared	0.026	0.027	0.036	0.030	0.030	0.034
Sample Mean (Bilateral Crime Prop.)	0.01	0.01	0.02	0.03	0.03	0.03

Note: OLS estimations based on the sample of male adult cohorts of asylum seekers originating from countries that have experienced civil conflict or mass killing since 1946. Robust standard errors are two-way clustered at the nationality of perpetrator and nationality of victim levels. The dependent variable *Violent Bilateral Crime Propensity* corresponds to bilateral propensity to violent crime. $\mathbb{I}_{n=v}$ is a binary indicator function equal to 1 if the perpetrator and the victim are co-nationals. KID [1-12]$_{n,a,t}$ is a binary measure of childhood exposure to civil conflict or mass killing. All estimations include a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$ interacted with $\mathbb{I}_{n=v}$. Therefore, the group of reference consists of the individuals born after the last years of violence. Abbreviations CC and MK refer to Civil Conflict and Mass Killing.

Table A9.28: Bilateral Crime Regressions: Female Perpetrators

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Violent Bilateral Crime Propensity			Violent Bilateral Crime Propensity - No Family		
Exposure to	CC or MK	CC	MK	CC or MK	CC	MK
$\mathbb{I}_{n=v}$	0.270	0.259	0.387	0.167	0.160	0.228
	(0.075)	(0.073)	(0.111)	(0.069)	(0.067)	(0.107)
KID [1-12]$_{n,a,t} \times \mathbb{I}_{n=v}$	0.335	0.369	0.423	0.203	0.224	0.343
	(0.132)	(0.133)	(0.201)	(0.113)	(0.114)	(0.164)
Nationality of perpetrator × Year × Age FE	Yes	Yes	Yes	Yes	Yes	Yes
Nationality of victim × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,652,053	1,647,714	962,563	1,652,053	1,647,714	962,563
R-squared	0.012	0.012	0.014	0.009	0.009	0.010
Sample Mean (Bilateral Crime Prop.)	0.005	0.005	0.006	0.004	0.004	0.005

Note: OLS estimations based on the sample of female adult cohorts of asylum seekers originating from countries that have experienced civil conflict or mass killing since 1946. Robust standard errors are two-way clustered at the nationality of perpetrator and nationality of victim levels. The dependent variable *Violent Bilateral Crime Propensity* corresponds to bilateral propensity to violent crime. $\mathbb{I}_{n=v}$ is a binary indicator function equal to 1 if the perpetrator and the victim are co-nationals. KID [1-12]$_{n,a,t}$ is a binary measure of childhood exposure to civil conflict or mass killing. All estimations include a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$ interacted with $\mathbb{I}_{n=v}$. Therefore, the group of reference consists of the individuals born after the last years of violence. Abbreviations CC and MK refer to Civil Conflict and Mass Killing.

Table A9.29: Bilateral Crime Regressions: Inclusion of Bilateral Fixed Effects

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Violent Bilateral Crime Propensity			Bilateral Violent Crime Propensity - No Family		
Exposure to	CC, MK	CC	MK	CC, MK	CC	MK
KID [1-12]$_{n,a,t} \times \mathbb{I}_{n=v}$	2.201	2.281	2.369	1.428	1.509	0.970
	(0.715)	(0.723)	(0.732)	(0.619)	(1.188)	(0.546)
Nationality of perpetrator × Year × Age FE	Yes	Yes	Yes	Yes	Yes	Yes
Nationality of victim × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Nationality of victim × Nationality of perpetrator FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,379,509	2,369,698	1,084,247	2,379,509	2,369,698	1,084,247
R-squared	0.069	0.069	0.073	0.057	0.057	0.056
Sample Mean (Bilateral Crime Prop.)	0.04	0.04	0.05	0.04	0.04	0.04

Note: OLS estimations based on the sample of male adult cohorts of asylum seekers originating from countries that have experienced civil conflict or mass killing since 1946. Robust standard errors are two-way clustered at the nationality of perpetrator and nationality of victim levels. The dependent variable *Violent Bilateral Crime Propensity* corresponds to bilateral propensity to violent crime. $\mathbb{I}_{n=v}$ is a binary indicator function equal to 1 if the perpetrator and the victim are co-nationals. KID [1-12]$_{n,a,t}$ is a binary measure of childhood exposure to civil conflict or mass killing. All estimations include a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$ interacted with $\mathbb{I}_{n=v}$. Therefore, the group of reference consists of the individuals born after the last years of violence. Abbreviations CC and MK refer to Civil Conflict and Mass Killing.

Table A9.30: Bilateral Crime Regressions: Militarized Interstate Disputes (MID)

	(1)	(2)	(3)
Dependent Variable	Violent Bilateral Crime Propensity		
$\mathbb{MID}_{n,v}$	0.044	0.033	
	(0.025)	(0.029)	
$\mathbb{I}_{n=v}$	2.024	1.224	
	(0.310)	(0.262)	
MID [1-12]$_{n,a,t} \times \mathbb{MID}_{n,v}$		0.046	0.021
		(0.044)	(0.042)
KID [1-12]$_{n,a,t} \times \mathbb{I}_{n=v}$		1.449	2.200
		(0.518)	(0.715)
Nationality of perpetrator × Year × Age FE	Yes	Yes	Yes
Nationality of victim × Year FE	Yes	Yes	Yes
Nationality of victim × Nationality of perpetrator FE	No	No	Yes
Observations	2,303,460	2,303,460	2,302,346
R-squared	0.034	0.036	0.069
Sample Mean (Bilateral Crime Prop.)	0.04	0.04	0.04

Note: OLS estimations based on the sample of male adult cohorts of asylum seekers originating from countries that have experienced civil conflict or mass killing since 1946. Robust standard errors are two-way clustered at the nationality of perpetrator and nationality of victim levels. The dependent variable *Violent Bilateral Crime Propensity* corresponds to bilateral propensity to violent crime. $\mathbb{MID}_{n,v}$ is a binary indicator function equal to 1 if perpetrator and victim are from a country pair having had a militarized inter-state dispute (MID). $\mathbb{I}_{n=v}$ is a binary indicator function equal to 1 if perpetrator and victim are co-nationals. MID [1-12] is a binary measure of childhood exposure to MID. KID [1-12]$_{n,a,t}$ is a binary measure of childhood exposure to civil conflict or mass killing. All estimations include a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, ..., 80+\}$ interacted with $\mathbb{I}_{n=v}$. Therefore, the group of reference consists of the individuals born after the last years of violence.

A10 Additional Results on Integration Policies

A10.1 Data Description

In what follows we describe in detail the integration policy variables of interest and control variables used in Section 7 and the current appendix section.

Open job access is coded as as 0 in cantons where the cantonal administration (i) extends the working ban beyond three months or (ii) restricts work by asylum seekers to certain sectors of activity or (iii) explicitly discourages their access to jobs. As discussed in subsection A2, 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`). The cantons have the power to prolong this generalized work ban for a longer duration and also to restrict work permits delivery to only a subset of sectors of the economy. *Source:* The offficial communication of the highest inter-cantonal authority Konferenz der Sozialdirektoren, `http://sodk.ch/fileadmin/user_upload/Fachbereiche/Migration/2012.08.27_Schreiben_SODK_an_SPK-S_Asylgesetzrevision_Webversion_d.pdf` and `http://www.sodk.ch/fileadmin/user_upload/Fachbereiche/Migration/2017/2017.06.29_tableau_et_explications.pdf`, and personal communication with cantonal officials.

Occupation rate corresponds to the average occupation rate of all asylum seekers at the level of the canton as average during the whole period 2009-2016 or at the level of canton $\times$ year in the previous year. It is constructed as the ratio between the number of employed asylum seekers and the total number of asylum seekers. *Source:* Federal Statistical Office.

Civic courses takes the value of 1 if general knowledge or civic education 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" (Wichmann et al., 2011), question 17 (are counted as civic education courses both "Cours de culture generale" and "Education civique et cours similaires").

Active labor market measures takes the value of 1 if a given canton has put in place at least one of the following: public or private occupation programs, integration conventions, promotion of labor market or professional integration measures, and 0 otherwise *Source:* Wichmann et al. (2011), survey on "Migration and Federalism", questions 17 and 18.

Language courses takes the value of 1 if basic or advanced language courses are offered, and 0 otherwise. *Source:* Wichmann et al., (2011), Survey on "Migration and Federalism", question 17. Language courses are considered both "Cours de langue pour usage quotidien" and "Cours de langues de niveau approfondi".

Private management takes the 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 0 otherwise. *Source:* `www.abs-ag.ch`, `www.ors.ch` and private communication with canton officials.

Financial social assistance 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).

Pre-sample cantonal GDP per capita measures the cantonal economic performance at current prices

(in 1000 CHF) per capita in 2008 *Source:* Federal Statistical Office.

Pre-sample cantonal unemployment rate measures the cantonal unemployment rate in 2008. *Source:* Federal Statistical Office.

Pre-sample right-wing vote represents the average percentage of citizens in a canton voting in the period 1991-2007 for the main right-wing party, the Swiss People Party (UDC in French or SVP in German). *Source:* `http://www.bfs.admin.ch/bfs/portal/de/index/themen/17/02/blank/key/national_rat/mandatsverteilung.html`.

Second generation employment rates gap is the difference between the employment rate of natives and the employment rate of second generation immigrants at the canton level in 2012. *Source:* Swiss Survey on Active Population Database.

French-speaking is a dummy variable of whether a canton is (in majority) French-speaking or not. *Source:* Federal Statistical Office.

Acceptance rate corresponds to the percentage of recognized refugees (residence B permit) among asylum seeker demands, by nationality n in 2008. *Source:* Authors' calculations based on the raw data that we have received from the Federal Office for Migration (FOM).

Readmission treaties takes the value of 1 if there exists a bilateral readmission agreement between Switzerland and the country of origin n (i.e. an agreement of the origin country to take back migrants who have not obtained refugee status in Switzerland). *Source:* Coded using the official list of readmission treaties from the FOM: `www.sem.admin.ch/sem/fr/home/internationales/internat-zusarbeit/bilateral/rueckuebernahme.html`.

Pre-sample country-level population is the population in the country of origin in 2008. *Source:* World Development Indicators - World Bank (2017).

Pre-sample country-level GDP per capita is the GDP per capita (PPP, Constant International USD) in the country of origin in 2008. *Source:* World Development Indicators - World Bank (2017).

Country-level ethnic fractionalization is the ethnic fractionalization in the country of origin. *Source:* Fearon (2003).

Pre-sample Polity IV score measures the level of democracy in the country of origin in 2008. *Source:* Polity IV democracy scores from Polity IV (2017).

In Table A10.31 we further display the summary statistics for the policy and control variables and in Table A10.32 we show the correlations between these variables. Interestingly, while variables capturing the promotion of economic and social integration are typically positively correlated, the correlation is far from perfect, pointing out the need to consider all the various aspects of public policy.

Table A10.31: Integration Policies and Controls: Summary Statistics

Variable	Obs.	Mean	Std. Dev.	Max	Min
Cantonal variables of interest					
Open job access	25	0.640	0.490	0	1
Civic courses	22	0.409	0.503	0	1
Occupation rate	26	0.059	0.039	0.002	0.201
Active labor market measures	22	0.727	0.456	0	1
Private management	26	0.269	0.452	0	1
Language courses	22	0.773	0.429	0	1
Financial social assistance	26	6.993	0.127	6.75	7.385
Country of origin variables of interest					
Pre-sample acceptance rate	62	0.147	0.156	0	0.669
Readmission agreement	66	0.227	0.422	0	1
Cantonal controls					
Pre-sample cantonal GDP/cap	26	68303	24291	47165	147769
Pre-sample unemployment rate	26	2.254	1.129	0.8	5.7
Right-wing vote	24	17.451	9.136	4.814	33.464
Second gen. empl. rates gap	26	0.021	0.046	-0.107	0.064
French-speaking canton	26	0.269	0.452	0	1
Country of origin controls					
Population	66	5.03e+07	1.64e+08	686223	1.32E+09
GDP/cap	63	8234.543	9481.63	577.615	43567.31
Ethnic fractionalization	63	0.563	0.242	0.039	0.933
Polity IV index	62	1.226	6.028	-10	10

Note: All variables are time-invariant. For the cantonal variables the unit of observation is the canton (c), while for the country of origin variables the unit of observation is the nationality level (n).

Table A10.32: Cantonal Integration Policies and Controls: Correlations

Variables	OJA	CC	OR	ALM	PM	LC	SA	GDP	UR	UDC	2GER	FR
Open job acc. (OJA)	1.000											
Civic courses (CC)	0.167	1.000										
Occup. rate (OR)	0.231	0.158	1.000									
Active lab. mkt. meas. (ALM)	0.091	0.510	0.176	1.000								
Private manag. (PM)	-0.089	-0.171	-0.363	-0.239	1.000							
Language courses (LC)	-0.032	0.451	0.049	0.886	-0.328	1.000						
Social assistance (SA)	0.123	-0.214	-0.198	-0.090	-0.126	-0.164	1.000					
GDP/cap (GDP)	-0.021	0.014	-0.030	0.080	-0.019	0.033	0.611	1.000				
Unemployment rate (UR)	-0.170	0.250	-0.146	-0.100	-0.037	-0.032	0.419	0.411	1.000			
Right-wing vote (UDC)	0.141	-0.071	0.192	-0.002	0.344	-0.146	-0.217	-0.036	-0.389	1.000		
Sec. gen. empl. rate (2GER)	-0.112	0.025	0.193	0.263	-0.194	0.273	-0.567	-0.159	-0.518	0.309	1.000	
French canton (FR)	-0.089	0.321	-0.268	0.146	0.023	0.332	-0.130	-0.030	0.597	-0.336	-0.086	1.000

Note: All variables are time-invariant and the unit of observation for all variables is the canton (c).

A10.2 Balancing Tests for Policy Variables

Below we present the balancing tests for five key policy variables, namely OPEN JOB ACCESS, ABOVE MEDIAN OCCUPATION RATE DUMMY, CIVIC COURSES, PRIVATE MANAGEMENT, and LANGUAGE COURSES. These results are organized analogously to the balancing test we have carried out for cohort characteristics (Table 4) with the same sample used as in the baseline regressions (i.e. the canton-level sample of adult cohorts of asylum seekers originating from countries that have experienced civil conflict or mass killing since 1946). The first five columns of A10.33 display the unconditional correlation, while columns (6) to (10) include the set of control variables $\text{EXPO}(k)_{n,a,t}$ and the battery of fixed effects $\mathbf{FE}_{n,t}$, $\mathbf{FE}_a$ and $\mathbf{FE}_g$ used in our master econometric equation (1). In most columns KID [1-12] does not statistically significantly correlate with the main policy variables and the coefficients estimated are small compared to the sample means of the policy variables. In particular, the correlation is never statistically significant for our key policy variable of open job access, and the estimated coefficient is of dismal magnitude.

Table A10.33: Balancing Tests: Policy Variables

Dependent Variable	(1) Open Job Access	(2) High Occ. Rate	(3) Civic Courses	(4) Private Manag.	(5) Language Courses
Panel A	Unconditional				
KID [1-12]	-0.001	0.013	-0.011	0.003	-0.008
	(0.006)	(0.013)	(0.005)	(0.016)	(0.005)
Observations	93,696	94,176	84,215	94,176	84,215
R-squared	0.000	0.000	0.000	0.000	0.000
Sample Mean (Dep. Var)	0.62	0.40	0.44	0.45	0.72
	(0.49)	(0.49)	(0.5)	(0.5)	(0.45)
Dependent Variable	(6) Open Job Access	(7) High Occ. Rate	(8) Civic Courses	(9) Private Manag.	(10) Language Courses
Panel B	Country × Year, Age and Gender Fixed Effects				
KID [1-12]	-0.005	0.003	-0.015	0.011	-0.014
	(0.005)	(0.005)	(0.006)	(0.008)	(0.005)
Observations	93,635	94,115	84,150	94,115	84,150
R-squared	0.011	0.022	0.018	0.029	0.015
Sample Mean (Dep. Var)	0.62	0.40	0.44	0.45	0.72
	(0.49)	(0.49)	(0.5)	(0.5)	(0.45)

Note: OLS estimations based on the canton-level sample of adult cohorts of asylum seekers originating from countries that have experienced civil conflict or mass killing since 1946. Robust standard errors are clustered at the nationality level. The dependent variable consists of open job access (OJA), above median occupation rate dummy (high OR), civic courses (CC), private management (PM), and language courses (LC). The explanatory variable is the exposure to civil conflict or mass killings between 1 and 12 years old, i.e. KID [1-12] (CC OR MK).

A10.3 Additional Results on Labor Market Integration

Propensity Score Matching: Open Job Access– The PSM methodology provides a flexible way in which various factors are allowed to affect the treatment and reduces the dimensionality of the estimation of the treatment effect.

As argued by Imbens and Wooldridge (2009) and Imbens and Rubin (2015), linear regressions rely heavily on extrapolation and hence are sensitive to the specification of the regression function. This issue can be attenuated through the use of propensity scores in various ways, leading Imbens and Wooldridge (2009: 25) to conclude that "currently, the best practice is to combine linear regression with either propensity score or matching methods". An important caveat is that although propensity score matching can be used also for small samples (Holmes and Olsen, 2010), its findings of course need to be interpreted with caution, as the small number of observations naturally limits the numbers of covariates that can be included and makes it harder to find enough good matches. To perform the propensity score matching, we have considered specifications with linear, squared term and interactive combinations of the five control variables and additional variables. Following the approach proposed by Imbens and Rubin (2015), we have found a specification using unemployment, employment rates of the second generation, right-wing vote, and French canton dummy that leads to a reduction in the differences between the treated and the untreated observations.

The computed propensity scores for Table 9 are listed below in Table A10.34 for all cantons included in the baseline regression of Table 9, column (3), and the diagnostic tests for the propensity score matching are displayed in Table A10.35. Notice first that even without propensity score matching the treated cantons (where Open job access takes a value of 1) are quite comparable to the control cantons (where Open job access equals zero), as shown by the fact that the t-test always statistically significantly rejects the null hypothesis of a difference in coefficients. Implementing propensity score matching further reduces any differences, with the means of treatment and control cantons getting closer for most variables and the overall bias being reduced by more than half. The pseudo R2 of 0.02 after matching also witnesses that the sample has become very comparable indeed. Finally, note that the results in the main text (Table 9) would be very similar under alternative matching methods (e.g. first-step logit, two nearest neighbors matching, etc. Results available upon request).

Table A10.34: Treated/Control Groups: Open Job Access

Canton	OPEN JOB ACCESS	Propensity Score	Common Support
GE	0	0.11	1
GL	0	0.47	1
ZG	1	0.48	1
SH	0	0.48	1
UR	0	0.48	1
AR	1	0.50	1
JU	0	0.53	1
SO	1	0.53	1
NE	1	0.54	1
BS	1	0.55	1
VS	0	0.58	1
TG	1	0.60	1
BL	0	0.61	1
GR	1	0.63	1
TI	1	0.67	1
ZH	0	0.67	1
SZ	1	0.70	1
FR	1	0.72	1
VD	1	0.75	1
BE	1	0.80	1
SG	1	0.81	1
LU	1	0.84	1
AG	0	0.87	1

Table A10.35: Diagnostic Tests for the Propensity Score Matching: Open Job Access

Diagnostic for each variable

		Mean		Bias		t-test		V(T)/V(C)
Variable		Treated	Control	% bias	%reduc. bias	t	p>t	
Unemployment	Unmatched	2.314	2.556	-20.4		-0.5	0.622	0.44
	Matched	2.314	2.364	-4.2	79.3	-0.17	0.865	2.64
Right-wing vote	Unmatched	18.976	16.446	27.6		0.65	0.521	0.81
	Matched	18.976	20.802	-19.9	27.8	-0.52	0.61	0.75
Second gener. empl.	Unmatched	0.011	0.026	-35.4		-0.79	0.439	2.43
	Matched	0.011	0.009	3.9	88.9	0.11	0.916	2.85
French canton	Unmatched	0.286	0.333	-9.8		-0.23	0.819	.
	Matched	0.286	0.214	14.7	-50	0.42	0.676	.

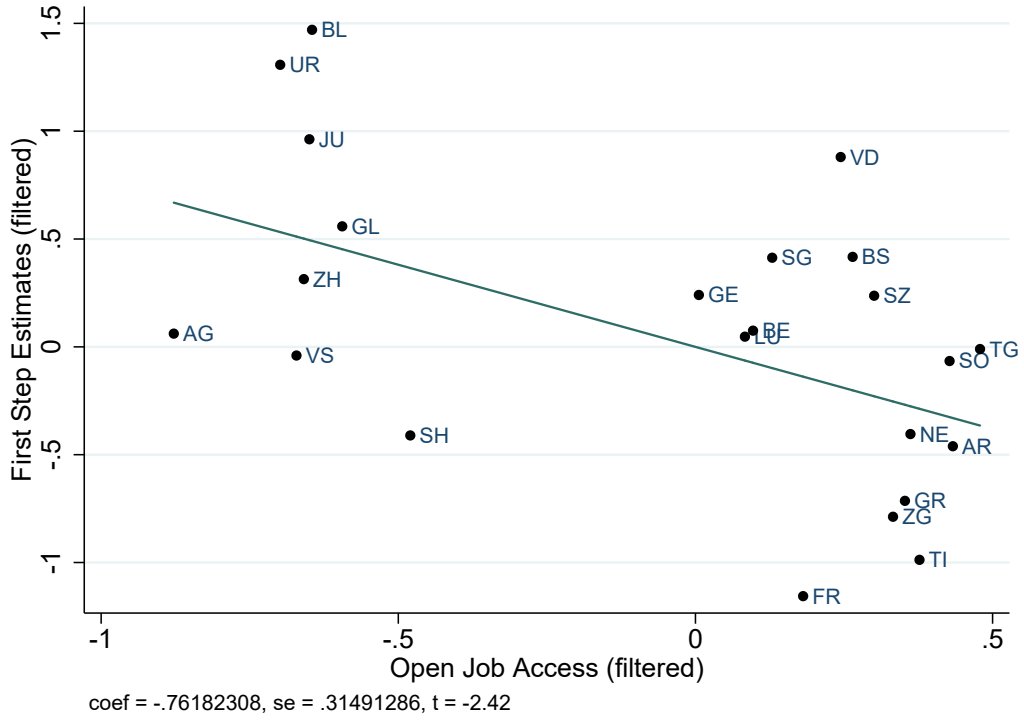
Notes: Variance ratio never outside [0.32; 3.12] for U and [0.32; 3.12] for M.

Overall diagnostic

	Pseudo R2	p>chi2	MeanBias	MedBias
Unmatched	0.09	0.597	23.3	24
Matched	0.02	0.943	10.7	9.5

Outliers Analysis: Open Job Access– In this subsection, we perform an outlier analysis for Table 9. In Figure A10.10 we display graphically the filtered first stage coefficients of KID [1-12] and the filtered values of OPEN JOB ACCESS, when controlling for the five control variables listed above (GDP per capita, unemployment rate, second generation employment rates, right-wing vote, French speaking canton). Visual inspection suggests that Thurgau (TG) and Basel-Stadt (BS) may be the biggest outliers. When dropping them from the sample, the coefficient is of very similar magnitude and statistical significance at the 1 percent level, as discussed below.

Figure A10.10: Estimated Coefficients of KID [1-12] Across Cantons with Respect to Open Job Access



In Table A10.36 we assess the robustness of the findings of Table 9 to dropping outliers and to alternative coding choices. In the column (1) we drop all cantons where the estimated residuals are beyond 1.5 standard deviations from the zero mean.[19] Further, in column (2) we code open job access as 0 instead of 1 in the canton Appenzell Ausserrhoden (AR) since the information for this canton is less clear cut than for the others. In columns (3) and (4) we code open job access as 0 resp. 1 for the canton Appenzell-Innerhoden (AI). As mentioned above, this canton is dropped in the baseline as its policy changes over time.

To provide a graphical illustration, in Figure A10.11 we drop each canton one by one, showing that in general the coefficient is stable and remains statistically significant.

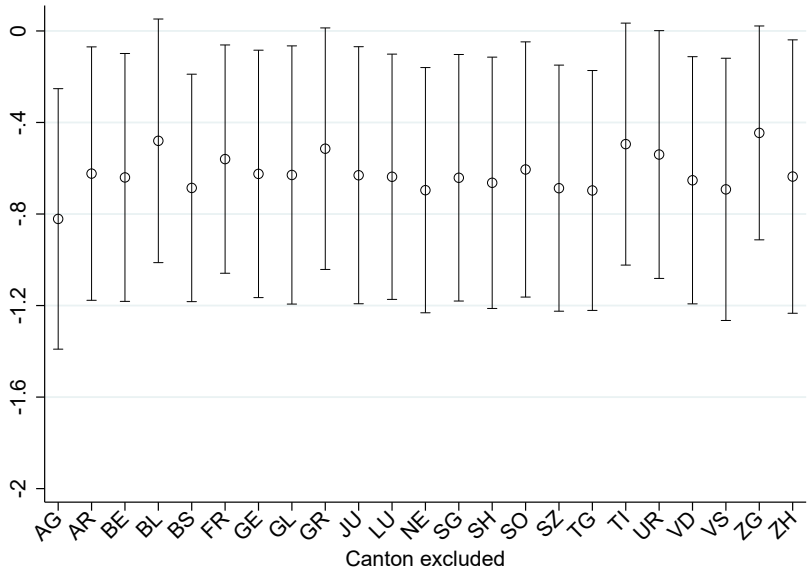
[19]Note that if we were to use a threshold of 2 standard deviations, we would end up with the same sample as in the baseline, i.e. no cantons would be dropped. In contrast, a threshold of 1 standard deviation would drop a third of cantons, leaving us with only 15 observations.

Table A10.36: Robustness Checks: Open Job Access

	(1)	(2)	(3)	(4)
Dependent Variable		$\hat{\beta}_c$		
Specification	+/- 1.5 SD	AR = 0	AI = 0	AI = 1
Dropped cantons	AI, TG, BS	AI	–	–
Open job access	-0.740	-0.582	-0.658	-0.576
	(0.305)	(0.327)	(0.308)	(0.321)
Number of cantons	21	23	24	24
R-squared	0.668	0.584	0.604	0.578

Note: GLS estimations at the canton level for the regression equation 5. The dependent variable consists of the estimated coefficients at the canton level from the regression equation 4. Open Job Access is a binary variable equal to 1 for cantons where asylum seekers can start working in all sectors of activity after 3 months upon arrival or it is equal to 0 in cantons where the cantonal administration (i) extends the working ban beyond three months or (ii) restricts work by asylum seekers to certain sectors of activity or (iii) actively discourages their access to jobs. Cantons NW and OW are also not included due to missing values in the right wing voting control (see discussion in the text). Standard errors in parentheses.

Figure A10.11: The Impact of Open Job Access When Dropping Cantons One by One



Note: Error bounds correspond to 10 percent confidence intervals.

Alternative Measure for Labor Market Integration: Occupation Rate – So far we have considered 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 of asylum seekers. We follow the same two-step approach as before, but now in the second step regress the canton-specific beta coefficients on the average occupation rate over the period, labelled Occupation rate. The latter is defined as the number of employed asylum seekers divided by the total number of asylum seekers. This continuous measure ranges from below 1 percent for Appenzell-Innerhoden to slighly above 20 percent for Graubuenden.

In column (1) of Table A10.37 we start with the basic OLS regression without controls, whereas in column (2) we add our standard set of canton controls (GDP per capita, unemployment rate, right-wing vote, language and labor market outcomes of second generation migrants). In both columns occupation rate has a negative sign, and while not significant in column (1), it becomes significant in column (2). In column (3) we instrument the current occupation rate using pre-sample occupation rates, to address the concern that there could be reversed causation of crime affecting employment. These results are however to be taken with a grain of salt as the Kleibergen-Paap F statistic is 2.5, revealing a severe weak instrument problem.

Another way to tackle potential worries about reversed causation is to move to the Panel (cohort × year level), exploiting variation over time and lagging the independent variable, as well as including canton and year fixed effects. To be able to do this, we now estimate the coefficient of Kid [1-12] separately for each canton-year, hence resulting in a Panel of 26 cantons times 8 years. Then in the second step we regress these beta coefficients on the lagged annual average occupation rate for asylum seekers, Occupation rate$_{c,t-1}$. For example for the canton of Zurich in 2012 this variable corresponds to the average occupation rate of asylum seekers in the canton of Zurich in 2011. Column (4)of Table A10.37 displays the results. The coefficient of interest has a negative sign but slightly misses statistical significance. In column (5) we replicate the regression of column (4) but including the two variables of our standard set of controls for which we have annual data (i.e. gdp per capita and unemployment rates).

Table A10.37: The Impact of Occupation Rate

	(1)	(2)	(3)	(4)	(5)
Dependent variable		$\hat{\beta}_c$		$\hat{\beta}_{c,t}$	
	OLS	OLS	IV	OLS	OLS
Sample	Cross-section	Cross-section	Cross-section	Panel	Panel
Occupation Rate	-2.099	-5.186	-2.191	-8.212	-7.873
	(5.548)	(2.577)	(7.255)	(5.036)	(5.575)
Canton FE	No	No	No	Yes	Yes
Controls	No	Yes	Yes	No	Yes
Observations	26	24	24	182	156
R-squared	0.357	0.595	0.225	0.196	0.212

Note: Estimations at the canton level for the regression equation 5. GLS estimations in columns (1) to (2) and (4) to (5) and IV estimations in column (3), using as instrument the pre-sample occupation rates. The dependent variable consists of the estimated coefficients at the canton level from the regression equation 4. Occupation rate corresponds to the number of employed asylum seekers divided by the total number of asylum seekers. Robust standard errors in parentheses.

Alternative Labor Market Integration Policy Measure: Active Labor Market Measures – It is useful to also consider alternative measures of labor market integration. While our measure Open job access captures well labor market access, it is interesting to also assess whether on top of this active labor market policy makes a difference. To put it differently, while our main measure captures opportunities (i.e. the ability to seek work), alternative variables of active labor market policies may pick up nudging (i.e. active labor market intervention).

Alternative data has been collected by Wichmann et al. (2011), capturing up to what extent cantons actively put in place promotion measures for fostering integration in the job market, as well as professional training, such as coaching and internships. In particular, we code a dummy variable called Active labor market measures, taking a value of 1 if a given canton has put in place at least one of the following: public or private occupation programs, integration conventions, promotion of labor market or professional integration measures (*source:* Wichmann et al. (2011), survey on "Migration and Federalism", questions 17 and 18).

This data has two downsides with respect to our preferred measure Open job access used in the main text. While our main measure of labor market access is based on an official document of the highest cantonal authority, the Wichmann et al. (2011) variable Active labor market measures draws on an informal survey among an arguably more heterogeneous pool of civil servants. Further, while our preferred Open job access variable covers all 26 cantons, the Wichmann et al. (2011) measure is confined to only 22. Finally, while we have measures of our variable Open job access both in 2012 and 2017 (confirming persistence), the Wichmann et al. (2011) survey is only conducted at one point in time, in 2011. For these reasons we have focused in the main text on Open job access and relegated the results based on the Wichmann et al. (2011) data to the appendix.

The results with the Wichmann et al. (2011) variable Active labor market measures are displayed in Table A10.38 which follows the structure and specifications of Table 10 described in the main text in section 7. It is found that the coefficient of interest is in almost all columns of the expected negative sign, but it is never statistically significant.

Table A10.38: The Impact of Active Labor Market Measures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable				$\hat{\beta}_c$			
Sample	2009-2012	2009-2012	2009-2012	2009-2012	2009-2012	2009-2012	2009-2016
Specification	No controls	Controls	Trimming	PSM RHS	Inv. Prob. W.	PSM	PSM
Active Labor Market Measures	-0.427	-0.362	0.150	-0.319	-0.119	-0.094	-0.186
	(0.648)	(0.724)	(0.650)	(0.600)	(0.519)	(0.487)	(0.347)
Number of cantons	22	21	18	21	21	21	21
R-squared	0.210	0.411	0.611	0.314	0.331	N/A	N/A

Note: Estimations at the canton level for the regression equation (5). GLS estimations in columns (1) to (4), OLS estimation in column (5), and propensity score matching estimation in columns (6) to (7). The dependent variable consists of the estimated coefficients at the canton level from the regression equation (4). Active Labor Market Measures is a binary variable taking the value of 1 if a given canton has put in place at least one of the following: public or private occupation programs, integration conventions, promotion of labor market or professional integration measures (*Source:* Wichmann et al. (2011), survey on "Migration and Federalism", questions 17 and 18). Standard errors in parentheses.

Impact of Open Job Access on Non-exposed Individuals – In the manuscript we stress that the policy regressions focus only on how canton-level policies affect the *relative* crime propensity of conflict-exposed individuals compared to non-exposed ones (the absolute level of criminality being absorbed by the canton × year fixed effects in the first-step equation 4), as our analysis is not aimed at estimating how policies reduce crime rates of all asylum seekers. Addressing this question raises empirical challenges that go beyond the scope of this paper. In particular, while in the policy analysis carried out in the paper, comparing individuals from the same nationality but with different conflict exposure during childhood, we are able to filter out many potential confounders with the canton-year fixed effects, studying overall crime differences between cantons precludes the inclusion of canton-year fixed effects (as now the variation used is at this level), making it hence difficult to convincingly rule out omitted variable bias.

This being said, below we shall still present some very tentative correlations (that need to be interpreted with much caution), speaking to the question of whether policies may reduce crime rates of all asylum seekers. To this purpose we directly estimate equation 5 with the canton-specific average crime propensity as dependent variable, replicating all the specifications of our baseline analysis of the impact of OPEN JOB ACCESS (see Table 9) with this new variable. The estimation results are reported in Table A10.39 below for all nationalities and in Table A10.40 for the subsample of nationalities with a crime propensity above the median. There is a trade-off, as restricting the sample to the nationalities with the highest crime propensity allows to focus on a sample for which effects are potentially most detectable, while restricting the sample too much would result in a loss of too much information. Hence, restricting the sample to the above-median nationalities may be a good compromise.

The structure of those two tables is the same as for our baseline analysis of the impact of OPEN JOB ACCESS on the effect of childhood exposure in Section 7 (see the detailed description in this section). We detect in neither of the two Tables A10.39 and A10.40 any statistical effect of our policy variable OPEN JOB ACCESS on canton-level criminality. Note that we find a similar non-result for the other key policy variable CIVIC COURSES (not displayed, available upon request). This contrasted finding suggests that policies are effective only for those who were conflict-exposed during childhood but not for all asylum seekers. One reason for this finding could be that the overall crime propensity is substantially smaller for the asylum seekers who were not conflict-exposed, which means that the potential for any policy to curb crime is (mechanically) lower (intuitively, in the hypothetical extreme case of a group having zero crime propensity, by definition the scope for further crime reduction would be removed).

Note however that we need to be extremely cautious with the interpretation of this non-result because the impossibility of filtering out for canton × year fixed effects in the first-step equation 4 makes the dependent variable very noisy and raises concerns on omitted variable bias in the estimation procedure of equation 5.

Table A10.39: Impact of Open Job Access - Absolute Level of Crime Propensity

Dependent Variable	$\hat{\beta}_c$					
	(1)	(2)	(3)	(4)	(5)	(6)
Sample	2011-2016	2011-2016	2009-2016	2009-2016	2009-2016	2009-2016
Specification	No controls	Controls	Controls	PSM RHS	Inv. Prob. W.	PSM
Open Job Access	0.023	0.510	0.343	0.498	0.067	0.317
	(0.508)	(0.443)	(0.441)	(0.556)	(0.791)	(0.531)
Number of cantons	25	23	23	23	23	23
R-squared	0.000	0.714	0.731	0.448	0.355	-

Note: Estimations at the canton level for the regression equation (5). GLS estimations in columns (1) to (4), OLS estimation in column (5), and propensity score matching estimation in column (6). The dependent variable consists of the canton-specific average violent crime propensity. OPEN JOB ACCESS is a binary variable equal to 1 for cantons where asylum seekers can start working in all sectors of activity after 3 months upon arrival or it is equal to 0 in cantons where the cantonal administration (i) extends the working ban beyond three months or (ii) restricts work by asylum seekers to certain sectors of activity or (iii) actively discourages their access to jobs. Standard errors in parentheses.

Table A10.40: Impact of Open Job Access - Absolute Level of Crime Propensity - Most Violent Nationalities

Dependent Variable	$\hat{\beta}_c$					
	(1)	(2)	(3)	(4)	(5)	(6)
Sample	2011-2016	2011-2016	2009-2016	2009-2016	2009-2016	2009-2016
Specification	No controls	Controls	Controls	PSM RHS	Inv. Prob. W.	PSM
Open Job Access	-0.581	1.180	0.768	0.897	0.296	0.738
	(1.067)	(0.744)	(0.725)	(0.999)	(1.401)	(0.972)
Number of cantons	25	23	23	23	23	23
R-squared	0.013	0.812	0.818	0.547	0.426	-

Note: Estimations at the canton level for the regression equation (5). GLS estimations in columns (1) to (4), OLS estimation in column (5), and propensity score matching estimation in column (6). The dependent variable consists of the canton-specific average violent crime propensity. OPEN JOB ACCESS is a binary variable equal to 1 for cantons where asylum seekers can start working in all sectors of activity after 3 months upon arrival or it is equal to 0 in cantons where the cantonal administration (i) extends the working ban beyond three months or (ii) restricts work by asylum seekers to certain sectors of activity or (iii) actively discourages their access to jobs. Standard errors in parentheses.

A10.4 Additional Results on Social Integration

Propensity Score Matching: Civic Courses– The computed propensity scores for Table 10 are listed for all cantons in Table A10.41 and the diagnostic tests for the propensity score matching are displayed in Table A10.42.

Table A10.41: Treated/Control Groups: Civic Courses

Canton	Civic courses	Propensity Score	Common Support
LU	1	0.23	0
SG	0	0.25	1
UR	0	0.26	1
AG	0	0.26	1
AI	0	0.28	1
OW	0	0.29	1
SZ	1	0.31	1
GL	1	0.32	1
BL	0	0.33	1
AR	0	0.33	1
TI	0	0.34	1
SO	1	0.34	1
BS	0	0.35	1
ZG	1	0.36	1
ZH	0	0.36	1
SH	0	0.40	1
BE	0	0.59	1
FR	1	0.59	1
VD	1	0.65	1
VS	1	0.65	1
JU	0	0.66	1
GE	1	0.89	0

Table A10.42: Diagnostic Tests for the Propensity Score Matching: Civic Courses

Diagnostic for each variable

		Mean		Bias		t-test		V(T)/V(C)
Variable		Treated	Control	% bias	%reduc. bias	t	p>t	
Unemployment	Unmatched	2.656	2.077	48.1		1.15	0.262	2.23
	Matched	2.314	2.386	-5.9	87.7	-0.16	0.879	1.6
Second gener. empl.	Unmatched	0.018	0.015	5		0.11	0.912	0.63
	Matched	0.023	0.020	7.7	-55.1	0.2	0.841	1.79
French canton	Unmatched	0.444	0.154	63.5		1.51	0.145	.
	Matched	0.429	0.429	0	100	0	1	.

Notes: Variance ratio never outside [0.23; 4.43] for U and [0.17; 5.82] for M.

Overall diagnostic

	Ps R2	p>chi2	MeanBias	MedBias
Unmatched	0.091	0.438	38.9	48.1
Matched	0.003	0.997	4.5	5.9

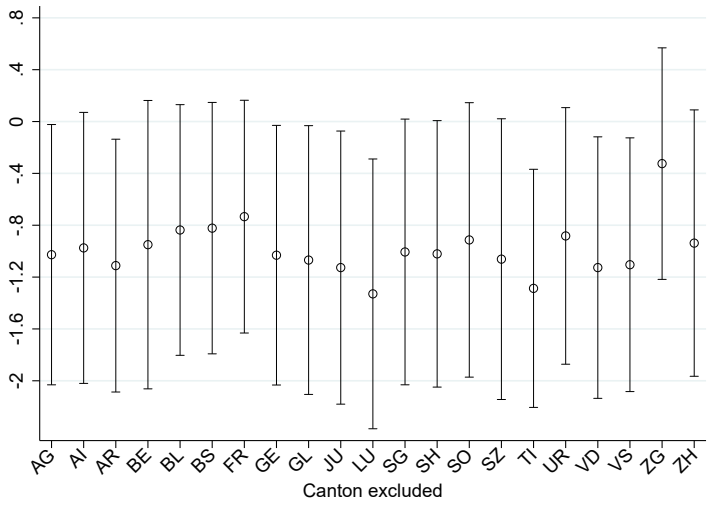
Outliers Analysis: Civic Courses– Below we shall assess in Table A10.43 the robustness of the findings of Table 10 to dropping outliers. First we perform an outlier robustness analysis for column (2) of Table 10. It turns out that using a cut-off of 2 standard deviations results in the same sample as in column (2) of Table 10. In contrast, a 1.5 standard deviations threshold leads to a massive drop in cantons, removing namely Appenzell-Ausserrhoden (AR), Basel-Stadt (BS), Fribourg (FR) and Uri (UR). This is displayed in column (1) of Table A10.43. While the coefficient of interest still has a negative sign, it loses statistical significance. Moving on to assessing outlier robustness of column (3) of Table 10, it turns out that when applying the 2 standard deviations removal cut-off, the sample again remains unchanged with respect to column (3) of Table 10. With a 1.5 standard deviations threshold two cantons get dropped, Basel-Stadt (BS) and Appenzell-Ausserrhoden (AR). As shown in column (2) of Table A10.43, this outlier removal has almost no impact on the results, with the coefficient of interest remaining of similar magnitude and statistically significant. To provide a graphical illustration, in Figure A10.12 we drop each canton one by one, showing that in general the coefficient is stable.

Table A10.43: Robustness Checks: Civic Courses

	(1)	(2)
Dependent Variable	$\hat{\beta}_c$	
Trimming	No (Col. 2 Main Table)	Yes (Col. 3 Main Table)
Specification	+/- 1.5 SD	+/- 1.5 SD
Dropped cantons	AR, BS, FR, UR	AR, BS
Civic Courses	-0.517	-1.307
	(0.490)	(0.625)
Number of cantons	17	17
R-squared	0.762	0.659

Note: GLS estimations at the canton level for the regression equation 5. The dependent variable consists of the estimated coefficients at the canton level from the regression equation 4. Civic courses is a binary variable, taking a value of 1 if a canton offers courses on general knowledge about Swiss history, social norms and civic culture targeted at asylum seekers, and zero otherwise. The cantons which are dropped in each column are additional to the corresponding specification. Standard errors in parentheses.

Figure A10.12: The Impact of Civic Courses When Dropping Cantons One by One



Note: Error bounds correspond to 10 percent confidence intervals.

Alternative Social Integration Measure: Language Courses– A further potentially important factor for social integration may be the promotion of language skills. The rationale is that knowing well the language of the host country may be an important factor affecting the social and economic integration. The variable LANGUAGE COURSES takes a value of 1 if basic or advanced language courses are offered, and 0 otherwise (*Source:* Wichmann et al., (2011), Survey on "Migration and Federalism", question 17. Language courses are considered both "Cours de langue pour usage quotidien" and "Cours de langues de niveau approfondi").

The results are displayed in Table A10.44 which follows the structure and specifications of Table 10 described in the main text. It is found that the coefficient of interest is of volatile sign and never statistically significant.

Table A10.44: The Impact of Language Courses

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable				$\hat{\beta}_c$			
Sample	2009-2012	2009-2012	2009-2012	2009-2012	2009-2012	2009-2012	2009-2016
Specification	No controls	Controls	Trimming	PSM RHS	Inv. Prob. W.	PSM	PSM
LANGUAGE COURSES	-0.403	-0.480	-0.391	-0.236	0.254	0.434	0.235
	(0.672)	(0.809)	(1.033)	(0.734)	(0.554)	(0.873)	(0.523)
Number of cantons	22	21	15	21	21	21	21
R-squared	0.207	0.415	0.503	0.225	0.220	N/A	N/A

Note: Estimations at the canton level for the regression equation (5). GLS estimations in columns (1) to (4), OLS estimation in column (5), and propensity score matching estimation in columns (6) to (7). The dependent variable consists of the estimated coefficients at the canton level from the regression equation (4). The variable LANGUAGE COURSES takes a value of 1 if basic or advanced language courses are offered, and 0 otherwise (*Source:* Wichmann et al., (2011), Survey on "Migration and Federalism", question 17. Language courses are considered both "Cours de langue pour usage quotidien" and "Cours de langues de niveau approfondi"). Standard errors in parentheses.

Alternative Social Integration Measure: Private Management of Asylum Centers– We shall now study how the method of management of asylum centers and the level of funding affect crime incentives. The variable PRIVATE MANAGEMENT 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 (*source:* www.abs-ag.ch, www.ors.ch and private communication with canton officials).[20] One reason for cantons to outsource asylum services to private firms is to save on costs. In the Swiss media there has been a considerable controversy on whether quality standards are guaranteed when private for-profit companies run asylum structures.[21]

The results are displayed in Table A10.45 which follows the structure and specifications of Table 10 described in the main text. It is found that the coefficient of interest is of volatile sign and never statistically significant.

Table A10.45: The Impact of Private Management

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable				$\hat{\beta}_c$			
Sample	2009-2012	2009-2012	2009-2012	2009-2012	2009-2012	2009-2012	2009-2016
Specification	No controls	Controls	Trimming	PSM RHS	Inv. Prob. W.	PSM	PSM
PRIVATE MANAGEMENT	0.280	-0.003	-0.131	0.009	-0.433	-0.520	-0.145
	(0.582)	(0.668)	(0.736)	(0.660)	(0.521)	(0.440)	(0.248)
Number of cantons	26	24	22	24	24	24	24
R-squared	0.174	0.410	0.419	0.219	0.227	N/A	N/A

Note: Estimations at the canton level for the regression equation (5). GLS estimations in columns (1) to (4), OLS estimation in column (5), and propensity score matching estimation in columns (6) to (7). The dependent variable consists of the estimated coefficients at the canton level from the regression equation (4). PRIVATE MANAGEMENT is a dummy variable taking the 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 (*source:* www.abs-ag.ch, www.ors.ch and private communication with canton officials). Standard errors in parentheses.

[20]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.

[21]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`.

Alternative Social Integration Measure: Financial Social Assistance– The final aspect that we study and which could affect social integration is the extent of social assistance. The variable FINANCIAL SOCIAL 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. 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 would expect more financial assistance and better funded accommodation structures to deter crime.

The results are displayed in Table A10.46 which follows the structure and specifications of Table A10.37 described above. The coefficient of interest is never statistically significant.

Table A10.46: The Impact of Financial Social Assistance

	(1)	(2)	(3)	(4)	(5)
Dependent Variable		$\hat{\beta}_c$		$\hat{\beta}_{c,t}$	
	OLS	OLS	IV	OLS	OLS
Sample	Cross-section	Cross-section	Cross-section	Panel	Panel
FINANCIAL SOCIAL ASSISTANCE	0.402	2.160	2.308	1.765	1.854
	(0.933)	(1.689)	(2.073)	(1.833)	(2.144)
Canton FE	No	No	No	Yes	Yes
Controls	No	Yes	Yes	No	Yes
Number of cantons	26	24	24	182	156
R-squared	0.353	0.588	0.235	0.190	0.208

Note: Estimations at the canton level for the regression equation 5. GLS estimations in columns (1) to (2) and (3) to (4) and IV estimations in column (3), using as instrument the pre-sample financial social assistance. The dependent variable consists of the estimated coefficient at the canton level from the regression equation 4. FINANCIAL SOCIAL ASSISTANCE 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)). Robust standard errors in parentheses.

A10.5 Controlling for Other Policies

The cross-canton correlation between Open job access and Civic courses is quite low, around 0.16, showing that economic and social integration policies are not systematically implemented by the same cantons. The joint estimation of the effect of Open job access and Civic courses is statistically challenging for two reasons. Firstly, due to data limitation since the former variable is accurately measured over the period 2011-2016, while the latter one is defined over 2009-2012. Secondly, the implementation of PSM methods with multiple treatments is constrained by the small sample size. Nevertheless, in what follows we replicate each of the Tables 9 and 10 with the other policy variable included as a control (either in the OLS specifications, either in the propensity score model). This is a demanding exercise since the sample size is reduced with respect to the results from the main text where policies are independently estimated. However, the point estimates remain quite stable.

Table A10.47: The Impact of Open Job Access Controlling for Civic Courses

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable				$\hat{\beta}_c$		
Sample	2011-2016	2011-2016	2009-2016	2009-2016	2009-2016	2009-2016
Specification	No controls	Controls	Controls	PSM RHS	Inv. Prob. W.	PSM
Open Job Access	-0.640	-0.645	-0.580	-0.549	-0.547	-0.566
	(0.366)	(0.404)	(0.370)	(0.364)	(0.284)	(0.322)
Number of cantons	25	20	20	20	18	20
R-squared	0.246	0.540	0.644	0.485	0.527	-

Note: Estimations at the canton level for the regression equation 5. GLS estimations in columns (1) to (4), OLS estimation in column (5), and propensity score matching estimation in column (6). The dependent variable consists of the estimated coefficients at the canton level from the regression equation 4. Open Job Access is a binary variable equal to 1 for cantons where asylum seekers can start working in all sectors of activity after 3 months upon arrival or it is equal to 0 in cantons where the cantonal administration (i) extends the working ban beyond three months or (ii) restricts work by asylum seekers to certain sectors of activity or (iii) actively discourages their access to jobs. In columns (2) to (6) we control for Civic courses. Standard errors in parentheses.

Table A10.48: The Impact of Civic Courses Controlling for Open Job Access

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable				$\hat{\beta}_c$			
Sample	2009-2012	2009-2012	2009-2012	2009-2012	2009-2012	2009-2012	2009-2016
Specification	No controls	Controls	Trimming	PSM RHS	Inv. Prob. W.	PSM	PSM
Civic courses	-1.083	-0.565	-0.616	-0.855	-0.669	-0.479	-0.390
	(0.555)	(0.638)	(0.654)	(0.655)	(0.548)	(0.561)	(0.221)
Number of cantons	22	20	19	21	21	21	21
R-squared	0.322	0.591	0.610	0.327	0.358	N/A	N/A

Note: Estimations at the canton level for the regression equation 5. GLS estimations in columns (1) to (4), OLS estimation in column (5), and propensity score matching estimation in columns (6) to (7). The dependent variable consists in the estimated coefficients at the canton level from the regression equation 4. Civic courses is a binary variable, taking a value of 1 if a canton offers courses on general knowledge about Swiss history, social norms and civic culture targeted at asylum seekers, and zero otherwise. In columns (2) to (6) we control for Open Job Access. Standard errors in parentheses.

A10.6 Stringency of Asylum Policy

Another key aspect of integration policies relates to the outcome of the demand for political asylum: Presumably asylum seekers with a high chance of obtaining political asylum have higher incentives to abide by the law in order to maximize their asylum chances as compared to asylum seekers whose asylum demand has no chance. In Switzerland asylum policy is set at the federal level, not the cantonal one. This leads us to adjust our empirical design by grounding the identification on cross-nationality variations.

We consider two policy variables related to the stringency of the asylum procedure. Firstly, ACCEPTANCE RATE$_n$ corresponds to the pre-sample percentage of recognized refugees (residence B permit) among asylum seeker demands, by nationality n.[22] There is substantial cross-nationality heterogeneity in acceptance rates, as the administrative decision to grant asylum foremost depends on the political situation in the country of origin (summary statistics are displayed in Table A10.31). Secondly, 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 she is from a country with a readmission agreement, Switzerland is able to send her back to her country of origin by force, while people from countries without readmission agreement cannot be expelled by force even if their asylum application has been rejected. Hence, our variable, READMISSION$_n$ takes the value of 1 if there exists a bilateral readmission agreement between Switzerland and the country of origin n, and zero otherwise.

Equipped with those nationality-specific policy variables, we implement a two step procedure analogous to the one described above for cantonal policies (see equation 4), but now computing distinct coefficients of KID [1-12] separately for each nationality of origin. The specification of the first step is as follows:

$$\text{CP}_{n,a,t} = \beta_n \times \text{KID [1-12]}_{n,a,t} + \sum_{k=13}^{k=80+} \gamma(k) \times \text{EXPO}(k)_{n,a,t} + \mathbf{FE}_{n,t} + \mathbf{FE}_a + \varepsilon_{n,a,t}, \tag{A10.6}$$

The previous equation is estimated on the baseline sample of adult males (column 2, Table 5, Panel A). We end up with a set of 66 point estimates of $\hat{\beta_n}$ as several nationality-specific coefficients cannot be estimated due to perfect collinearity with one of the fixed effects (e.g. nationalities where KID [1-12] equals always 0 or always 1). The second-step regression is then given by:

$$\hat{\beta}_n = \alpha \times \text{Acceptance}_n + \gamma \times \text{Readmission}_n + \delta \times \text{Acceptance}_n \times \text{Readmission}_n + \mathbf{X}_n'\beta + \varepsilon_n, \tag{A10.7}$$

where $\mathbf{X}_n$ is a vector of nationality-level control variables that include pre-sample GDP per capita, pre-sample population, ethnic fractionalization and pre-sample Polity IV democracy scores.[23]

Table A10.49 displays the estimation results of equation A10.7. Control variables, included in columns (2), (4) and (6), serve the purpose of reducing unobserved heterogeneity but lead to a drop of observations due to missing information. In the first two columns we find that a higher acceptance rate goes along with lower crime propensity. This is consistent with the notion that a larger likelihood of being able to remain in Switzerland in the medium-run fosters the incentives to abide by the law. 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. In the

[22] Note that the use of the (more exogenous) 2008 pre-sample measure of ACCEPTANCE RATE$_n$ results in a drop of four countries (namely Djibouti, Equatorial Guinea, Tajikistan and Venezuela) due to missing information.

[23] The distribution of β_n is reported graphically in Figure A10.13.

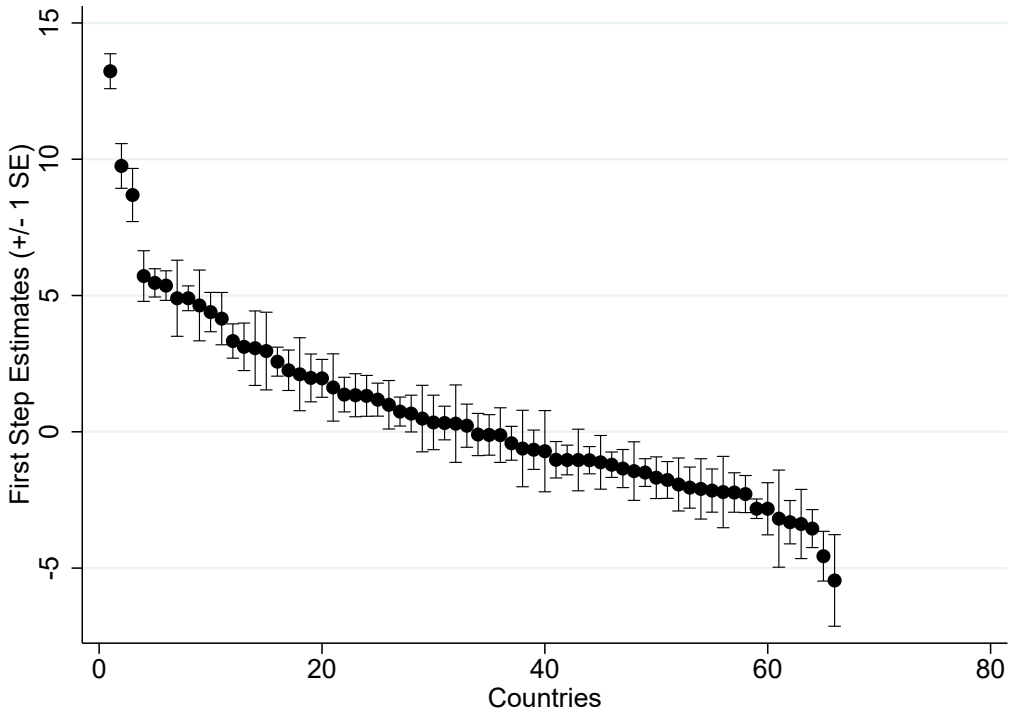
columns (3) and (4) we focus on the overall effect and find that when there exists a readmission treaty for a given country the crime propensity of those exposed to conflict during childhood is higher. To further understand the underlying mechanism at work, it is possible to disentangle the two countervailing forces. For someone with very low chances of being granted refugee status, a readmission agreement shortens the time horizon in Switzerland and hence reduces the incentives for cooperation. 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 a 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. To check this, we interact the acceptance rate and readmission agreement variables. As shown in columns (5) and (6), the coefficient of this interaction 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 incentives when acceptance chances are high, exactly as predicted by the Beckerian logic outlined above.

Table A10.49: The Impact of the Acceptance Rate and Readmission Treaties

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable				$\hat{\beta}_n$		
ACCEPTANCE RATE	-4.488	-7.073			0.042	-1.606
	(2.533)	(2.366)			(2.013)	(1.848)
READMISSION			4.253	4.916	7.331	7.297
			(1.289)	(1.870)	(1.642)	(2.176)
ACCEPTANCE RATE × READMISSION					-39.823	-40.527
					(14.856)	(19.012)
Country controls	No	Yes	No	Yes	No	Yes
Number of countries	62	53	66	56	62	53
R-squared	0.089	0.244	0.295	0.348	0.434	0.485

Note: OLS estimations at the nationality level for the regression equation (A10.7). The dependent variable consists of the estimated coefficients at the canton level from the regression equation (A10.6). ACCEPTANCE RATE corresponds to the percentage of recognized refugees (residence B permit) among asylum seeker demands, by nationality n in 2008. READMISSION is a binary variable which takes the value of 1 if there exists a bilateral readmission agreement between Switzerland and the country of origin n, and zero otherwise. Country controls include GDP per capita (PPP, Constant International USD in 2011) and population in 2008 (both from the World Development Indicators - World Bank, 2017), ethnic fractionalization (from Fearon, 2003) and Polity IV democracy scores in 2008 ("Revised Combined Polity Score" from Polity IV, 2017). Robust standard errors in parentheses.

Figure A10.13: The Estimated Coefficients of Kid [1-12] Across Countries



Note: The 66 points correspond to the $\hat{\beta}_c$ coefficients of the first step estimation for the 66 countries (estimated on the sample of adult male asylum seekers for the 2009-2016 period), ordered by magnitude. The vertical lines represent +/- 1 standard error.

Additional References

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