American Economic Review 2019, 109(12): 4378–4425
https://doi.org/10.1257/aer.20170263

The Violent Legacy of Conflict: Evidence on Asylum Seekers, Crime, and Public Policy in Switzerland[†]

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We study empirically how past exposure to conflict in origin countries makes migrants more violence-prone in their host country, focusing on asylum seekers in Switzerland. We exploit a novel and unique dataset on all crimes reported in Switzerland by the nationalities of perpetrators and of victims over 2009–2016. Our baseline result is that cohorts exposed to civil conflict/mass killing during childhood are 35 percent more prone to violent crime than the average cohort. This effect is particularly strong for early childhood exposure and is mostly confined to co-nationals, consistent with inter-group hostility persisting over time. We exploit cross-region heterogeneity in public policies within Switzerland to

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[†]Go to https://doi.org/10.1257/aer.20170263 to visit the article page for additional materials and author disclosure statements.

document which integration policies are best able to mitigate the detrimental effect of past conflict exposure on violent criminality. We find that offering labor market access to asylum seekers eliminates two-thirds of the effect. (*JEL* D74, F22, K42, Z18)

Political violence is often persistent and wars tend to recur.[1] Moreover, there is much anecdotal evidence that exposure to conflict makes individuals more violence-prone.[2] Various mechanisms explain why people tend to reproduce violence when they are haunted by having either perpetrated or witnessed violence in the past: psychological trauma, a collapse of trust and moral values, economic deprivation, to name a few. Yet beyond case studies and anecdotes, the identification of a causal impact of past exposure to conflict on future proneness to violence and unlawful behavior has proved challenging. This is primarily due to the fact that in most cases, people remain in the same environment that caused war to break out in the first place, making it hard to isolate the individual effects of war exposure from the impact of their surroundings (e.g., weak institutions, natural resource abundance, or ethnic cleavages). This lack of systematic large-scale evidence is worrying, as the persistence of violence and crime, and the vicious cycles leading to the recurrence of war are key issues in development economics, and are of foremost importance for post-conflict reconstruction.[3]

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

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

[2] In the popular press, there are numerous well-publicized cases of people with war experience who after leaving the battlefield are convicted of serious crimes. The amplitude of the phenomenon is such that even the prestigious daily *The New York Times* put together the series "War Torn," (2008) which includes "a series of articles and multimedia about veterans of the wars in Iraq and Afghanistan who have committed killings, or been charged with them, after coming home." The series is available at: https://archive.nytimes.com/query.nytimes.com/gst/fullpage-9F0CE0D91E31F93BA35754C0A96E9C8B63.html.

[3] The relative scarcity of studies on violence reproduction using regional/country-level variation contrasts with the existence of a number of micro-level studies using individual-level data and exploiting differences in individual- or village-level conflict exposure, such as, e.g., Bellows and Miguel (2009); Blattman (2009); Voors et al. (2012); Gilligan, Pasquale, and Samii (2014); Bauer et al. (2014).

Several institutional features make Switzerland an ideal laboratory to tackle these methodological issues. In particular, we make use of an original and exhaustive dataset on violent crimes in Switzerland over the 2009–2016 period that has the crucial feature of documenting the nationalities of perpetrators. We combine this information with a new and fine-grained dataset on all asylum seekers living in Switzerland during the same period and estimate a crime regression at the cohort level. Controlling for unobserved heterogeneity thanks to a battery of fixed effects (i.e., age, nationality × year), our main source of identification corresponds to variations in crime-propensities across cohorts of the same nationality and migration wave, with differing exposure to civil conflicts and mass killings (i.e., born before/after conflict). We also exploit the fact that asylum seekers are exogenously assigned to (and required to reside in) one of the 26 Swiss administrative regions (i.e., *cantons*) following a distribution key that allocates quotas based on canton population size only and not on migrant characteristics. While this does not prevent asylum seekers from spending more time in particular areas within cantons (which are already relatively small units), it does rule out self-selection of asylum seekers into more crime-prone cantons, as well as helps to account for potential differences between cantons in terms of policies or crime detection (the main unit for many decisions on policies and criminal policing is the canton). For the sake of interpretation, it is also important to rule out self-selection into conflict exposure. To this regard, our data allow us to isolate one group who were *not* perpetrators: cohorts who were children in wartime. While inheritable traits or norms could potentially be transmitted by perpetrators to their children, youth born after the war (the reference category) or before the end of war are likely to be affected to a similar extent by such inheritable traits and norms transmitted over generations.

Our baseline result is that adult male asylum seekers exposed to civil conflict/mass killing during childhood (below the age of 12) are 35 percent more prone to violent crime than the average cohort of asylum seekers. This effect is particularly strong for early childhood exposure; is stable through the life cycle; is present for both civil conflict and mass killing exposure; and is not detected (i) for women, (ii) for property crimes, or (iii) for conflicts of low severity. Our findings are robust to alternative estimation techniques, disaggregation levels, and coding options of exposure during childhood.

The crime effect of past conflict exposure that we detect at the cohort level encompasses, among others, direct and indirect forms of victimization at the individual level, such as being personally targeted by acts of violence (e.g., being injured or witnessing the killing of a family member), being exposed to a war context characterized by economic deprivation or social capital depletion (e.g., growing up in poverty or in a place marked by the collapse of moral values), suffering from exposure to a conflict-related erosion of state capacity and the rule of law (e.g., spending one's childhood in the absence of democratic institutions or human rights protection), or experiencing school closures as a result of conflict (e.g., accumulating less human capital during childhood and thus having worse job perspectives later in life).[4] Conflict-induced compositional effects at the population level may also drive

[4] Disentangling direct versus indirect victimization is not easy. While conflict-induced fatalities and injuries are by any standards substantial (Wagner et al. 2018), displaced populations have also often directly witnessed

part of the results (e.g., only the physically strongest or most aggressive may survive the war and migrate). Our baseline analysis does not discriminate against any of the channels above as we believe them all to be potentially important and part of the effect we are interested in here. Indeed, from the perspective of a receiver country, but also within a post-war reconstruction context, all these possible facets of the violent legacy of victimization are highly policy-relevant.

Data availability constrains our ability to disentangle these channels. In particular, information on individual and family characteristics is limited. Without data on asylum seekers' precise region of origin within their country, we must rely on a relatively coarse measure of conflict exposure at the country level. To further refine the analysis and restrict the array of potential channels, we consequently exploit information on the nationalities of both perpetrators and victims. We estimate a *bilateral crime regression* that documents the propensity of cohorts of a given nationality to target victims from specific nationalities. Crucially, this allows for the inclusion of cohort fixed effects, resulting in our inference being based purely on bilateral characteristics, an approach grounded in the gravity trade literature. With all cohort-specific unobserved heterogeneity being filtered-out, most of the channels described above are muted. The bilateral results show that past conflict exposure makes asylum seekers more crime-prone, in general, toward Swiss natives, with a violent crime premium close to that of the baseline (41 percent). More importantly, we find that this premium is almost tripled in the case of violent crimes targeting co-nationals (114 percent), showing that the increase in the propensity for violent crime among individuals exposed to conflict during (early) childhood is mostly confined to their own communities. This finding is consistent, among others, with theories of civil war recurrence, which stress persistence in the destruction of social ties and in intra-national hostility.

Finally, we exploit the fact that Switzerland is a federal state with large variations in institutions and public policies across its 26 cantons. We are particularly interested in whether there exists a set of integration policies that can mitigate the risk of increased criminality for conflict-exposed individuals. Our main finding is that fostering prospects for the labor market integration of asylum seekers can reduce the effect of conflict exposure by two-thirds. More specifically, the opportunity to apply rapidly for jobs in all sectors together with the promotion of labor market access mitigate the crime-inducing impact of conflict exposure. We also find that social integration measures such as civic education courses help to protect against the risk of conflict exposure, lessening future crime propensity. Due to the absence of a randomization scheme in the implementation of policies at the canton level, our exercise relative to policy evaluation cannot go much beyond correlations. Thus, while our findings are consistent with the local environment having a causal impact, the data do not allow us to rule out all potential confounders. While limited, this preliminary evidence is, to the best of our knowledge, new to the literature and fills an important gap by documenting how public policies can tackle the recurrence of violence in the aftermath of conflict.

violence, lost a family member, or experienced malnutrition (see, for example, de Jong et al. 2002, for Sri Lanka, or *The Guardian*, February 11, 2016, "Report on Syria Conflict Finds 11.5% of Population Killed or Injured").

An alternative way to study the role of economic and social integration is to focus on the larger sample of economic migrants in Switzerland (roughly one-fifth of the total population). As discussed below, this group has better long-run prospects and higher employment rates than asylum seekers, and we expect these features to translate into a lower impact of conflict exposure during childhood on future violent crime propensity. Our estimations on the sample of economic migrants confirm that conflict exposure during childhood increases future violent crime proneness, a reassuring result with respect to the external validity of our study. But, as expected, the magnitude of the effect is smaller in comparison to our estimates based on the sample of asylum seekers (17 percent versus 35 percent).

Taken together, our results are consistent with the notion that the destruction of social ties resulting from conflict may matter for future violence, in line with our evidence on intra-national hostility, but that in addition to societal factors, material incentives also play a major role. Indeed, both social and economic elements affect the cost-benefit calculations of prospective criminals: while lower social ties reduce the costs of attacking co-nationals, better economic options raise them again. Both our suggestive evidence on policies and the results relative to economic migrants indicate that even when conflict has torn apart social ties, economic prospects and job opportunities can foster resilience.

Besides its academic interest, the question of what factors might make immigrants more crime-prone is of great societal importance. In many developed Western countries, this topic has fueled heated and politically-loaded debates, triggering the rise of populist parties. To this regard, one policy-relevant conclusion of our paper is that the crime risk of asylum seekers with conflict backgrounds can be strongly reduced by implementing public policies that facilitate finding employment and thus incentivize law-abiding behavior.

The remainder of the paper is organized as follows. Section I reviews the related literature, while Section II describes the data. Section III explains our identification strategy and Section IV presents our baseline results, as well as a battery of robustness checks. Bilateral crime regressions documenting violence toward specific nationalities are studied in Section V. Section VI analyzes the role of public policies and Section VII applies the analysis to the larger group of economic migrants. Finally, Section VIII concludes.

I. Literature Review

Since the pioneering work of Becker (1968), the literature on the economics of crime has examined various drivers of criminal behavior,[5] yet the nexus between migration and crime has received relatively little attention. Notable exceptions are the work of Bianchi, Buonanno, and Pinotti (2012) on Italy; Bell, Fasani, and Machin (2013) on the United Kingdom; and Butcher and Piehl (1998) on the United States. However, in these countries migrants are able to self-select their location, and the available data are much less fine-grained than in Switzerland.

[5] Prominent topics include the role of poverty and police activity (Levitt 1997 and Kelly 2000), unemployment and recessions (Fougère, Kramarz, and Pouget 2009), mineral discoveries (Couttenier, Grosjean, and Sangnier 2017), and urbanization (Glaeser and Sacerdote 1999).

Recent years have seen a growing literature on the effect of war experience. Rohner, Thoenig, and Zilibotti (2013a), for example, builds a model of vicious cycles of war experience resulting in low inter-group trust, and in turn, a higher likelihood of future violence. Empirically, the impact of conflict on education and health has been found to be unequivocally detrimental, yet its effect on collective action and trust remains an open question (see the survey by Bauer et al. 2016).[6] While several micro-level studies find positive effects of war exposure on (within-group) cooperation and social behavior at the local level (see Bauer et al. 2016), other research finds between-group hostility in the aftermath of conflict (e.g., Rohner, Thoenig, and Zilibotti 2013b). These contrasting findings can be reconciled by the view that the experience of conflict elevates "parochial altruistic" motivation (i.e., cooperation with in-group members and hostility toward out-group members), in line with Choi and Bowles (2007) and Cacault et al. (2015). In contrast, however, to previous studies that highlight the positive side of parochialism (i.e., altruism toward people from the same communities/villages having faced a similar threat), these authors emphasize a potentially darker side of parochial behavior (i.e., hostility toward the antagonistic group).

Of particular relevance as well is the literature on the persistence of violence. Miguel, Saiegh, and Satyanath (2011), for example, finds a strong positive relationship between the extent of civil conflict in a soccer player's home country and his propensity to behave violently on the field. Grosjean (2014) studies the persistence of a "culture of honor" (enforcing violent vendetta) "imported" into the United States by migrants, while Fisman and Miguel (2007) shows the persistence of social norms on corruption using data on the parking tickets of diplomats. A recent study by Blattman and Annan (2016) experimentally shows for Liberian ex-fighters that the persistence of violence can be attenuated by economic opportunities.

Various studies have examined the impact of exposure to different kinds of events during childhood. The psychology literature finds a particular vulnerability to war trauma for children between the ages of 5 and 9 (Kuterovac-Jagodić 2003; Barenbaum, Ruchkin, and Schwab-Stone 2004), while economists Giuliano and Spilimbergo (2014) detect a persistent effect of having experienced a recession in one's youth on individual beliefs and political preferences. In contrast, other research highlights the importance of high-quality early childhood interventions (e.g., Heckman, Pinto, and Savelyev 2013). Along these lines, Gould, Lavy, and Paserman (2011) identifies a beneficial impact of a "modern environment" during early childhood (0–5 years of age) on various socio-economic outcomes later in life. Conversely, Damm and Dustmann (2014) finds that the share of young criminals in a given neighborhood increases the probability of a young man committing a crime later in life. There is also experimental evidence that the formation of pro-social preferences, particularly those related to altruism, egalitarianism, meritocracy, and envy, is very active before the age of 12, especially between 6 and 12 (Almås et al. 2010; Bauer, Chytilová, and

[6] See, for example, recent papers studying the effect of war exposure on educational attainment (Blattman and Annan 2010, Shemyakina 2011), on mental health (Barenbaum, Ruchkin, and Schwab-Stone 2004; Derluyn et al. 2004), on political beliefs and participation and local collective action (Bellows and Miguel 2009, Blattman 2009, Humphreys and Weinstein 2007), and on trust and social capital (Fearon, Humphreys, and Weinstein 2009; Voors et al. 2012; Cassar, Grosjean, and Whitt 2013; Rohner, Thoenig, and Zilibotti 2013b; Besley and Reynal-Querol 2014; Gilligan, Pasquale, and Samii 2014).

Pertold-Gebicka 2014; Bauer, Fiala, and Levely 2018; Fehr, Bernhard, and Rockenbach 2008; Fehr, Glätzle-Rützler, and Sutter 2013). Cassar, Grosjean, and Whitt (2013) and Bellows and Miguel (2009) find a strong impact on preferences for conflict exposure in, respectively, the first 14 and 15 years of life.

Finally, our paper is also related to the literature on the economics of immigration (cf. Borjas 2003, Card 2001, Damm and Dustmann 2014) and work exploiting the exogenous allocation of migrants to study labor market outcomes (Edin, Fredriksson, and Åslund 2003; Beaman 2012; Hainmueller, Hangartner, and Lawrence 2016).

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

II. Data and Descriptive Statistics

Switzerland is a federal state with 26 cantons (the main subnational entities), with a population of about 8 million people, and a strong humanitarian tradition. According to the Swiss Federal Statistical Office, in 2016 about 25 percent of the population were foreign nationals. The number of asylum seekers defined as individuals who have applied and are waiting for approval of refugee status is considerably smaller: over the 2009–2016 period the yearly average of asylum seekers was around 34,300 individuals, corresponding to about 0.4 percent of the Swiss population. Most of these individuals originate from countries that have experienced war, genocide, political instability, and autocracy. The Swiss federal administration sets stringent conditions for obtaining political asylum. In particular, individuals must demonstrate that a return to their home country would endanger their lives, and economic deprivation cannot be the official reason for the request. As a result, on average only 10 percent of applicants are granted asylum (B permit), while around 20 percent are awarded temporary protection (F permit). The processing time for asylum requests varies with the number of submissions, but was approximately 249 days in 2016 according to the State Secretariat of Migration Annual Report. Online Appendix A2 provides more details on the admission procedure.

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

A. *Data on Asylum Seekers and Economic Migrants*

The Swiss State Secretariat for Migration (SEM) provided us with non-publicly available individual-level administrative data for all asylum seekers and economic migrants arriving in Switzerland from 1992 onward. We use the SEM information

from 2009–2016, as crime data are only available for this period. For each individual, we know their beginning and end of stay, location, nationality, age, gender, and residence status (i.e., type of permit held).[7]

B. *Data on Past Exposure to Conflict*

We use information on different forms of past exposure to conflict to construct our main explanatory variables. For *exposure to civil conflict*, we retrieve information from the Uppsala Conflict Data Program (UCDP 2017), which is by far the most widely used data on civil conflict. We include all civil conflicts reaching the UCDP/PRIO's threshold of at least 25 battle-related fatalities. For *exposure to mass killings*, we rely on a dataset on mass killings collected by the Political Instability Task Force (PITF 2016), where mass killings 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."[8] Note that exposure to a mass killing of civilians is a very different type of violence than that to civil war. We code an event as civil war only when the fighting is two-sided and battle-related casualties are sizable for all conflict parties. In contrast, mass killings of civilians are one-sided, where the latter are helpless victims, and the killings not necessarily related to battles. Hence, in many cases, mass killings take the form of purges by the state against civilians rather than armed conflict between the state and armed rebels.

Our individual data on asylum seekers do not report information on exposure to violence during conflict at the individual level. We consequently measure past conflict exposure at the cohort level. Our baseline measure is $kid\,[1–12]$, a binary variable that codes for cohorts aged 1 to 12 when the civil conflict or mass killing occurred in their origin country. Note that this cohort effect encompasses direct and indirect forms of exposure to conflict that are not possible to disentangle, such as being personally targeted by the act of violence (e.g., being injured or witnessing the killing of a family member), or being exposed to a context of war where economic deprivation and social capital depletion prevail. We focus on the first 12 years of life, in line with substantial evidence that many preferences and attitudes are formed during this period of time.[9] Moreover, beyond its intrinsic interest, a focus on exposure to

[7] There are various kinds of Swiss residence permits. For EU/EFTA citizens, there is the *L EU/EFTA permit* (short-term residents), the *B EU/EFTA permit* (resident foreign nationals with a valid employment contract; permit is issued for 5 years, renewable), the *C EU/EFTA permit* (settled foreign nationals who have been in Switzerland for at least 5 years; the holder's right to settle in Switzerland is not subject to any time restrictions or conditions), and the *G EU/EFTA permit* (cross-border commuters). For non EU/EFTA citizens there are analogous *B*, *C*, and *G* permits, as well as a *Permit F* (former asylum seekers who have been granted temporary protection), and a *Permit N* (asylum seekers). The law also foresees a so-called *Permit S* (for former asylum seekers who have been granted refugee status), but it has thus far hardly ever been used in practice, with asylum seekers obtaining permanent protection being awarded the *B* permit instead (Hofmann, Buchmann, and Trummer 2014, p. 23). For more information, see https://www.sem.admin.ch/sem/en/home/themen/aufenthalt.html.

[8] By this definition, episodes of mass killing have taken place in 28 different countries over the last 50 years, and include all of the most notorious historical instances of large-scale massacres such as, for example, those that occurred in Sudan, Rwanda, Bosnia, and Cambodia.

[9] See, for instance, Kuterovac-Jagodić (2003); Barenbaum, Ruchkin, and Schwab-Stone (2004); Fehr, Bernhard, and Rockenbach (2008); Almås et al. (2010); Fehr, Glätzle-Rützler, and Sutter (2013); Bauer et al. (2014); Bauer, Fiala, and Levely (2018).

violence during childhood alleviates endogeneity issues due to self-selection into violence (e.g., excluding former perpetrators, see Section III). Finally, in some specifications, we split our variable of exposure into two categories, *kid*[1–12] (*only CC*) and *kid*[1–12] (*only MK*), which correspond respectively to exposure to civil conflict or mass killing.

C. *Crime Data*

The Swiss Federal Statistical Office (FSO) provided us with non-publicly available exhaustive data on all crimes detected by the police in Switzerland between 2009 and 2016. This individual-level dataset was collected by local police services and covers all cases where a person was charged with infraction to the Swiss Penal Code. Remarkably, the data include precise information on the nationalities and residency status of both the victims and perpetrators of any detected crime, as well as on the place, time, and type of the crime. As we are interested in the perpetuation of violence, our main explanatory variable is a measure of violent crimes perpetrated by asylum seekers that encompasses offenses against life and limb (Swiss Penal Code, Title 1), felonies and misdemeanors against liberty (Swiss Penal Code, Title 4), and offenses against sexual integrity (Swiss Penal Code, Title 5). We also construct a measure of property crime that includes thefts, burglaries, robberies, or scams (Swiss Penal Code, Title 2). In the baseline analysis, we make no distinction in terms of nationality or background of the victims. This makes sense given that, in the data, violent asylum seekers target not only other asylum seekers but also the rest of the population: 36.65 percent of victims are themselves asylum seekers, 33.13 percent are foreign residents, 30.22 percent are natives. However, intra-asylum seeker violence is clearly over-represented and victim targeting is not random. This pattern is consequently at the core of the mechanism we investigate in Section V.

Due to the need to protect confidentiality, the FSO does not allow the merging of individual level crime data with migration data. Given this legal restriction, together with the fact that our explanatory variables of past exposure to violence are already measured at the cohort level, we conduct our statistical analysis at the level of a cohort of asylum seekers of nationality n, age a, in year t.[10] Hence, combining migration and crime datasets at the cohort-level, we build our main dependent variable, *violent crime propensity*, labeled $CP_{n,a,t}$, which corresponds to the share (scaled in percentage points) of individuals in the cohort who perpetrate at least one violent crime in a given year.[11] In the same vein, we define the victimization rate ($VR_{n,a,t}$) as the share of individuals in the cohort who are victims of at least one violent crime during a given year.

Exhaustive information of such high quality is available in Switzerland only for the detection data of crimes charged, but not for final convictions by a court. While the number of crimes charged is highly correlated with the amount of convictions, there may be discrepancies if, for example, in some cantons or years police

[10] In a previous version of this paper, data were only available until 2012 and age brackets were used instead of age.

[11] This definition of a cohort-specific crime propensity is not affected by recidivism, as we count the number of crimes committed by different individuals (i.e., if person A commits two crimes and person B of the same cell commits one crime, this results in the same overall crime propensity as when both of them commit one crime each).

authorities were more active and successful than in others. Such spatial differences in the detection probabilities of crime are however, accounted for by the exogenous allocation of asylum seekers across the Swiss territory (or the inclusion of canton × year fixed effects). There may also be a wedge between the crime rates of nationals and foreigners if, for example, some police forces were to predominantly control foreign-looking individuals. This would not, however, bias our estimation as we restrict ourselves to within-asylum seeker comparisons and do not compare asylum seeker crime rates with those of Swiss citizens.

D. *Descriptive Statistics*

We begin by describing the sample of asylum seekers. Over the 2009–2016 period the yearly average of asylum seekers was around 34,300 individuals. Table 1 displays some descriptive statistics on their age structure (for economic migrants, see Section VII) and lists the top ten countries of origin. As expected, it is not balanced in terms of age: 68 percent below the age of 30. Hence, young individuals, who are generally known for being the most violence-prone, are clearly over-represented among asylum seekers. Further, 24 percent of asylum seekers arrive in Switzerland with their family. One-half of the sample originates from either Eritrea, Afghanistan, Syria, or Sri Lanka.[12]

After aggregating asylum seekers by nationality n and age a, for each year t, this leaves us with our baseline dataset, composed of 13,720 male cohorts and 9,503 female cohorts (from 107 countries over 8 years and 71 age categories).[13] Table 2 reports the main descriptive statistics. The average cohort is composed of 11 individuals for males and 6 for women. Around 92 percent of male or female cohorts originate from countries that have experienced at least one episode of civil conflict or mass killing since 1946: civil conflicts occurred in 77 countries and mass killings in 27 countries. These are the nationalities that contribute to the identifying variations. More specifically, all these places experienced violence in some, but not all, years, leading to within-nationality, *inter-cohort* variations in exposure to violence: the sample mean of childhood exposure, $kid\,[1–12]$, is equal to 51 percent. Note that 46 percent of cohorts arrived in Switzerland in a year when active civil conflict was still raging in their home country. Further, 6 percent of cohorts originate from a country that is coded as experiencing current one-sided mass killings. Among the 77 countries of origin with a history of at least one civil conflict, around 12 years have passed on average since the last year of conflict.[14] As for the 27 origin countries with a history of at least one mass killing episode, 27 years have passed on average since the last year of such an event. Note that the chances of being recognized as a

[12] A graphical representation of the age distribution and nationality composition is provided in online Appendix A1.

[13] Note that asylum seekers who have lived in Switzerland for several calendar years are part of more than one observation unit.

[14] When weighted by the share of each nationality among the total number of asylum seekers, the average number of years since the last year of civil conflict is around 7.9 years. As expected, there are larger inflows of asylum seekers from countries with active or recent conflict. More specifically, there are, on average, 12.24 (SD = 35.55) asylum seekers in cohorts from countries with current conflict, 9.57 (SD = 16.63) in cohorts from countries where the last conflict occurred up to 10 years before, and 10.91 (SD = 41.27) asylum seekers in cohorts from countries where the last conflict occurred more than 10 years before.

Table 1—Share of Asylum Seekers in Switzerland by Age and Top Ten Countries of Origin

Age Class	Percent	Age Class	Percent	Country	Percent	Country	Percent
[0–17]	22.99	[45–49]	2.39	Eritrea	21.27	Nigeria	4.32
[18–20]	11.24	[50–54]	1.33	Afghanistan	11.09	Iraq	4.02
[21–24]	15.53	[55–59]	0.79	Syria	10.47	China	3.30
[25–29]	18.33	[60–64]	0.54	Sri Lanka	6.84	Turkey	2.81
[30–34]	13.34	[65–69]	0.28	Somalia	4.44	Serbia	2.62
[35–39]	8.27	[70–79]	0.23				
[40–44]	4.69	[80+]	0.04				

Note: Data from the State Secretariat of Migration over the 2009–2016 period.

Table 2—Summary Statistics for the Cohorts of Asylum Seekers

	Men				Women			
Variable	Mean	Standard deviation	Min.	Max.	Mean	Standard deviation	Min.	Max.
Cohort size (number of individuals)	11.18	34.88	1	1,251	6.08	15.08	1	265
History of civil conflict or mass killing (%)	91.58	27.77	0	100	91.11	28.46	0	100
History of civil conflict (%)	91.22	28.31	0	100	90.92	28.74	0	100
History of mass killing (%)	41.81	49.33	0	100	53.48	49.88	0	100
kid [1–12] (CC or MK, %)	50.58	49.99	0	100	50.55	49.99	0	100
kid [1–12] (CC, %)	49.13	49.99	0	100	48.68	49.99	0	100
kid [1–12] (MK, %)	17.48	37.98	0	100	21.68	41.21	0	100
Violent crime propensity ($CP_{n,a,t}$, %)	3.09	11.34	0	100	0.30	3.69	0	100
Victimization rate ($VR_{n,a,t}$, %)	3.39	14.92	0	100	2.61	13.83	0	100

Notes: All summary statistics are based on 13,720 cohorts of males and 9,503 cohorts of females, all at least 18 years of age. Abbreviations CC and MK refer respectively to civil conflict and mass killing.

refugee increase if there is proof of having been personally persecuted, as opposed to solely coming from a country currently experiencing civil conflict. The fact that asylum seekers come from places with recent conflict increases the accessibility and reliability of such testimonies. In online Appendix A1 we provide a full description of the various civil conflicts and mass killings for each nationality.

We now turn to cohort-level (violent) crime propensities. A well-documented pattern in the crime literature is the fact that only a negligible part of violence is perpetrated by women (Freeman 1999). This gender imbalance is also true for asylum seekers: average violent crime propensity amounts to 3.09 percent for males and 0.30 percent for females. Consequently, most of our empirical investigation is conducted on the sample of men (the entire analysis is replicated for women in online Appendix A6.4).

Figure 1 explores the age-crime nexus by reporting average propensity by age for the two groups of cohorts at the core of our identification strategy: cohorts exposed to civil conflict or mass killing during childhood (in black) and those born afterward (in gray).[15] For both groups, we see a clear spike in violent crime in early adulthood and then a steady decrease across ages. The pattern and magnitude conform to the large

[15] We restrict ourselves to the subsample of cohorts from countries with a conflict or mass killing background that were born after war or exposed during their childhood ($kid[1–12] = 1$). For each age, we average $CP_{n,a,t}$ across cohorts and time.

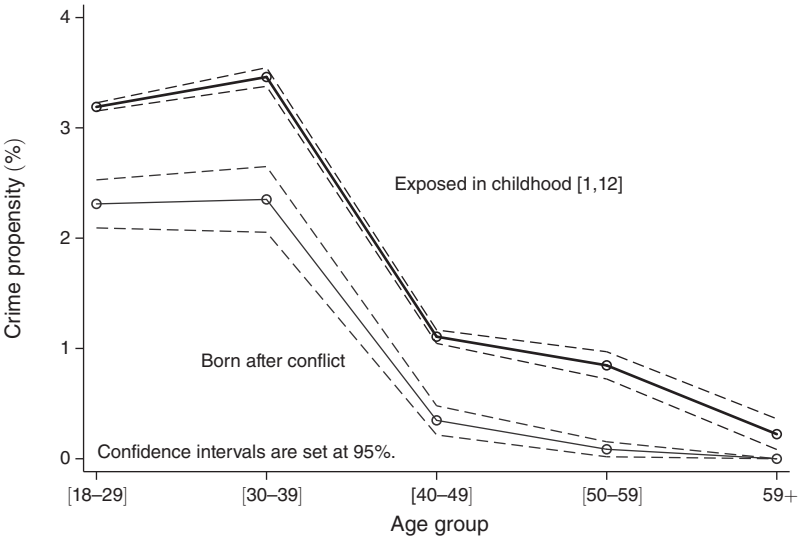

FIGURE 1. AGE-VIOLENCE CURVES

Note: The figure reports the average crime propensity by age for two groups of cohorts: (i) cohorts exposed to civil conflict or mass killing during childhood (in black); (ii) cohorts born after a conflict (in gray).

body of evidence on age-crime curves in the criminology literature for other populations and periods (see Freeman 1999 for a review on the determinants of criminal behavior). The striking and novel point here relates to the crime differential between the two groups: cohorts with past exposure to conflict during childhood have significantly higher crime propensities than those born after conflict. This discrepancy is observed across all age categories; only after the age of 60 do the two curves converge on a low level. The average differential is equal to 0.73 percentage points, a substantial wedge that implies that cohorts exposed during childhood are on average 1.75 times more prone to violent crimes than those born after a conflict. This graphical evidence illustrates our main result. The econometric analysis that follows aims to confirm that this excess crime propensity is in fact related to the exposure to violence in childhood, accounting for a variety of potential confounding factors.

III. Identification Strategy

Our source of identification corresponds to variations in crime-propensities across cohorts of asylum seekers from the *same nationality and migration wave* but with different exposure to conflict (i.e., born after war/exposed in childhood/exposed at later ages). Because these cohorts inevitably differ in terms of age, we must control for the direct effect of age by comparing them to other cohorts with similar age structure but with different exposure to conflict in another country. To give an intuitive example, our strategy consists of computing the crime differential of two Rwandese cohorts, one born in 1996 (after the 1994 genocide), and one born in 1990 (exposed during childhood), both migrating to Switzerland in 2015. Their crime differential is compared to the one of two Nigerian cohorts of the same ages but both being born

(in 1990 and 1996) after the 1967–1970 civil war. Our comparison of the black/gray crime-age curves in Figure 1 follows the same logic.

More formally, our unit of observation is a cohort, and our baseline crime regression corresponds to[16]

$$(1) \qquad CP_{n,a,t} = \alpha \times kid[1\text{–}12]_{n,a,t} + \sum_{k=13}^{80+} \beta(k) \times expo(k)_{n,a,t} + \mathbf{FE}_{n,t} + \mathbf{FE}_{a} + \varepsilon_{n,a,t},$$

where $CP_{n,a,t}$ stands for the violent crime propensity of a cohort of nationality (n) and age (a) living in Switzerland in year (t). Note the absence of a subscript for gender in the previous specification, the reason being that, throughout the paper, all specifications are estimated separately on the subsamples of men and women. As discussed above, our main explanatory variable is $kid[1\text{–}12]_{n,a,t}$, a binary measure of early age exposure. The set of control variables $expo(k)_{n,a,t}$ are also binary variables coding for past exposure, but at the later ages k.[17] Crucially, the richness of our dataset allows for the inclusion of fixed effects that account for unobserved heterogeneity in nationality $\times$ year $(\mathbf{FE}_{n,t})$ and in age $(\mathbf{FE}_{a})$. Hence, the identifying variations come from all nationalities with substantial cross-cohort heterogeneity in past exposure such that the exposure dummies, $kid[1\text{–}12]_{n,a,t}$ and $expo(k)_{n,a,t}$, are not collinear with the nationality fixed effect $\mathbf{FE}_{n,t}$. Finally, robust standard errors are clustered at the nationality level.

In equation (1), the implicit reference group consists of cohorts born after a conflict (such that all the exposure dummies are equal to zero). Thus, our parameter of interest α can be interpreted as the crime differential between cohorts exposed during their childhood and cohorts born after the conflict. Our identifying assumption is that past exposure to conflict is the only reason why there is less decline in crime rates with age for asylum seekers exposed in childhood than for their co-nationals born after the conflict.[18] A threat to our identification strategy would be that post-war contexts are systematically associated with a flattening of the age-crime curve for

[16] The decision of whether to perpetrate a crime is, however, made at the individual level. A specification based on microdata would have the individual as unit of observation and would estimate a random-utility discrete-choice model, such as, e.g., a binomial logit. For samples based on grouped data, like ours, Durlauf, Navarro, and Rivers (2010) shows that the logit model translates into a linear specification where the dependent variable is the log of the odds ratio of the crime propensity. They recommend implementing this aggregate logit procedure only when the aggregation level is sufficiently high such that sampling errors are limited and group-level crime frequencies approximate well the underlying crime probabilities. In our context, the average cohort size is not large (i.e., 11 individuals) and, more importantly, the variance is large with many small cohorts: those with 5 individuals or less represent 68 percent of the sample. Hence, sampling errors become a salient issue and, together with the fact that crime is a rare event, implies that the number of zeros is very large ($CP_{n,a,t} = 0$ for 83.6 percent of cohorts), making the computation of an odds ratio problematic. We consequently prefer a baseline specification that is compatible with extreme values in $CP_{n,a,t}$ by estimating a linear crime regression with $CP_{n,a,t}$ as dependent variable. This choice follows the standard practice in the crime literature (see, e.g., Bell, Fasani, and Machin 2013). Finally, we investigate in robustness analyses alternative options and econometric specifications for dealing with small cohorts and extreme values (online Appendix A5).

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

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

all cohorts. In this respect, a reassuring fact is the robustness of our results when the estimation sample is restricted to nationalities with a recent history of conflict (columns 2 to 4 in the baseline Table 5, panel A). In this case, all in-sample cohorts have been exposed to a conflict or to a post-conflict context.

Another encouraging pattern is the observation in column 2 of Table 14 of a *sharp decrease* in crime propensity between cohorts born during conflicts and those born *just* after.

A. *Exogenous Spatial Allocation of Asylum Seekers in Switzerland*

The potential concern about self-selection into a crime-facilitating canton is addressed by the exogenous allocation of asylum seekers on the part of the Swiss authorities.[19] In what follows, we provide a brief overview of this process of allocation as well as discuss statistical evidence supporting the view that the distribution key is based on canton population size only and is exogenous to migrant characteristics.

Overview of the Allocation Process: Most asylum seekers enter Switzerland through one of the borders near which the national reception and procedure centers (RPC) are located with the purpose of applying for asylum. In the RPC, asylum seekers are interviewed, and asked to provide proof of identity, fingerprints, and their reasons for application. After this initial selection, only the so-called "credible" asylum seekers are granted a temporary N permit by the Swiss authorities. Given the difficulty in assessing the threat of persecution in the home country and the large number of applicants (around 34,300 per year over the 2009–2016 period), the asylum process takes substantial time. The average duration of the process was 249 days in 2016 according to the State Secretariat of Migration report (for further details on the asylum process, see online Appendix A2).

Crucially, during this period, holders of the N permit are exogenously allocated to cantons and are not allowed to change canton.[20] The allocation of new N permit holders to the 26 Swiss cantons is determined by an exogenous allocation key based on the cantonal population. This approach was introduced with an amendment to the Aliens Law in 1988, presumably to minimize self-segregation and ghetto effects and avoid social tension between natives and asylum seekers. The State Secretariat of Migration (SEM) assigns the canton and the decision cannot be appealed with the exception of very specific conditions (i.e., if a minor was allocated to a different canton than her parents or if the asylum seeker or a third person were under serious threat) and any change is only possible with the approval of both cantons. According

[19] While this exogenous allocation to cantons rules out self-selection of asylum seekers into more crime-prone cantons, it does not, of course, exclude that asylum seekers may choose to spend greater amounts of time in more crime-prone areas *within* a given canton. However, given that Swiss cantons are relatively small units and that many crucial policies are implemented at the canton level, the exogenous allocation across cantons is arguably of crucial importance.

[20] This exogenous allocation is described in official documents, such as in Chapter F6 of the State Secretary of Migration's *Handbook of Asylum and Return* (2015), which describes "random allocation" (Zufallsprinzip). This system is also exploited by Hainmueller, Hangartner, and Lawrence (2016, p. 2) which similarly states that "During their waiting period, asylum seekers are housed in accommodations in a randomly assigned Swiss canton and they are not allowed to leave this canton."

to Hofmann, Buchmann, and Rummer (2014), it is extremely rare that asylum seekers change canton or cantons refuse asylum seekers. Once an asylum seeker has been allocated to a given canton, the latter organizes accommodation in a cantonal center or individual flat and is responsible for interviews and financial matters.

Balancing Tests: We now address the question of whether there is indeed exogenous allocation of asylum seekers through the official population-based distribution key, as the Federal administration claims, or if there remains some selection on relevant dimensions. For example, such selective allocation could occur if, say, the urban (and supposedly more crime-prone) cantons of Zürich or Geneva were more likely to host young males fleeing a conflict zone, while rural and quieter cantons were to host other asylum seekers. We consequently estimate whether the placement of asylum seekers is overall uncorrelated with canton characteristics. Table 3 displays a series of unconditional and conditional balancing tests which compare characteristics of the canton of residence between the conflict-exposed cohorts at early ages ($kid\,[1–12] = 1$) and the other cohorts ($kid\,[1–12] = 0$). The regression equation is given by

$$(2)\qquad canton_{c,t} = \alpha \times kid\,[1–12]_{n,a,t} + \sum_{k=13}^{80+} \beta(k) \times expo(k)_{n,a,t} + \mathbf{FE}_{n,t} + \mathbf{FE}_{a} + \mathbf{FE}_{g} + \varepsilon_{n,a,g,c,t},$$

where the unit of observation is a cohort of nationality (n) $\times$ age (a) and gender (g) living in canton (c) in year (t). The previous equation is estimated on our baseline sample of adult asylum seekers originating from countries that have experienced civil conflict or mass killing since 1946, disaggregated at the canton level. For the sake of space, we report only the results for the pooled sample (i.e., with male and female cohorts); (unreported) results are similar when balancing tests are conducted separately on the two subsamples. The set of covariates $kid\,[1–12]_{n,a,t}$ and $expo\,(k)_{n,a,t}$, and the fixed effects $\mathbf{FE}_{n,t}$, $\mathbf{FE}_{a}$ are identical to those included in our baseline econometric equation (1); $\mathbf{FE}_{g}$ is a gender fixed effect. The outcome variable $canton_{c,t}$ corresponds to an observable characteristic of the canton c in year t to which the cohort has been allocated. The set of cantonal characteristics includes the local composition of asylum seekers, the composition of the existing local immigrants' enclave, GDP/cap, total population, and total crime rate (definitions of the variables are relegated to the table note).

Each entry in the table corresponds to an estimation for a given characteristic. Columns 1 to 3, 7 to 9, and 13 to 15 display unconditional mean differences and columns 4 to 6, 10 to 12, and 16 to 18 report the conditional mean differences when the battery of fixed effects is included. In most cases, the mean differences are not statistically different from zero (at the conventional thresholds). More importantly, they are small in magnitude with respect to both the sample mean and the standard deviation of the considered characteristic. We conclude from this exercise that, reassuringly, there is no evidence of the placement of asylum seekers along the $kid\,[1–12]$ dummy. We complement this analysis in online Appendix A3 by testing for the difference in means between cantons for other observable characteristics. All in all, the

Table 3—Balancing Tests: Cantonal Placement

	Asylum Seekers					
	Unconditional			Conditional		
Dependent variable:	Between countries (1)	Within countries (2)	Sex ratio (3)	Between countries (4)	Within countries (5)	Sex ratio (6)
kid [1–12]	−1.241	−0.251	0.069	0.039	0.091	−0.183
	(1.391)	(0.494)	(2.953)	(0.060)	(0.104)	(0.104)
	[0.374]	[0.613]	[0.981]	[0.513]	[0.382]	[0.083]
Observations	91,195	91,195	91,195	91,134	91,134	91,134
R^2	0.007	0.000	0.000	0.919	0.308	0.690
Sample mean (dep. var.)	6.4	7.45	71.18	6.4	7.45	71.18
	(7.37)	(7.02)	(18.43)	(7.37)	(7.02)	(18.43)
	Economic Migrants					
	Unconditional			Conditional		
Dependent variable:	Between countries (7)	Within countries (8)	Sex ratio (9)	Between countries (10)	Within countries (11)	Sex ratio (12)
kid [1–12]	−0.377	−0.533	−0.714	0.029	0.287	−0.171
	(0.590)	(0.548)	(2.644)	(0.020)	(0.105)	(0.150)
	[0.525]	[0.334]	[0.788]	[0.164]	[0.008]	[0.261]
Observations	64,325	64,325	64,325	64,283	64,283	64,283
R^2	0.003	0.001	0.001	0.716	0.081	0.568
Sample mean (dep. var.)	1.28	7.29	56.2	1.28	7.29	56.2
	(2.93)	(7.66)	(13.47)	(2.93)	(7.66)	(13.47)
	Unconditional			Conditional		
Dependent variable:	GDP/cap (13)	Population (14)	Crime rate (15)	GDP/cap (16)	Population (17)	Crime rate (18)
kid [1–12]	−0.443	−3.179	−0.187	−0.158	1.770	−0.014
	(0.392)	(6.104)	(0.108)	(0.291)	(2.631)	(0.034)
	[0.261]	[0.604]	[0.087]	[0.589]	[0.503]	[0.685]
Observations	70,544	94,176	94,176	70,498	94,115	94,115
R^2	0.000	0.000	0.001	0.017	0.088	0.088
Sample mean (dep. var.)	1,121.79	1,282.28	6.44	1,121.79	1,282.28	6.44
	(24.73)	(92.56)	(2.64)	(24.73)	(92.56)	(2.64)

Notes: 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 in parentheses are clustered at the nationality level. *p*-values are reported on brackets. The dependent variable at the cantonal-year level is alternatively (i) the number of adult asylum seekers by country of origin × year × canton over the number of adult asylum seekers by year × canton (columns 1 and 4); (ii) the number of adult asylum seekers by country of origin × year × canton over the number of adult asylum seekers by country × year (columns 2 and 5); (iii) the proportion of male asylum seekers (columns 3 and 6); (iv) the number of adult economic migrants (B&C permits) by country of origin × year × canton over the number of adult economic migrants (B&C permits) by year × canton (columns 7 and 10); (v) the number of adult economic migrants (B&C permits) by country of origin × year × canton over the number of adult economic migrants (B&C permits) by country × year (columns 8 and 11); (vi) the proportion of male economic migrants (B&C permits) (columns 9 and 12); (vii) the cantonal GDP/cap (2009–2014) (columns 13 and 16); (viii) the total permanent population (2009–2016) (columns 14 and 17); (ix) the total crime rate (2009–2016) (columns 15 and 18). The sample mean and the standard deviation of the dependent variables are reported. The explanatory variable is the exposure to civil conflict or mass killing between the ages of 1 and 12 (*kid* [1–12]). Country × year, age, and gender fixed effects are included in the conditional estimations, namely in columns 4 to 6, 10 to 12, and 16 to 18.

tests confirm that the characteristics of asylum seeker cohorts are balanced across Swiss cantons.

B. *Post-Conflict Exposure and Selection into Migration*

As discussed above, our empirical design relies on the assumption that past exposure to conflict is the only reason why there is less decline in crime rates with age among asylum seekers exposed to conflict in childhood than for their co-nationals born after. In what follows, we provide supportive evidence of this assumption by showing that cohorts exposed to conflict during childhood do not differ substantially on a range of post-exposure characteristics compared to other cohorts. More specifically, we estimate an equation that is similar to our master econometric equation (1) except that the left-hand-side variable is now an observable cohort characteristic. The overall sample is also identical to the baseline one, the male and female subsamples being pooled together. The array of cohort characteristics corresponds to: the number of asylum seekers per cohort; a dummy equal to 1 if the age of the cohort is below the median age of the co-national cohorts; the percentage of men; the share of asylum seekers who arrive with family members; the share of asylum seekers working in services or offices; the size of the enclave (i.e., number of economic migrants with a B or C permit) relative to the cohort size; the number of ethnic groups.[21]

The results of these balancing tests are displayed in Table 4. Each entry reports the estimated coefficient of the variable $kid[1–12]$ for a given cohort characteristic. This coefficient can be interpreted as the mean difference of observable characteristics between early age exposed cohorts ($kid[1–12] = 1$) and other cohorts ($kid[1–12] = 0$). Panel A displays unconditional mean differences and panel B reports the conditional mean differences when all the fixed effects are included. We see that the mean differences are not statistically different from zero (at conventional thresholds) and are small in magnitude with respect to the sample mean and standard deviation of the characteristic. These results support the view that there is no differential selection into migration based on childhood exposure. In online Appendix A3.2 we reach a similar conclusion when the balancing tests are conducted in a multivariate way. We also show that cohorts exposed during childhood are not systematically from a different ethnic group than other cohorts. Finally our robustness analysis shows that our baseline results on the impact of childhood exposure on crime propensity are unchanged when controlling directly for all those cohort characteristics in the econometric equation (1) (see online Appendix A5.4).

Finally, in online Appendix A4 we complement this analysis by estimating whether there is any cross-cohort difference in the incentives to apply for asylum in Switzerland. Any different perspective for obtaining asylum could drive differences in incentives for engaging in crime. The decision to grant or not asylum is determined by the current level of threat faced by our sample of adult asylum seekers, and exposure many years prior, during childhood, is presumed not to affect the decisions of the authorities. We test this idea by estimating the cohort-specific likelihood of being granted a residence permit on the $kid[1–12]$ dummy. We find that conflict

[21] Information on the sector of occupation is especially incomplete. Given the available information, we define as white collar asylum seekers those working in services or offices, science and arts, or architecture and technology.

Table 4—Balancing Tests: Cohort-Level Observable Characteristics

Dependent variable	Cohort size	Below median age	Sex ratio	% Have family	% White collar	Enclave size/ cohort size	# Ethnic groups
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Unconditional							
kid [1–12]	3.569	0.263	0.036	−0.023	0.008	−9.237	0.346
	(2.494)	(0.071)	(0.025)	(0.032)	(0.009)	(11.231)	(0.156)
	[0.156]	[0.000]	[0.146]	[0.480]	[0.377]	[0.413]	[0.030]
Observations	21,383	21,383	21,383	20,725	3,744	21,383	17,802
R^2	0.004	0.069	0.003	0.001	0.001	0.002	0.018
Sample mean	9.72	0.49	0.67	0.32	0.04	28.18	1.87
(dep. var.)	(29.72)	(0.49)	(0.31)	(0.39)	(0.16)	(105.23)	(1.26)
	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Panel B. Country × year, age, and gender fixed effects							
kid [1–12]	0.806	−0.007	0.010	0.018	−0.002	−1.229	0.053
	(4.139)	(0.029)	(0.011)	(0.019)	(0.011)	(4.967)	(0.092)
	[0.846]	[0.821]	[0.384]	[0.345]	[0.892]	[0.805]	[0.565]
Observations	21,320	21,320	21,320	20,662	3,631	21,320	17,756
R^2	0.326	0.773	0.460	0.365	0.136	0.682	0.504
Sample mean	9.75	0.49	0.67	0.32	0.03	28.1	1.87
(dep. var.)	(29.76)	(0.49)	(0.3)	(0.39)	(0.16)	(104.94)	(1.27)

Notes: 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 consists of the set of cohort observable characteristics and the explanatory variable is the exposure to civil conflict or mass killings between the ages of 1 and 12 (*kid* [1–12]). The dependent variable is alternatively (i) the number of asylum seekers per cohort (columns 1 and 8); (ii) a dummy equal to 1 if the age of the cohort is below the median age of the co-national cohorts (columns 2 and 9); (iii) the percentage of men (columns 3 and 10); (iv) the share of asylum seekers who arrived with family members (columns 4 and 11); (v) the share of asylum seekers working in services or offices (columns 5 and 12); (vi) the ratio between the number of economic migrants holding a B or C permit and the number of asylum seekers (columns 6 and 13); (vii) the number of ethnic groups (columns 7 and 14). The sample mean and the standard deviation of the dependent variables are reported.

exposure during childhood ($kid[1–12] = 1$) does not affect the current chances of obtaining asylum. By contrast, we find that present institutional quality reduces the acceptation rate, while the current outbreak of a civil conflict or mass killings in the country of origin increases the acceptation rate; economic development does not have any impact.

IV. Conflict Exposure and Violent Crimes Empirical Results

A. *Baseline Results*

This section establishes our core result, namely that early age exposure to civil conflict/mass killing substantially increases the violent crime propensity of adult male asylum seekers. Many policy-relevant channels contribute to this phenomenon and Section V focuses specifically on the role of intra-national grievances. We do not, however, detect any effect of exposure during childhood for females, for non-violent crimes, or for victimization rates. Consequently, in what remains of the paper we focus our analysis on the violent criminal behavior of men.

Violent Crime Propensity: Panel A of Table 5 displays the results of our cohort-level crime regression (equation (1)) estimated on the sample of adult male

Table 5—Violent Crime Propensity and Conflict Exposure

Sample:	Violent crime propensity			
	Full	CC and MK	CC	MK
	(1)	(2)	(3)	(4)
Panel A. Men				
kid [1–12]	1.096	1.085		
	(0.461)	(0.454)		
kid [1–12] (*only CC*)			1.220	
			(0.478)	
kid [1–12] (*only MK*)				1.167
				(0.591)
Age FE	Yes	Yes	Yes	Yes
Nationality × year FE	Yes	Yes	Yes	Yes
Observations	13,720	12,564	12,514	5,737
R^2	0.193	0.200	0.200	0.196
Sample mean (crime propensity)	3.09	3.09	3.11	2.59
	(5)	(6)	(7)	(8)
Panel B. Women				
kid [1–12]	0.026	0.033		
	(0.117)	(0.118)		
kid [1–12] (*only CC*)			−0.045	
			(0.129)	
kid [1–12] (*only MK*)				0.275
				(0.203)
Age FE	Yes	Yes	Yes	Yes
Nationality × year FE	Yes	Yes	Yes	Yes
Observations	9,503	8,658	8,640	5,082
R^2	0.092	0.055	0.055	0.063
Sample mean (crime propensity)	0.30	0.27	0.27	0.32

Notes: OLS estimations based on the sample of 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. 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 later ages $k \in \{13, 14, 15, \ldots, 80+\}$. 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.

asylum seekers (above 18 years old). We report only our coefficient of interest, $\hat{\alpha}$, which captures the violent crime over-propensity of cohorts exposed to civil war or mass killing during childhood (1–12 years), the reference group being cohorts born after conflict. Column 1 reports the results of a specification with age and country × year fixed effects where the identifying variations come from a within-nationality/between-cohort comparison. The coefficient of interest is positive and significant at the 5 percent threshold. In column 2 we exclude all cohorts originating from countries that have experienced no civil conflict or mass killing since 1946 (as discussed in Section III). This is our preferred specification (baseline). The point estimate barely changes in spite of the sample size reduction. In terms of magnitude, we observe that the crime propensity of cohorts exposed during childhood is on average 1.1 percentage points higher than the propensity of their

co-national cohorts born after the war: a substantial effect given that the sample mean of violent crime propensity is equal to 3.1 percentage points. Thus, cohorts exposed to civil conflict/mass killing during childhood are 35 percent (= 1.1/3.1) more prone to violent crime than the average cohort of asylum seekers.[22] As a benchmark, age has comparable consequences on crime propensity (coefficients not reported here): on average, the 20–29-year-old cohorts have a 5.2 percentage point higher crime propensity than cohorts older than the age of 50; this drops to 4.22 percentage points for the 30–39 age bracket and to 1.77 percentage points for the 40–49-year-olds. In sum, even if age tends to be a powerful determinant of crime, past exposure to conflict in childhood still substantially matters. In columns 3 and 4 we estimate the same specification as in column 2, but now separately for conflict and mass killings. In each case, the sample is again restricted to countries having experienced each specific type of violence.

Panel B replicates the previous approach for the sample of adult female asylum seekers. The coefficient of interest loses its statistical significance at conventional thresholds in all specifications. More importantly, its magnitude drops massively with respect to its counterpart for the sample of males, both in absolute and relative terms, as the coefficient is reduced by a factor of 30 in the baseline specification (i.e., comparing columns 2 and 6) while the sample mean is reduced by a factor of 10. This shows that childhood exposure to civil conflict or mass killing does not significantly affect the future violent criminal behavior of women.

Types of Crimes: In Table 6, we further document the nature of the crimes perpetrated by asylum seekers. To this end, we split our outcome variable of violent crime propensity into three subcategories and, for each, estimate our baseline specification (Table 5, panel A, column 2) on the sample of adult male asylum seekers.[23] We consider crimes against life and limb (column 1); felonies and misdemeanors against liberty (column 2); and offenses against sexual integrity (column 3). Both the magnitude and statistical significance of our coefficient of interest are strikingly lower for sexual offenses. This is in line with previous findings that sexual offenses are often explained by other, more idiosyncratic, drivers than general criminal and antisocial behavior (see, for example, the meta-analysis by Seto and Lalumière 2010).[24] For comparative purposes, in column 4 we consider property crimes, namely a large class of non-violent crimes, which is excluded from our baseline analysis but encompasses thefts, burglaries, robberies, or scams (Swiss Penal Code, Title 2, art. 137 to 172). The coefficient loses

[22] Note that the sample consists of 50.6 percent of observations where $kid[1–12] = 1$, and 49.4 percent of observations where $kid[1–12] = 0$. Given that the estimated value of the coefficient of $kid[1–12]$ is equal to 1.1, this implies that the counterfactual sample mean would be equal to 2.54 percent if all cohorts had not been exposed during childhood (i.e., $kid[1–12]$ switches to 0 for all cohorts). Hence, if we were to ask the (somewhat different) accounting question of by how much does conflict exposure during childhood contribute to the observed crime propensity in the sample, we would obtain an even larger number, namely 43 percent (= 1.1/2.54).

[23] The category "life and limb" (Swiss Penal Code, Title 1, art. 111 to 136) includes murder, homicide, endangering the life of others, and bodily injuries. The category "felonies and misdemeanors against liberty" (Swiss Penal Code, Title 4, art. 180 to 186) corresponds to threat, constraint, abduction, and home invasion. The category "sexual integrity" (Swiss Penal Code, Title 5, art. 187 to 200) includes rape, sexual constraint, and prostitution.

[24] This result for sexual offenses is important as it speaks to controversy recently sparked by events at Carnival in Cologne (see *New York Times*, January 8, 2016, "18 Asylum Seekers Are Tied to Attacks on Women in Germany").

Table 6—Crime Propensities and Conflict Exposure by Types of Offenses

Dependent variable:	Crime propensity			
Type of offenses:	Life and limb (1)	Against liberty (2)	Sexual integrity (3)	Property (4)
Panel A. Men				
kid [1–12]	0.592	0.892	0.010	0.773
	(0.277)	(0.407)	(0.062)	(0.848)
Age FE	Yes	Yes	Yes	Yes
Nationality × year FE	Yes	Yes	Yes	Yes
Observations	12,564	12,564	12,564	12,564
R^2	0.112	0.198	0.069	0.396
Sample mean (crime propensity)	1.39	2.34	0.25	5.02
	(5)	(6)	(7)	(8)
Panel B. Women				
kid [1–12]	0.061	0.015	0.006	−0.224
	(0.111)	(0.075)	(0.012)	(0.377)
Age FE	Yes	Yes	Yes	Yes
Nationality × year FE	Yes	Yes	Yes	Yes
Observations	8,658	8,658	8,658	8,658
R^2	0.053	0.053	0.040	0.114
Sample mean (crime propensity)	0.17	0.17	0.006	1.04

Notes: 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 crime propensity of a cohort of nationality (n) × age (a) × year (t) for the different types of crimes. *kid* [1–12] is a binary measure of childhood exposure to civil conflict or mass killings. 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 later ages $k \in \{13, 14, 15, \ldots, 80+\}$. Therefore, the reference group consists of the individuals born after the last years of violence. The category *Life and limb* (Swiss Penal Code, Title 1, art. 111 to 136) includes murder, homicide, and endangering the lives of others, and bodily injuries. The category *Felonies and misdemeanors against liberty* (Swiss Penal Code, Title 4, art. 180 to 186) corresponds to threat, constraint, abduction, and home invasion. The category *Sexual integrity* (Swiss Penal Code, Title 5, art. 187 to 200) includes rape, sexual constraint, and prostitution. The category *Property* (Swiss Penal Code, Title 2, art. 137 to 172) encompasses thefts, burglaries, robberies, or scams.

statistical significance at the conventional thresholds and its magnitude relative to the sample mean $(0.77/5.02)$ is slightly reduced. This result suggests that exposure to conflict during childhood impacts the future violent behavior of men, but leaves their future non-violent criminality unaffected. We interpret this contrasting evidence as a first indication that our effect captures a mechanism of perpetuation of violence, a point we develop in more detail in Section V. From a causal analysis perspective, we interpret the absence of an effect for property crime as an indication that the correlation between past exposure and violent crime is unlikely to be spuriously driven by omitted factors (unless such factors were to affect differentially violent crime and property crime). Finally, note that once again, no effect is detected for women (panel B).

Victimization Rate: To complement our analysis on criminal behavior, we also study adult asylum seekers as victims rather than criminals. As displayed in Table 2, on average 3.39 percent of adult male asylum seekers are victims of violent crimes

Table 7—Victimization Rate

Dependent variable:	Victimization rate			
	Violent crimes (1)	Life and limb (2)	Against liberty (3)	Sexual integrity (4)
Panel A. Men				
kid [1–12]	−0.013	−0.261	0.220	−0.004
	(0.395)	(0.360)	(0.202)	(0.078)
Age FE	Yes	Yes	Yes	Yes
Nationality × year FE	Yes	Yes	Yes	Yes
Observations	12,564	12,564	12,564	12,564
R^2	0.083	0.080	0.058	0.044
Sample mean (victimization rate)	3.59	2.67	1.23	0.05
	(5)	(6)	(7)	(8)
Panel B. Women				
kid [1–12]	0.446	0.369	0.357	−0.066
	(0.343)	(0.294)	(0.234)	(0.111)
Age FE	Yes	Yes	Yes	Yes
Nationality × year FE	Yes	Yes	Yes	Yes
Observations	8,658	8,658	8,658	8,658
R^2	0.084	0.077	0.064	0.061
Sample mean (victimization rate)	2.70	1.62	1.41	0.33

Notes: 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 victimization rate of a cohort of nationality (n) × age (a) × year (t) for the different types of crimes. 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 and nationality × year fixed effects. The reference group consists of those individuals born after the last years of violence. The category *Life and limb* (Swiss Penal Code, Title 1, art. 111 to 136) includes murder, homicide, endangering the lives of others, bodily injuries. The category *Felonies and misdemeanors against liberty* (Swiss Penal Code, Title 4, art. 180 to 186) corresponds to threat, constraint, abduction, and home invasion. The category *Sexual integrity* (Swiss Penal Code, Title 5, art. 187 to 200) includes rape, sexual constraint, and prostitution. The category *Property* (Swiss Penal Code, Title 2, art. 137 to 172) encompasses thefts, burglaries, robberies, or scams.

in a given year (2.61 percent of females). We test whether conflict-related victimization during childhood translates into a higher likelihood of being victimized later in life, the rationale put forward in the social psychology literature being that early age trauma has a long-standing effect on personality and vulnerability. To this end, we replicate our baseline specifications for men and women respectively (columns 2 and 6 in Table 5) with the cohort-level victimization rate as dependent variable (in place of crime propensity). Table 7 displays the main results. We focus on victimization related to all violent crimes in columns 1 and 5 and by category of violent crime in the other columns. In all specifications, the coefficient of interest is small in magnitude (both in absolute and relative terms) and statistically insignificant at the conventional thresholds. Hence, we detect no effect of childhood exposure to civil conflict and mass killing on future victimization, either for men or women.

Sensitivity Analysis: In online Appendix A5 we show that the baseline estimate of Table 5, panel A, column 2 is robust to a battery of sensitivity checks. In particular, we investigate robustness to the exclusion of potential outliers (i.e., a specific set of

nationalities), 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, control for additional cohort-level cohorts, and test the robustness when adding ethnic group fixed effects. We find that our results are robust to all these sensitivity tests.

B. *Age Effects*

In this section, we examine the modulating effect of age on the criminal behavior of male asylum seekers. In our context, age matters in two respects: (i) its dampening effect on crime propensity and (ii) the time of life during which individuals are exposed to conflict. While the former is relatively standard in the crime literature, the latter is particularly important given our emphasis on conflict exposure during *childhood*. We thus fully investigate how conflict exposure at different ages impacts future criminal behaviors. The results confirm the critical role of childhood and, in particular, of the first five years of life.

Life-Cycle Modulation and Early Conflict Exposure: In Table 14, we examine age effects for male asylum seekers who were exposed to conflict during childhood. In column 1, we are interested in life-cycle modulations of the impact of past exposure. We interact our main explanatory variable with (mutually exclusive) decade dummies coding for the current age of the cohorts. The interaction terms, though imprecisely estimated for some age brackets, show a positive impact of past exposure on crime propensity for all brackets. The coefficients are not statistically different from each other and consequently confirm the insights of Figure 1, which suggests a stable pattern of behavioral effects of conflict exposure over the entire lifetime (with a slight decline in later ages). This result is consistent with previous findings on the persistent effects over the life cycle of adversities experienced during childhood (Gould, Lavy, and Paserman 2011; Shonkoff et al. 2012).

In the next two columns, we further assess the period of childhood during which the conflict occurred. In column 2, we add a novel variable of *post-war* exposure. The variable *born after* [0–12] takes a value of 1 if there was a civil conflict or a mass killing in the 12 years *before* birth. If the main effect of conflict exposure is about economic deprivation or institutional collapse, we should expect the variable *born after* [0–12] to be a similarly powerful predictor as our variable of interest. The results show that this is not the case, an issue we return to in Section IVD. In fact, while our main explanatory variable of past exposure retains its magnitude and statistical significance, the coefficient of *born after* [0–12] is of much smaller magnitude and is not statistically significant at conventional levels. This *sharp decrease* in crime propensity for cohorts born just after the war with respect to those exposed during their childhoods is again reassuring for our causal inference as it makes any contamination of the results by omitted variable bias unlikely. In column 3, *kid* [1–12] is split in two, with a separate variable capturing the impact of war exposure during the first five years of life, and another variable capturing war exposure between the sixth and the twelfth year of life. We observe that the effect is somewhat stronger for *kid* [1–5]. This is consistent with the literature on early child development (e.g., Heckman, Pinto, and Savelyev 2013) and with earlier studies that show the crucial

importance of these first five years of life for a large array of social and economic outcomes (Gould, Lavy, and Paserman 2011), as well as for delinquency during adulthood (see, e.g., Shaw and Gross 2008). Finally, given that in other contexts prenatal wartime famine has been linked to antisocial behavior later in life (e.g., Neugebauer, Hoek, and Susser 1999), we include a measure of prenatal exposure (i.e., *prenatal* takes the value of 1 for conflict taking place in the year of birth). We do not detect any effect of the latter in our sample (column 4).

Later Ages of Conflict Exposure: We now study the impact on criminal behavior of conflict exposure at later ages for the baseline sample of male asylum seekers. In our baseline analysis, the main explanatory variable is $kid\,[1–12]$, a binary measure of childhood exposure between ages 1 and 12. Here we expand the analysis to all potential periods of exposure over the lifetime. For every starting age $s \in [1, 30]$ and end age $e \in [s, 70]$, we construct a dummy $expo[s–e]$ that indicates whether a given cohort was exposed to a civil conflict/mass killing between the ages of s and e. For each possible combination of $[s–e]$ we estimate a variant of equation (1) where *kid* is now replaced by $expo[s–e]$; note that the set of other exposure dummies $expo\,(k)$ is adjusted accordingly such that $k \notin [s, e]$. The t-statistic of the estimated coefficient of $expo[s–e]$ is then reported in Figure 3 with different shades of gray, darker colors representing higher significance. Panel A focuses on early age exposure. The results once again confirm the crucial importance of exposure in the five earliest years of life, although we also find that exposure during early adulthood matters for violent crime later in life. Additional analyses (victimization, women) are performed in online Appendix A6.

C. *Intensity of Conflict Exposure*

Are children more likely to develop future criminal behaviors when they are exposed to more severe conflicts? Or to rampant conflicts that last for many years? The baseline analysis focuses on the effect of exposure at the extensive margin (i.e., our binary measure of exposure $kid\,[1–12]$). In this section, we look at the intensive margin and investigate whether the severity of conflict and the duration of exposure affect asylum seekers' future criminal behavior.

Severity of Conflicts: Table 15 shows the heterogeneous impact of past exposure according to the severity of the conflict in the origin country.[25] In column 1 our main explanatory variable is interacted with the inverse of the country of origin size. The reasoning here is that the UCDP/PRIO conflict threshold is defined in terms of the absolute number of fatalities and hence is likely to pick up more minor conflicts in large than in small countries. We measure size in terms of surface (km^2) and not population in order to mitigate any reverse causation bias from conflict

[25] Ideally, we would exploit both the geographical location of conflict within a country-year and place of birth to build a more accurate measure of individual exposure to violence. Unfortunately, we have neither information on the birthplace of asylum seekers nor on the within country-year location of conflict worldwide (i.e., providers of microdata such as the Armed Conflict Location and Event Data Project (ACLED) cover some but not all countries).

severity to population size.[26] The coefficient of the interaction term is positive but not significantly different from 0. In column 2 we turn to a more accurate assessment of the severity of past exposure. We construct three mutually exclusive quantiles of conflict severity, measured as the number of battle-related deaths in a given country-year weighted by the area of the country.[27] The results show that exposure to high-severity violence during childhood has a stronger impact on violent crime than exposure to medium- or low-severity violence. In the same vein, column 3 displays the effect of mass killing severity. To construct the three quantiles of severity, we rely on the country-year number of deaths index provided by the Political Instability Task Force (2016), weighted by country area.[28] Here again the coefficients are ordered very clearly: the largest impact is for highly severe mass killings, followed by events of medium severity, while we detect no impact for mass killings of low severity.

Duration of Exposure: The role played by the duration of exposure to conflict is explored in Table 16.[29] We measure this intensive margin with a new variable of interest, *nb. years of exposure,* which captures the total number of years of exposure, only during childhood in the first two columns, during all life in the remaining columns. We start with the intensive margin only (column 1), then the extensive margin is added (column 2). We replicate these specifications with the intensive margin extended to all life exposure (columns 3 and 4). The point estimates show that both margins matter but with some quantitative imbalance (6 : 1 ratio in column 4): the first year of exposure increases future crime propensity by 1.085 percentage points, any additional year increases it by 0.163 percentage points. Hence, there is evidence of some cumulative effect of longer exposure but most of the action is driven by the very fact of being exposed at least one year. We detect a similar pattern when we focus on exposure to civil conflict only or mass killings only (columns 5 to 8). However the imbalance is larger in the case of civil conflict (8 : 1 ratio) and smaller for mass killings (2 : 1 ratio), which could be related to the fact that the latter events specifically target civilian populations.

Finally, equipped with this measure of the intensive margin, we revisit the role of the timing of the first year of exposure to conflict. Figure 4 (panel A) replicates the specification of column 3 in Table 16 with the intensive margin being split into 12 different *conditional* intensive margins, i.e., [*nb. years of exposure if first exposure at age t*]; hence, there is a coefficient for each possible year of first exposure before age 12. For example, for an individual who has been exposed to a conflict from the age of 6 to 16 we code [*nb. years of exposure if first exposure at age* 6] $=$ 11, and all the other conditional margins at zero. As this exercise is demanding information-wise, we observe some variability in the coefficients.

[26] A second major concern with population is that, beyond affecting how binding a given fatality threshold is, it has been found in the literature to have a direct conflict-fueling effect (see, e.g., Fearon and Laitin 2003, Collier and Hoeffler 2004, Brückner 2010), which would make it tricky to interpret the coefficient if our main explanatory variable were to be interacted with the inverse of the population size.

[27] For an average area in the sample, low severity corresponds to less than 899 casualties by country-year, medium severity to up to 6,059 casualties, and high severity to more than 6,059 casualties.

[28] The first quantile corresponds to a range of 0 to 32,000 deaths, the second quantile up to 128,000 deaths, and the third quantile contains the rest.

[29] We use a similar approach here to that of Chetty and Hendren (2018).

In order to differentiate between the two margins, we replicate in Figure 4 (panel B) the specification of column 4 in Table 16 with the set of 12 conditional intensive margins complemented with a set of 12 conditional extensive margins, i.e., *first exposure at age t*. Here again we find an imbalance between the extensive margin and the intensive margin. The overall implication is that while the cumulative intensity may matter somewhat, clearly the really big driver of our effect is the extensive margin, i.e., being exposed versus non-exposed.

D. *Contextual Factors during Childhood*

Various potential mechanisms contribute to the causal impact of violence exposure on future crime propensity, the main ones being (i) the psychological trauma of victimization, (ii) the effects of economic deprivation during war on pervasive developmental disorders or on human capital acquisition and preferences, or (iii) societal factors such as the collapse of social capital and altered moral norms creating lasting grievances toward other groups. While challenging to completely rule out some channels and prove the predominance of others, in this section we take a first step in this direction by evaluating the relative plausibility of several mechanisms. More specifically, we examine in Table 17 the role of the socio-economic environment during childhood for conflict-exposed cohorts. To this end we control for contextual factors from the country of origin that could covary with exposure to civil conflict or mass killing during childhood. In other words, we construct average measures of these contextual factors during the childhood of cohorts (i.e., between 1 and 12 years of age). In column 1 we control for the average percentage of people with access to primary schooling in the years when the underlying individuals in our study were aged younger than 12.[30] Column 2 estimates the baseline specification on the same reduced sample of column 1, but without including the education exposure variable. The coefficient of interest of $kid\,[1\text{–}12]$ is almost identical in both columns, and the coefficient of education exposure is statistically insignificant. In columns 3 and 4, we find that while the average democracy score in the home country during childhood is not statistically significant, the coefficient of conflict exposure remains significant and of similar magnitude with and without the democracy exposure control. In columns 5 to 6 we focus on GDP per capita, but detect no impact of poverty during childhood. However, the coefficient of exposure to conflict remains positive but loses its statistical significance due to the sample size reduction. We reach similar conclusions in column 7 where the three childhood contextual factors are simultaneously included.

Overall, our results suggest that it is unlikely that the mechanism at work is purely based on human capital depletion, economic deprivation, or institutional collapse. Note that the scope of this exercise is constrained by the limited data availability of cohort-level characteristics. However, this problem is alleviated in the next section

[30] Note that the available education indicator used here is somewhat coarse: it corresponds to the percentage of the population aged above 15 with at least a primary education. While it captures the state of the education system in a given year, it is not a specific, precise measure of education exposure of the cohort being of schooling age, but also reflects the education of older generations.

by the inclusion of cohort-specific fixed effects in the bilateral crime regressions that absorb all the (observed and unobserved) heterogeneity.

V. Bilateral Crime Regressions and Targeted Violence

In this section we exploit a unique feature of our dataset on criminality in Switzerland, namely information on the nationalities of both perpetrators *and* victims. We use this information to build the bilateral crime propensity, $CP_{n,a,v,t}$, defined as the cohort-level share of individuals of nationality n and age a who perpetrate at least one violent crime in year t against victims from a specific nationality v.[31] Crucially, this enables the inclusion of cohort fixed effects, resulting in the inference being based purely on bilateral characteristics, an approach grounded in the gravity trade literature. Hence, all the cohort-specific unobserved heterogeneity is now absorbed by the fixed effects. As shown below, our main result is that, everything else equal, the likelihood that perpetrators and victims are co-nationals is much larger among asylum seekers who have been exposed to civil conflict or mass killing during childhood. A possible interpretation is that inter-group hostilities persist over time and transit to Switzerland.

When building bilateral crime propensities, we continue to assess cohorts of asylum seekers on the perpetrator side. In line with our previous analysis, we restrict our main estimates to the sample of adult male cohorts from countries that experienced at least one conflict since 1946. For potential victims, we pool together all nationals of a given country living in Switzerland, independent of their gender, age, or status (i.e., asylum seekers, migrants, natives). Our cohort-level sample is crossed with the nationality of victims such that the unit of observation is now a cohort of perpetrators of nationality (n) × age group (a) × year (t) targeting victims of nationality (v). We estimate the following bilateral version of crime equation (1):

$$(3)\qquad CP_{n,a,v,t} = \alpha_0 \times \mathbf{1}_{n=v} + \alpha_1 \times \left(kid[1\text{–}12]_{n,a,t} \times \mathbf{1}_{n=v}\right) + \sum_{k=13}^{80+} \beta(k) \times \left(expo(k)_{n,a,t} \times \mathbf{1}_{n=v}\right) + \mathbf{FE}_{n,a,t} + \mathbf{FE}_{v,t} + \varepsilon_{n,a,v,t},$$

where $CP_{n,a,v,t}$ corresponds to bilateral propensity to violent crime and $\mathbf{1}_{n=v}$ is a binary indicator function equal to 1 if perpetrator and victim are co-nationals. Our focus here is on the interaction term $kid[1\text{–}12]_{n,a,t} \times \mathbf{1}_{n=v}$. Finding a positive coefficient α_1 means that there is an over-propensity to target co-nationals for cohorts exposed during childhood. Note that the variables coding for past exposure at later age, $expo(k)_{n,a,t}$, are also interacted.

Importantly, the bilateral nature of the data allows us to go beyond the battery of fixed effects already present in our benchmark crime regression (1). In fact, in addition to including $\mathbf{FE}_{v,t}$, which are fixed effects in the nationality of victim × year

[31] Given that our crime data do not contain any information on the ethnic group of the perpetrator or victim of a crime, we focus on the general crime propensity among co-nationals, without being able to construct an inter-ethnic crime propensity.

Table 8—Bilateral Crime Regressions

Dependent variable:	Violent crime propensity					
Exposure to:	CC, MK (1)	CC (2)	MK (3)	CC, MK (4)	CC (5)	MK (6)
$\mathbf{1}_{n=v}$	1.223	1.197	1.746	1.265	1.237	1.814
	(0.262)	(0.259)	(0.251)	(0.296)	(0.293)	(0.314)
$kid\,[1–12]_{n,a,t} \times \mathbf{1}_{n=v}$	1.449	1.551	1.989	1.437	1.541	1.975
	(0.518)	(0.527)	(0.733)	(0.538)	(0.547)	(0.783)
$\mathbf{1}_{v=CH}$				1.761	1.713	2.516
				(0.182)	(0.179)	(0.238)
$kid\,[1–12]_{n,a,t} \times \mathbf{1}_{v=CH}$				0.709	0.834	−0.395
				(0.257)	(0.262)	(0.399)
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^2	0.035	0.035	0.043	0.033	0.034	0.040
Sample mean (bilateral crime propensity)	0.041	0.041	0.047	0.041	0.041	0.047

Notes: Ordinary least squares (OLS) estimations based on the sample of male adult cohorts of asylum seekers. Robust standard errors are two-way clustered at the nationality of perpetrator and victim levels. The dependent variable *Violent crime propensity* corresponds to bilateral propensity to violent crime. $\mathbf{1}_{n=v}$ is a binary indicator function equal to 1 if the perpetrator and the victim are co-nationals. $\mathbf{1}_{v=CH}$ is a binary indicator function equal to 1 if the victim is a native. $kid\,[1–12]$ 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 later ages $k \in \{13, 14, 15, \ldots, 80+\}$ interacted with $\mathbf{1}_{n=v}$. 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.

dimension, we can now also include cohort-fixed effects for perpetrators, $\mathbf{FE}_{n,a,t}$.[32] This is possible because, for a given cohort of perpetrator (n, a, t), we observe variations in bilateral crime propensities across nationalities of victims. Finally, standard errors are two-way clustered at the nationality of perpetrator level and at the nationality of victim level.

Main Results on Targeted Violence: Table 8 displays the estimates of equation (3) for the cohorts of perpetrators composed of adult male asylum seekers. Columns 1 and 4 pool exposure to civil conflict together with exposure to mass killing, columns 2 and 5 focus only on civil conflict, while columns 3 and 6 only on mass killing. In columns 1–3 we include the full battery of fixed effects described above, i.e., $\mathbf{FE}_{v,t}$ and $\mathbf{FE}_{n,a,t}$ and we report the coefficients of the aforementioned variable $\mathbf{1}_{n=v}$ and its interaction with childhood exposure, $kid\,[1–12]_{n,a,t} \times \mathbf{1}_{n=v}$. Column 1 shows that co-nationals are much more likely to be in a perpetrator-victim relationship. Indeed the coefficient of $\mathbf{1}_{n=v}$ is not only positive but its magnitude is very large: the premium attached to co-nationality is equal to 1.22 percentage points, representing 30 times the sample mean of bilateral crime propensity (equal to 0.041 percentage points). Our interpretation is that co-nationals interact more frequently (due, for example, to shared language or overlapping social networks), which increases the potential for disputes. Moreover, the detection probability is likely to be higher for

[32] Note that the linear terms $kid\,[1–12]$ and $expo(k)$ are absorbed by the cohort fixed-effect $\mathbf{FE}_{n,a,t}$.

crimes between co-nationals (perhaps because the victim and perpetrator know each other). More importantly for our research question, the coefficient of the interaction term is positive and sizable. Quantitatively, the already large premium attached to co-nationality increases by 118 percent (i.e., 1.449/1.223) when a given cohort of potential perpetrators has experienced conflict during childhood. Columns 2 and 3 confirm that the previous pattern is also detected when focusing only on civil conflict or only on mass killing.

Together, these findings show that exposed individuals are much more violent toward their own community than are individuals born after a civil conflict/mass killing. However, an open question remains as to whether this pattern is due to the fact that past exposure to conflict makes people more crime-prone in general or whether the conflict-exposed individuals are more violent specifically toward their own community. Certainly, knowing whether violent criminality is confined to foreign communities has policy implications for receiving countries. We deal with this question in columns 4 to 6 by comparing violence toward co-nationals with violence toward natives. To this end, we include in equation (3) a binary variable, $\mathbf{1}_{v=CH}$, indicating whether the victim is a native, together with its interaction term $kid\,[1\text{–}12]_{n,a,t} \times \mathbf{1}_{v=CH}$. The estimation of the linear term $\mathbf{1}_{v=CH}$ forces us to remove the victim fixed effects $\mathbf{FE}_{v,t}$.[33] In column 4 the positive coefficient of $\mathbf{1}_{v=CH}$ is quite sizable with respect to the bilateral sample mean (1.76 versus 0.041) showing that, on top of their co-nationals, perpetrators are more likely to target natives than nationals from third countries. Here again this likely reflects more frequent social interactions between asylum seekers and natives. The positive coefficient on the interaction term shows that exposed individuals are 41 percent more violent toward natives than are individuals born after a conflict (i.e., 0.709/1.761). Interestingly, the magnitude of the effect is very close to our baseline quantification (column 2, Table 5, panel A) based on a sample where all victim nationalities are pooled together. By comparison, violence of asylum seekers toward their own community is similar to the results of column 1: exposed individuals are 114 percent more violent toward their co-nationals than are individuals born after a conflict (i.e., 1.437/1.265). Note that the large sample size leads to coefficients that are precisely estimated, making between-coefficient comparison relatively reliable. In conclusion, although past conflict exposure makes individuals generally more crime-prone toward Swiss natives, we see that this premium almost triples for crimes targeting their own community (from 41 percent to 114 percent). This suggests that the greater propensity to violent crime of asylum seekers exposed to conflict during childhood is mostly confined to their national community, in line with the theories discussed below on persistence of civil conflicts and the depletion of social capital.[34] We detect a similar pattern for civil conflict (column 5) and mass killing (column 6) (although exposure to mass killing does not seem to increase violence

[33] Note that the residual category $\mathbf{1}_{v\notin\{n,CH\}}$ consists of nationalities of victims that are neither co-nationals nor natives. This linear term, together with its interaction with $kid\,[1\text{–}12]_{n,a,t}$, are not added due to collinearity with the cohort-fixed effects for perpetrators.

[34] On a related note, it is striking how low the detected effects are for crimes committed against natives: the coefficient of $\mathbf{1}_{v=CH}$ is of a similar order of magnitude as that of $\mathbf{1}_{n=v}$, meaning that asylum seekers are similarly likely to commit a crime against co-nationals (which may, in most cases, represent a tiny share of total population) as against Swiss natives (amounting to roughly three-quarters of the population). This finding complements the unconditional evidence presented in Section IIC that only one-third of violence perpetrated by asylum seekers involves natives.

toward natives). We replicate the full analysis after excluding intra-family crimes in the construction of bilateral crime propensities. The results remain unchanged, indicating that the observed crime over-propensity toward co-nationals is not driven by domestic violence (online Appendix Table A9.25).

Both the signs and the magnitude of the coefficients can be interpreted as evidence of persistence in targeted violence. Note that (un)observed characteristics of conflict-exposed cohorts are filtered out by the nationality of perpetrator $\times$ age $\times$ year fixed effects. Thus, the results are not driven by channels such as conflict-induced selection into migration, post-conflict educational dropout, pervasive developmental disorders, or changes in the composition of the population. In contrast, theories stressing that past conflicts damage social ties between co-nationals and that distrust and grievances acquired in early childhood persist over the lifetime are consistent with our findings. In particular, Rohner, Thoenig, and Zilibotti (2013a) and Acemoglu and Wolitzki (2014) argue that war leads to a collapse of inter-ethnic trust, in turn sowing the seeds for more ethnic inter-group conflict in the future. Taking this theory literally, one should not only expect a general tendency of war-exposed individuals to commit more crimes in post-war contexts but, on top of this, to be frequently involved in committing crimes that target co-nationals. To this regard, Roth (2009) gives numerous examples of murderous riots between Northern Irish and Catholic Irish emigrants in the United States. As far as Switzerland is concerned, for the diaspora of migrants from Sri Lanka living in Switzerland many incidents of "imported conflicts" between different ethnic political movements have been documented (Moret, Efionayi, and Stants 2007).

Additional Results and Robustness Checks: A series of additional results can be found in online Appendix A9, where we replicate columns 1–3 of Table 8 for property crimes instead of violent crimes; by category of victims (asylum seekers; Swiss natives; economic migrants); on the sample of female perpetrators; with an augmented battery of fixed effects that include bilateral fixed effects. We also study exposure to military inter-state disputes (MID) and find some evidence that targeted violence is observed in the case of extra-territorial conflicts.

VI. Combating the Legacy of Conflict: The Role of Policies

In this section, we study how institutions in the host country modulate the impact of past exposure to conflict on current criminality of asylum seekers. Our question of interest is whether the "right" design of institutions and policies can partly or fully alleviate the risk of increased criminality for exposed individuals.

The reference model of crime is that proposed by Becker (1968): the decision to commit a crime is driven by the opportunity cost (legal labor market salary) relative to returns to crime, discounted by the probability of being caught by the police and the sanction imposed by the criminal justice system. This framework is flexible enough to also accommodate psychological and social costs and gains from crime. For example, inter-group grievances may reduce the psychological costs of a crime directed against a member of the disliked group. However, economic incentives still matter. For example, say lower crime costs due to inter-group grievances can be offset by higher opportunity costs of crime due to better economic integration.

Following this Beckerian logic, in what follows we examine integration policies that are likely to shape asylum seekers' incentives and criminal behaviors. We focus on institutional arrangements related to their economic and social integration, which are decentralized and mainly set by cantons. In online Appendix A10.6, we complement this analysis by investigating the role played by the stringency of the asylum policy, which is harmonized at the federal level but varies across nationalities. Note that as integration policies are not randomized, this exercise cannot go much beyond correlations. It is, of course, entirely possible that the local environment has a causal impact, but the absence of randomization makes it challenging to rule out all possible confounders. Though limited, this preliminary evidence is, to our knowledge, new to the literature and fills a gap by documenting how public policies can tackle the recurrence of violence in the aftermath of conflict.

Cross-Canton Variations: The federalist constitution of Switzerland guarantees substantial autonomy to the cantons, which results in cross-canton heterogeneity of economic and social integration policies. Because asylum seekers are allocated early on by the federal state to different cantons, their incentives for engaging in crime are consequently shaped by local policies. Thus, a first step in our analysis consists of assessing whether the greater crime propensity of individuals exposed to conflict during childhood varies substantially across cantons. We show below that the answer is a clear yes. We then study the explanatory power of economic and social integration policies in shaping this cross-cantonal pattern.

We implement a two-step procedure in the spirit of Bertrand and Schoar (2003) and Bandiera, Prat, and Valletti (2009). First, we estimate the violent crime premium (i.e., the coefficient of $kid\,[1–12]$) separately for each canton, then we regress those canton-specific premiums on various canton-level characteristics that include policy variables. The specification for the first step is a canton-level version of the baseline equation (1) with flexible coefficients for the variable $kid\,[1–12]$:

$$(4)\quad CP_{n,a,t,c} = \beta_c \times kid\,[1–12]_{n,a,t} + \sum_{k=13}^{80+} \gamma(k) \times expo(k)_{n,a,t} + \mathbf{FE}_{c,t} + \mathbf{FE}_{n,t} + \mathbf{FE}_{a} + \varepsilon_{n,a,t,c},$$

where $\mathbf{FE}_{c,t}$ are canton $\times$ year fixed effects. The inclusion of this additional battery of fixed effects filters out all the canton-level factors susceptible to affecting the absolute level of criminality. Hence, identification of the violent crime premiums relies on within-canton, within-nationality, between-cohort variations in crime propensity. The previous equation is estimated on the baseline sample of adult males (column 2, Table 5, panel A), disaggregated at the canton level (Table A5.13). The variable $kid\,[1–12]$ encompasses childhood exposure to civil conflict or mass killing. Robust standard errors are two-way clustered at the nationality and canton-year levels.

Our interest lies in the vector of 26 canton-specific effects of childhood conflict exposure on future violent crime propensity, namely the point estimates $\hat{\beta}_c$ in equation (4). Figure 2 displays graphically these estimates (baseline sample, 2009–2016) together with $+/-1$ standard error. A visual inspection reveals striking variations

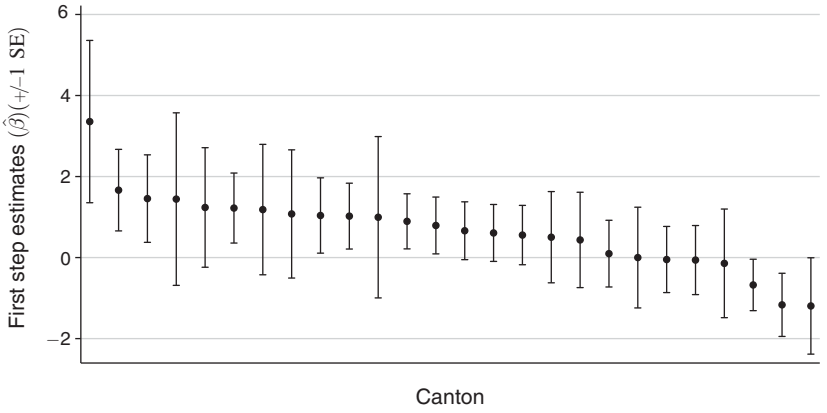

FIGURE 2. THE ESTIMATED COEFFICIENTS OF *kid*[1–12] ACROSS CANTONS

Note: The 26 points correspond to the $\hat{\beta}_c$ coefficients of the first step estimation for the 26 cantons (estimated on the sample of adult male asylum seekers for the 2009–2016 period), ordered by magnitude. The vertical lines represent $+/-1$ standard error.

across cantons, and this impression is confirmed quantitatively as the relative standard deviation of the distribution of $\hat{\beta}_c$ is equal to $0.947/0.612 = 1.547$, indicating a large dispersion. This suggests that contextual factors at the canton-level may substantially modulate the violent crime premium of individuals exposed to conflict during childhood. Likely factors include the design of integration policies, local economic conditions (GDP, unemployment), and political and cultural attitudes toward refugees and foreigners.[35] We next endeavor to assess the specific role of policies, controlling for the other factors. This leads to the following second-step equation:

$$\hat{\beta}_c = \alpha \times Policy_c + \mathbf{X}_c'\beta + \varepsilon_c, \tag{5}$$

with $\mathbf{X}_c$ representing canton-level controls. This equation captures only how canton-level policies affect the over-crime propensity of conflict-exposed individuals compared to non-exposed individuals (the *absolute* level of criminality being absorbed by the canton $\times$ year fixed effects in the first-step equation (4)). Hence, our two-step approach is not aimed at estimating how policies reduce crime propensities of all asylum seekers.[36]

[35] Our empirical findings below show that among these contextual factors, policies matter more than GDP or culture. Indeed the (unreported) OLS coefficients of the control variables in Tables 9 and 10 are quantitatively small.

[36] Addressing this question raises empirical challenges that go beyond the scope of this paper. We nonetheless make a first attempt by directly estimating equation (5) with canton-specific average crime propensity as dependent variable and by replicating all the specifications in Tables 9 and 10 with this new variable. The estimates show that no statistical effect of our two policy variables (*open job access* and *civic courses*, see below) on canton-level criminality is detected (see online Appendix A10.3 for more details). This finding suggests that policies are effective only for those who were conflict-exposed during childhood but not for all asylum seekers. Note however that we are extremely cautious in the interpretation of this non-result due to the impossibility of filtering out for canton $\times$ year fixed effects in the first-step equation (4), making the dependent variable very noisy and raising concerns relative to omitted variable bias in the estimation procedure of equation (5).

Given that we know the precision with which each cantonal coefficient $\hat{\beta}_c$ is estimated, the previous equation can be estimated using generalized least squares (GLS) technique. We account for the measurement error of the left-hand-side variable by weighting each observation by the inverse of the standard errors estimated in the first stage. Note that, with 26 observations, the small sample size makes the detection of any statistical effect challenging. We believe that the large dispersion in the distribution of $\hat{\beta}_c$ partially compensates for this drawback. Moreover, policy adoption at the cantonal level is not random, and the consequential problem of selection on observables is difficult to address given the limited number of degrees of freedoms (and thus the restricted set of controls we can account for). We circumvent this issue by making use of various propensity score matching methods (see below). Finally, the vector of control variables $\mathbf{X}_c$ consists of (i) pre-sample cantonal GDP per capita; (ii) pre-sample cantonal unemployment rates; (iii) difference in employment rates of second generation migrants with respect to non-migrant population at the canton level; (iv) average percentage of citizens in a canton voting in the years 1991–2007 for the main right-wing party, the Swiss People's Party (i.e., SVP/UDC)[37]; and (v) a dummy taking a value of 1 for (majority) French-speaking cantons (which have traditionally taken a more pro-migration stand). Detailed descriptions and summary statistics of all variables can be found in online Appendix A10.

Labor Market Integration: We start with policies related to the labor market integration of asylum seekers, which may affect the opportunity cost of engaging in criminal activity.[38] This factor is particularly salient for asylum seekers, who are often severely cash constrained and indebted from financing their travel to Europe. Besides altering the opportunity cost of crime, there is also a mechanical effect of hours spent working being time not dedicated to alternative, potentially illegal, activities.

Our main variable coding for labor market integration is *open job access*. This binary variable is equal to 1 for cantons where asylum seekers can start working in all sectors of activity three months after arrival; throughout Switzerland, federal law bans working for the first three months. It is equal to 0 in cantons where the cantonal administration (i) extends the working ban beyond three months, or (ii) restricts work carried out by asylum seekers to certain sectors, or (iii) actively discourages their access to jobs.[39] The coding of this variable is based on official documentation of the highest inter-cantonal authority (*Kantonsdirektorenkonferenz*), covering respectively the years 2011 and 2016. In 25 out of 26 cantons, the approach is the same in both years, consistent with our goal of capturing stable labor market policies over

[37] This party has, since the early 1990s, taken a very firm position on limiting immigration into Switzerland. This measure is missing for two small Swiss cantons (Nidwalden, Obwalden) where for a part of the period under study there were respectively single uncontested candidates and hence no elections took place.

[38] This issue has received considerable attention in the press and has been the subject of much public debate (see, e.g., *New York Times*, September 18, 2015, "Germany Works to Get Migrants Jobs," and *The Economist*, August 27, 2015, "Let Them in and Let Them Earn"). The academic literature on crime has found that labor market access critically affects criminal behavior (see, e.g., Freeman 1999; Gould, Weinberg, and Mustard 2002; Fougère, Kramarz, and Pouget 2009; Bell, Fasani, and Machin 2013).

[39] The legal framework relative to labor market integration is further discussed in online Appendix A2.

Table 9—The Impact of Open Job Access

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
	(1)	(2)	(3)	(4)	(5)	(6)
open job access	−0.640	−0.762	−0.625	−0.644	−0.419	−0.748
	(0.366)	(0.315)	(0.320)	(0.326)	(0.281)	(0.305)
Number of cantons	25	23	23	23	23	23
R^2	0.246	0.526	0.598	0.471	0.491	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 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 3 months after 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.

the entire time period (see details in online Appendix A10.1).[40] We exclude from the sample the only canton that changed its regulations (i.e., Appenzell-Innerrhoden, which incidentally is the smallest Swiss canton, accounting for roughly 0.26 percent of the asylum seekers over the period).[41] Our construction of the variable *open job access* deliberately emphasizes the *de jure* dimension of labor market participation. By contrast, the sensitivity analysis in online Appendix A10.3 also looks at *de facto* participation, or actual employment rates (which arguably are endogenous to the characteristics of the pool of asylum seekers considered). Importantly, as shown in a recent paper by Slotwinski, Stutzer, and Uhlig (2018), cantonal differences in labor market access regulations are a strong determinant of actual employment rates in Switzerland.

Table 9 reports the estimates of equation (5) with *open job access* as our policy variable. We restrict the sample used for the first-step estimation (equation (4)) to years for which our official sources provide reliable information, i.e., the 2011–2016 period (column 1). The coefficient of interest is statistically significant and has the expected negative sign, indicating that cantons granting labor market access to asylum seekers experience a lower effect on the violent crime propensity of conflict-exposed individuals. To alleviate concerns about omitted variable bias, we include in column 2 the battery of control variables mentioned above. The sample size decreases by 2 cantons (out of 26) due to missing information. Still, the coefficient is even more precisely estimated, with a magnitude comparable to its value in column 1. Restricting the sample to 2011–2016 is somewhat conservative, as the striking persistence of job market policies over this period may well extend further back in time. In column 3 we consequently replicate the previous specification with a first-step estimation based on the full period, 2009–2016. The point estimate is quantitatively similar.

As discussed above, the small sample size limits the number of included variables that control for endogenous selection of policy. However, selection does not seem to

[40] In 16 out of these 25 cantons the variable *open job access* takes a value of 1, while it equals 0 in the remaining 9 cantons.

[41] When we include Appenzell-Innerrhoden we get similar results. Whichever way we code it (as 0 or 1), the coefficient of interest has a negative sign and is statistically significant, as shown in online Appendix Table A10.36.

be a first-order issue in our data. Indeed, applying the methodology of Oster (2017) suggests that our results, if anything, underestimate the true effect. Nevertheless, in columns 4 to 6 we use a complementary approach based on propensity score matching (PSM) in order to make the samples of treated cantons (*open job access* $= 1$) and untreated observations (*open job access* $= 0$) more comparable. This provides a flexible way to allow various factors to affect the treatment (Imbens and Rubin 2015). To compute the propensity scores, we consider a Probit estimator together with linear, higher order, and interactive combinations within the set of control variables used above. We apply the approach of Imbens and Rubin (2015) for selecting the combination of predictors for the propensity score model. Based on the canton-specific estimates of the propensity scores (*ps*), we perform three standard exercises: (i) we directly include *ps* as a control variable in equation (5) (column 4); (ii) we implement a method of inverse probability weighting following Abadie and Cattaneo (2018) (column 5)[42]; (iii) we display the PSM estimates of the average treatment effect using nearest neighbor matching (with replacement, which is well suited, given the small sample size) (column 6).[43]

The coefficient of *open job access* in Table 9 displays a consistent negative pattern throughout the specifications.[44] It is both precisely estimated and stable in terms of magnitude. This supports the view that the labor market integration of asylum seekers mitigates the detrimental effect of childhood exposure to conflict on future violent criminality. Quantitatively, this mitigating effect is substantial. When excluding the two extreme point estimates, we see that the point estimate of *open job access* is on average equal to -0.66, showing that labor market integration reduces the violent crime premium by two-thirds with respect to its baseline level (equal to 1.08 in column 2, Table 5, panel A). We can also compute the counterfactual change in criminal behavior induced by a more/less lenient labor market policy. Recall that the overall effect revealed by the baseline estimates is that the effect of childhood exposure corresponds to 35 percent of the sample mean. The overall effect corresponds to the current situation: in 16 cantons *open job access* is present, in the remaining 9 it is absent. In contrast, under a scenario where (as it is currently the case in Germany and France) labor market access would be denied to asylum seekers across the Swiss territory (i.e., with the 16 cantons switching their policy to no job access), this number would go up to 48 percent. In other words, the crime propensity of conflict-exposed cohorts would be 48 percent larger than the propensity of the average cohort. Alternatively, under the opposite scenario where labor market access would be granted in all cantons (i.e., with 9 cantons granting open job access), this figure would drop to 25 percent.

Social Integration: Labor market integration is intertwined with social integration, and one can expect social integration to directly reduce the crime risk by increasing

[42] This estimator uses $1/ps$ to weight observations in the treatment group and $1/(1 - ps)$ to weight control group observations.

[43] A fourth exercise consists of trimming the sample by removing all observations that are off common support. While we do this, in fact, for other variables, it turns out that for *open job access* all observations are on support, such that trimming would result in the identical sample as used in column 3.

[44] In online Appendix A10.3 we report further methodological aspects of PSM (diagnostic tests, estimation, and matching methods), investigate the potential impact of outliers (Table A10.36), and use alternative measures of labor market integration (Tables A10.37 and A10.38).

the opportunity cost of unlawful behavior, as well as indirectly by improving labor market outcomes. In what follows, we investigate the impact on violent criminal behavior of civic courses targeting asylum seekers and transmitting general knowledge about Swiss history, social norms, and civic culture. Our measure, *civic courses*, is a binary variable, taking a value of 1 if a canton offers such courses for asylum seekers, and 0 otherwise. The information source is a retrospective survey conducted in 2011 among civil servants in 22 cantons by Wichmann et al. (2011).[45] Additional details are provided in online Appendix A10. 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. This result leads us to assess the effect of *civic courses* on violent crime premiums independently of *open job access*.[46] Table 10 displays the estimation results of equation (5) with *civic courses* as policy variable. Given that information on this policy is only available for 2011, we restrict the first-step estimation sample to the years close to the time of the survey (2009–2012) as a compromise between data availability and sample size reduction. Only the last specification (column 7) extends the sample to the full period 2009–2016. This last exercise has the virtue of testing the robustness of our results, but must be interpreted with caution as there is no guarantee that the policy variable remains stable in all cantons after 2012. In column 1 the OLS estimate of the coefficient of interest is negative and precisely estimated; it shows that *civic courses* tend to reduce the deleterious impact of childhood exposure to conflict on crime propensity. The inclusion of our set of canton-specific controls leaves the magnitude of the coefficient similar (column 2). Here again the methodology of Oster (2017) indicates that endogenous selection of integration policy is not a major issue in our context. As above, in Table 10 we once again employ PSM methods to make the sample of treated cantons more comparable to that of the non-treated cantons. Using the estimated propensity scores, four exercises are performed: (i) we trim the sample, removing all observations that are off common support (column 3); (ii) we include the propensity score a control variable (column 4); (iii) we perform an inverse probability weighting method (column 5); (iv) we report the PSM estimate (column 6); and replicate the previous one with the full period sample (column 7). All in all, our coefficient of interest remains negative, stable across specifications, and precisely estimated.[47] Quantitatively, the effect is sizable and comparable to that of labor market access.

VII. Economic Migrants

For the purpose of identification, our empirical analysis has thus far focused on asylum seekers as this category of migrants is exogenously allocated across cantons.

[45] The variable *civic courses* takes a value of 1 in 9 cantons, and 0 in the remaining 13 cantons.

[46] The joint estimation of the effect of *open job access* and *civic courses* is statistically challenging for two reasons. Firstly, due to data limitations, the former variable is accurately measured over the 2011–2016 period while the latter one is defined over the 2009–2012 period. Secondly, the implementation of PSM methods with multiple treatments is constrained by the small sample size. Nevertheless, in online Appendix A10.5, we circumvent these difficulties by replicating both Tables 9 and 10 with the other policy variable directly included as a control (either as covariate in OLS specifications, or as a predictor in the propensity score model). This demanding approach yields point estimates that are quantitatively comparable to the results based on independent estimation of the policy variables.

[47] In online Appendix A10.4 we discuss the methodological aspects of the PSM, perform an outlier analysis (Table A10.43), and present further results on variables linked to social integration policies, such as the provision of

Table 10—The Impact of Civic Courses

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
	(1)	(2)	(3)	(4)	(5)	(6)	
civic courses	−1.083	−0.997	−1.324	−1.165	−0.889	−1.183	−0.851
	(0.555)	(0.595)	(0.664)	(0.605)	(0.553)	(0.506)	(0.283)
Number of cantons	22	21	19	22	22	21	22
R^2	0.322	0.500	0.521	0.327	0.317	N/A	N/A

Notes: 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). *Civic courses* is a binary variable, taking a value of 1 if a canton offers courses on general Swiss history, social norms, and civic culture targeted at asylum seekers, and zero otherwise. Standard errors in parentheses.

As shown in the previous section, the integration of asylum seekers in the labor market dampens the impact of childhood exposure to conflict on crime propensity later in life. It remains, however, an open question as to whether this beneficial effect of integration is also present for a larger sample of migrants with a more complete and longer-run economic integration. We replicate our baseline analysis for the sample of all economic migrants in an effort to address this question.[48] Working from a Beckerian framework, one would expect economic migrants to have comparable social costs and benefits of crime as asylum seekers, but to face much more substantial economic costs due to their better integration in the labor market. We consequently expect these differential incentives to translate into a smaller violent crime premium of conflict exposure for economic migrants compared to for asylum seekers.

The group of migrants holding B or C permits represented 24 percent of the total Swiss population in 2016 (2 million out of 8.3 million people), which makes our analysis on this sample a particularly powerful external validity check.[49] Economic migrants are not allocated exogenously across cantons; they are completely free to settle wherever they prefer in Switzerland, i.e., crime-prone individuals can self-select into crime-facilitating cantons. To alleviate this concern, we include canton × year fixed effects in our baseline equation (1), requiring the use of the cohort-level sample disaggregated at the canton level. Hence, the dependent variable becomes $CP_{c,n,a,t}$, the crime propensity of cohort n, a, t in canton c.

language courses (Table A10.44), the type of management of asylum centers (private versus public) (Table A10.45), and the amount of financial social assistance (Table A10.46). For all of these additional policy measures we do not detect any statistically significant effect.

[48] Economic migrants have typically lived in Switzerland for many years, benefit from the long-run perspective to stay in Switzerland and are, unsurprisingly, on average much better integrated into the Swiss labor market. Employment rates for our sample of asylum seekers range from below 1 percent for the canton of Appenzell-Innerrhoden to slightly above 20 percent for Graubuenden, while the average employment rate of economic migrants lies around 80 percent, similar to that of the natives (Federal Statistical Office 2017).

[49] As described in Section II, B permit holders are those resident foreign nationals with a valid employment contract. This permit is issued for five years and is renewable. C permit holders are those settled foreign nationals who have been in Switzerland for at least five years and their right to settle in the country is not subject to any time restrictions or conditions. The B permit holders make up 8 percent, and the C permit holders 16 percent, of the total population. Source: Statistics on Foreigners and Asylum Seekers (2017, p. 13).

Table 11—Cohorts of Economic Migrants: Summary Statistics

	Men				Women			
Variable	Mean	Standard deviation	Min.	Max.	Mean	Standard deviation	Min.	Max.
Cohort size (number of individuals)	16.06	56.35	1	1,830	12.88	43.19	1	1,650
History of civil conflict or mass killing (%)	57.80	49.39	0	100	56.77	49.54	0	100
History of civil conflict (%)	58.51	49.27	0	100	57.20	49.48	0	100
History of mass killing (%)	20.05	40.03	0	100	18.95	39.19	0	100
kid [1–12] (CC or MK, %)	30.38	45.99	0	100	29.27	45.50	0	100
kid [1–12] (CC, %)	29.42	45.57	0	100	28.19	44.99	0	100
kid [1–12] (MK, %)	9.20	28.90	0	100	8.40	27.74	0	100
Violent crime propensity ($CP_{n,a,t}$, %)	1.15	8.13	0	100	0.26	3.79	0	100

Notes: All summary statistics are based on 357,661 cohorts of males and 375,915 cohorts of females aged at least 18. Abbreviations CC and MK refer respectively to civil conflict and mass killing.

In our sample, we observe a yearly average of around 718,000 adult male economic migrants and 605,000 adult female economic migrants over the 2009–2016 period. After aggregating by nationality n and age a, for each year t and each canton c, this leaves us with a sample of 357,661 adult male cohorts (83 age categories from 120 countries) and 375,915 adult female cohorts (83 age categories from 121 countries), which are our units of observation. Table 11 reports the main descriptive statistics for the cohorts of economic migrants. The share of male cohorts who originate from countries that have experienced at least one episode of civil conflict or mass killing since 1946 is much lower than for asylum seekers (58 percent versus 92 percent). The sample mean of conflict exposure during childhood, *kid* [1–12], is also lower for economic migrants, 30 percent versus 51 percent for asylum seekers. Finally, economic migrants are also on average less violent: the sample mean of their violent crime propensity is equal to 1.15 percent, roughly one-third of the average crime propensity of asylum seekers. This is in line with the results of Section VI on the crime-reducing impact of labor market access.

Baseline Results (Economic Migrants): Table 12 replicates a canton-level version of the baseline Table 5 with the sample of economic migrants. All specifications include age group, canton × year fixed effects, nationality × year fixed effects, and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, \ldots, 80+\}$. We report only our coefficient of interest that captures the violent crime over-propensity of cohorts exposed to civil war or mass killing during childhood (1–12 years), the reference group being cohorts born after conflict. Throughout all the columns, the coefficient of *kid* [1–12] is positive but less precisely estimated than its baseline counterpart for asylum seekers, even for the sample of men (e.g., the p-value is 0.17 in column 2 and 0.12 in column 3), likely due to measurement errors on childhood exposure of economic migrants.[50] More

[50] Note that a substantial share of economic migrants (holding B and C permits) are actually second- (or even third-) generation migrants and were born in Switzerland (true of 21 percent of the migrants for whom we have information on birth place). This aspect creates substantial statistical noise in the sample of economic migrants when defining the measure of exposure to conflict during childhood. In contrast, this measurement error is not present for asylum seekers, as hardly any of them grew up in Switzerland.

Table 12—Violent Crime Propensity and Conflict Exposure: Economic Migrants

Dependent variable:	Violent crime propensity			
Sample:	Full	CC and MK	CC	MK
	(1)	(2)	(3)	(4)
Panel A. Men				
kid [1–12]	0.307	0.280		
	(0.227)	(0.205)		
kid [1–12] (*only CC*)			0.304	
			(0.198)	
kid [1–12] (*only MK*)				0.781
				(0.722)
Age FE	Yes	Yes	Yes	Yes
Canton × year FE	Yes	Yes	Yes	Yes
Nationality × year FE	Yes	Yes	Yes	Yes
Observations	357,661	206,725	201,365	71,615
R^2	0.061	0.064	0.065	0.090
Sample mean (crime propensity)	1.15	1.64	1.67	2.65
	(5)	(6)	(7)	(8)
Panel B. Women				
kid [1–12]	0.028	0.009		
	(0.040)	(0.041)		
kid [1–12] (*only CC*)			0.025	
			(0.040)	
kid [1–12] (*only MK*)				−0.030
				(0.068)
Age FE	Yes	Yes	Yes	Yes
Canton × year FE	Yes	Yes	Yes	Yes
Nationality × year FE	Yes	Yes	Yes	Yes
Observations	375,915	213,400	206,504	71,117
R^2	0.012	0.013	0.013	0.017
Sample mean (crime propensity)	0.26	0.35	0.36	0.47

Notes: OLS estimations based on the sample of adult cohorts of economic migrants (B and C permits). 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 killing (columns 1 to 2), to civil conflict only (column 3), or to mass killing 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, canton × year fixed effects, nationality × year fixed effects, and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, \ldots, 80+\}$. 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.

importantly, the coefficient of interest is smaller in size than its counterpart for asylum seekers, both in absolute and relative terms. In our preferred specification of column 2 the (statistically non-significant) coefficient is equal to 0.28, which is indeed much smaller than its counterpart (i.e., 1.08) in the baseline Table 5. In addition, the ratio of the coefficient with respect to the sample mean of crime propensity is smaller for economic migrants (0.28/1.64 versus 1.08/3.09 for asylum seekers). In other words, cohorts of economic migrants exposed to civil conflict/mass killing during childhood are 17 percent more prone to violent crimes than the average cohort of economic migrants; this figure amounts to 35 percent for asylum seekers. This suggests that while our baseline results may be generalizable to a broader context than just that of asylum seekers, it also underlines once again the crucial role of

TABLE 13—ECONOMIC MIGRANTS: BILATERAL CRIME REGRESSIONS

Dependent variable:	Violent crime propensity					
Exposure to:	CC, MK	CC	MK	CC, MK	CC	MK
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbf{1}_{n=v}$	0.256	0.261	0.527	0.257	0.263	0.543
	(0.089)	(0.090)	(0.177)	(0.105)	(0.107)	(0.189)
$kid\,[1–12]_{n,a,t} \times \mathbf{1}_{n=v}$	0.597	0.624	1.256	0.598	0.625	1.252
	(0.276)	(0.285)	(0.478)	(0.282)	(0.292)	(0.483)
$\mathbf{1}_{v=CH}$				0.336	0.348	0.417
				(0.070)	(0.072)	(0.088)
$kid\,[1–12]_{n,a,t} \times \mathbf{1}_{v=CH}$				0.342	0.351	0.737
				(0.124)	(0.128)	(0.157)
Nationality of perpetrator × year × age FE	Yes	Yes	Yes	Yes	Yes	Yes
Nationality of victim × year FE	Yes	Yes	Yes	No	No	No
Observations	5,612,793	5,449,244	1,938,825	5,612,793	5,449,244	1,938,825
R^2	0.019	0.020	0.030	0.019	0.019	0.028
Sample mean (bilateral crime propensity)	0.01	0.01	0.01	0.01	0.01	0.01

Notes: OLS estimations based on the sample of male adult cohorts of economic migrants (B and C permits). Robust standard errors are two-way clustered at the nationality of perpetrator and victim levels. The dependent variable *Violent crime propensity* corresponds to bilateral propensity to violent crime. $\mathbf{1}_{n=v}$ is a binary indicator function equal to 1 if the perpetrator and the victim are co-nationals. $\mathbf{1}_{v=CH}$ is a binary indicator function equal to 1 if the victim is a native. $kid\,[1–12]$ 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 $k \in \{13, 14, 15, \ldots, 80+\}$ interacted with $\mathbf{1}_{n=v}$. 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.

economic integration in moderating the deleterious effect of past conflict exposure on violence.

Targeted Violence (Economic Migrants): We now document the persistence of intra-national violence following the logic of Section V. Table 13 replicates the bilateral crime regressions of Table 8 with the sample of economic migrants. Regarding the definition of bilateral violence, for perpetrators we focus on economic migrants only, while for potential victims we take into account all nationals of a given country living in Switzerland (i.e., asylum seekers, migrants, and natives). The large sample size yields precise estimates and the variables of interest all have the expected positive sign. From a qualitative and quantitative perspective, the results are similar to those obtained with asylum seekers: violent crimes mostly target co-nationals and this detrimental effect of co-nationality is exacerbated for cohorts exposed to civil conflict and mass killing during their childhood. Here again, the results suggest that, among economic migrants, inter-group hostilities may persist over time and transit to Switzerland.

VIII. Conclusion

This paper studies the population of asylum seekers in Switzerland over the 2009–2016 period and shows that exposure to civil conflict and mass killing during childhood (1–12 years of age) has a robust, strong, and persistent magnifying

effect on the propensity to commit violent crime later in life. Contrary to previous case studies of post-conflict reconstruction, our empirical strategy focuses on migrants and reveals that this persistence in violence is not due to poor institutions in post-conflict states, but that such behavior continues to hold even when people live in an environment with fully functional institutions (e.g., Switzerland). We find that cohorts exposed to civil conflicts/mass killings during childhood are 35 percent more prone to violent crimes than the average cohort. The impact of childhood conflict exposure on future violent crime propensity is found to be particularly strong for early childhood exposure and is confined to co-nationals, a finding consistent with inter-group hostility persisting over time.

Our results are relevant to current policy debate in that they do not advocate the need for more restrictive immigration policy. As found in Section VI, with a well-funded asylum system that offers rapid access to local labor markets and provides civil culture courses and opportunities for integration, the risk of an increase in crime perpetrated by migrants with conflict backgrounds can be well contained.[51] Our findings may also have implications for post-conflict reconstruction. The result on the key role of labor market integration in helping to break vicious cycles of persistent violence may apply more generally (as recently investigated by Blattman and Annan 2016), suggesting the need for particularly strong involvement and support from donor countries in the crucial first years following conflict. While, for example, the Marshall Plan certainly made a key contribution to Germany's "economic miracle" (*Wirtschaftswunder*) post-1945, helping to pave the way to a stable democracy, this lesson has perhaps been forgotten in more recent times, as witnessed by the ill-fated precipitated withdrawal from post-Gaddafi Libya. More research on the role of employment and long-run perspectives for post-conflict reconstruction should be encouraged.

[51] The issue of providing work opportunities is much discussed throughout Europe. There is, for example, a lively debate in Germany over the dismal share of asylum seekers with jobs. Indeed, there are only (maximum) 7,000 employed asylum seekers in the whole of Germany, according to *Die Welt* ("Warum kaum ein Asylbewerber Arbeit findet," March 14, 2017, https://www.welt.de/wirtschaft/article162839446/Warum-kaum-ein-Asylbewerber-Arbeit-findet.html). A report from the OECD (2017) argues that the very modest employment rates of asylum seekers in Germany may partially be due to low qualifications, poor language skills, and potential employers being deterred by asylum seekers' insecure legal status. The report stresses the usefulness of providing courses and training.

Appendix

Table 14—Life-Cycle Modulation and Early Exposure

	Violent crime propensity			
	(1)	(2)	(3)	(4)
kid [1–12] × *age* [18–29]	0.590			
	(0.761)			
kid [1–12] × *age* [30–39]	1.020			
	(0.491)			
kid [1–12] × *age* [40–49]	1.457			
	(0.573)			
kid [1–12] × *age* [50–59]	1.942			
	(1.117)			
kid [1–12] × *age* [60+]	0.705			
	(1.215)			
kid [1–12]		1.012		1.098
		(0.540)		(0.462)
born after [0–12]		−0.222	−0.324	
		(0.590)	(0.548)	
kid [1–5]			0.787	
			(0.406)	
kid [6–12]			0.400	
			(0.438)	
prenatal				−0.168
				(0.380)
Age FE	Yes	Yes	Yes	Yes
Nationality × year FE	Yes	Yes	Yes	Yes
Observations	12,564	12,564	12,564	12,564
R^2	0.200	0.200	0.200	0.200
Sample mean (crime propensity)	3.09	3.09	3.09	3.09

Notes: 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. *age* [*L*–*U*] is a dummy taking the value of 1 if the age of the cohort belongs to the age interval $[L, U]$. *born after* [0–12] takes a value of 1 if there has been a civil conflict or a mass killing in the 12 years *before* being born. *kid* [1–5] (*kid* [6–12]) is a binary measure of childhood exposure to civil conflict or mass killing between 1 to 5 years of age (6 to 12 years of age). *prenatal* is a binary measure of prenatal exposure to civil conflict or mass killing. 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 $k \in \{13, 14, 15, \ldots, 80+\}$.

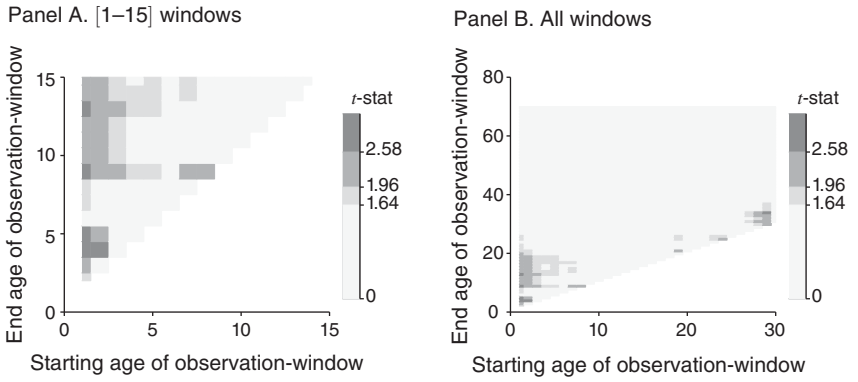

FIGURE 3. THE EFFECT OF VIOLENCE EXPOSURE ON VIOLENT CRIMES PROPENSITY: ALL POTENTIAL EXPOSURE WINDOWS

Notes: The figure reports the t-statistic of the estimated coefficient α in the following equation $CP_{n,a,t} = \alpha \times expo\,[s\text{–}e]_{n,a,t} + \sum_{k \notin [s,e]} \beta(k) \times expo\,(k)_{n,a,t} + \mathbf{FE}_{n,t} + \mathbf{FE}_{a} + \varepsilon_{n,a,t}$ where $expo\,[s\text{–}e]$ is a dummy equal to 1 for cohorts that have been exposed to a civil conflict/mass killing between the ages of s and e. Each pixel corresponds to a t-statistic for a particular combination of s and e in a separate regression, with $(s,e) \in [1,15] \times [s,15]$ in panel A and $(s,e) \in [1,30] \times [s,70]$ in panel B. Darker colors represents higher t-statistics so higher significance. The estimation sample consists of adult male cohorts of asylum seekers originating from countries that have experienced civil conflict or mass killing since 1946.

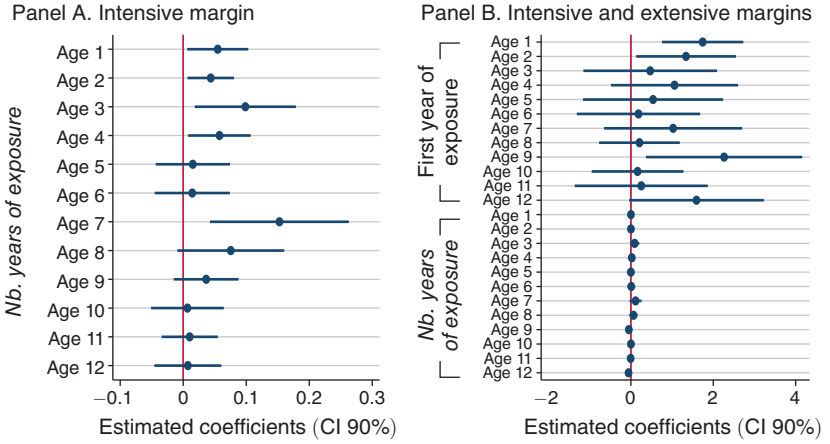

FIGURE 4. INTENSITY AND TIMING OF THE EXPOSURE TO CONFLICT DURING CHILDHOOD

Notes: Estimated coefficients and confidence interval at 90 percent are reported. Based on OLS estimations on the sample of adult male cohorts of asylum seekers, originating from countries that have experienced civil conflict or mass killing since 1946, with robust standard errors clustered at nationality levels. The dependent variable stands for the violent crime propensity of a cohort of nationality (n) × age bracket (a) × year (t). All underlying estimations include age fixed effects, nationality × year fixed effects and a set of binary variables coding for past exposure, but at the later ages $k \in \{13, 14, 15, \ldots, 80+\}$. Therefore, the reference group consists of those individuals born after the last years of violence.

TABLE 15—HETEROGENEOUS EFFECTS: SEVERITY OF CONFLICT

Dependent variable:	Violent Crime Propensity		
Exposure to:	CC and MK	CC	MK
Sample:	CC and MK	CC	MK
	(1)	(2)	(3)
kid[1–12]	1.141		
	(0.528)		
kid[1–12] $\times \frac{1}{Size}$	0.039		
	(0.180)		
kid[1–12]: *low severity*		−0.442	0.383
		(0.958)	(0.871)
kid[1–12]: *medium severity*		1.093	1.312
		(1.562)	(0.831)
kid[1–12]: *high severity*		2.093	2.206
		(1.200)	(0.849)
Age FE	Yes	Yes	Yes
Nationality × year FE	Yes	Yes	Yes
Observations	12,564	12,514	5,737
R^2	0.211	0.209	0.209
Sample mean (crime propensity)	3.09	3.11	2.59

Notes: 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. 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 $k \in \{13, 14, 15, \ldots, 80+\}$. Therefore, the reference group consists of those individuals born after the last years of violence. Our main explanatory variable as well as the exposure variables for the later ages $k \in \{13, 14, 15, \ldots, 80+\}$ are interacted in column 1 with the inverse of the country of origin size, in column 2 with the three quantiles of civil conflict severity, and in column 3 with the three quantiles of mass killings severity. Regarding the three quantiles of severity of civil conflict, for an average area in the sample, low severity corresponds to less than 899 casualties by country-year, medium severity to up to 6,059 casualties, and high severity to more than 6,059 casualties. Regarding the quantiles of severity of mass killing episodes, the first quantile ranges from 0 to 32,000 deaths, the second quantile goes up to 128,000 deaths, and the third quantile the remainder. Abbreviations CC and MK refer respectively to civil conflict and mass killing.

Table 16—Duration of Exposure: Cohorts Exposed during Childhood

Dependent variable:	Violent crime propensity							
Exposure to:	CC and MK	CC and MK	CC and MK	CC and MK	CC	CC	MK	MK
nb. years of exposure:	≤12	≤12	All	All	All	All	All	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
kid [1–12]		0.751		1.085		1.209		0.629
		(0.518)		(0.632)		(0.644)		(0.729)
nb. years of exposure	0.186	0.116	0.194	0.163	0.180	0.148	0.286	0.252
	(0.080)	(0.087)	(0.070)	(0.061)	(0.072)	(0.063)	(0.122)	(0.135)
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Nationality × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,564	12,564	12,564	12,564	12,514	12,514	5,737	5,737
R^2	0.200	0.200	0.201	0.202	0.200	0.201	0.197	0.197
Sample mean (crime propensity)	3.09	3.09	3.09	3.09	3.11	3.11	2.59	2.59

Notes: 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 killing (columns 1 to 4), to civil conflict only (columns 5 to 6), or to mass killing only (columns 7 to 8). *nb. years of exposure* is a continuous variable counting the number of years of exposure to civil conflict or mass killing (columns 1 to 4), to civil conflict only (columns 5 to 6), or to mass killing only (columns 7 to 8). In columns 1 and 2, *nb. years of exposure* ranges up to 12 years to match the childhood definition of 1 to 12 years of age used for *kid* [1–12]. In columns 1 to 4, the sample is restricted to cohorts originating from countries that have experienced civil conflict or mass killing since 1946, in columns 5 to 6 civil conflict only and in columns 7 to 8 mass killing episodes only. 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 $k \in \{13, 14, 15, \ldots, 80+\}$. 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.

Table 17—Controlling for Country of Origin Covariates during Childhood ([1–12])

Dependent variable:	Violent crime propensity							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
kid [1–12]	0.925	0.942	0.837	0.878	0.506	0.574	0.462	0.722
	(0.454)	(0.452)	(0.378)	(0.389)	(0.386)	(0.389)	(0.444)	(0.487)
% primary education rate [1–12]	0.058						0.104	
	(0.043)						(0.064)	
polity iv [1–12]			0.043				0.065	
			(0.060)				(0.073)	
gdp/cap [1–12]					0.000		0.000	
					(0.000)		(0.000)	
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Nationality × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,799	8,799	10,870	10,870	9,003	9,003	7,065	7,065
R^2	0.223	0.222	0.235	0.235	0.254	0.253	0.250	0.246
Sample mean (crime propensity)	3.03	3.03	3.19	3.19	3.07	3.07	3.03	3.03

Notes: 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 killing. 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 $k \in \{13, 14, 15, \ldots, 80+\}$. Therefore, the reference group of consists of those individuals born after the last years of violence. The measure of democracy *polity iv* comes from the Polity IV index (2017); *gdp/cap* is the real GDP per capita at constant prices, from the Penn World Table (2018), and the *% primary education rate* is obtained from the variable *% primary education rate for men aged more than 15* from Barro and Lee (2013) for a given year and nationality by shifting the year back by 15 years.

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