Civil war violence and refugee outflows

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May 27, 2020†

Abstract

Conflict forces millions of individuals from their homes each year, but surprisingly little research exists on violence and forcible displacement at the global level. Using a simple structural model and new refugee data, we produce the first set of estimates relating outflows to annual conflict magnitudes. The theory underlying the structural model implies that standard panel data approaches will underestimate the impact of conflict violence, by "differencing out" the effect of prior and expected levels of violence on the decisions to flee. We estimate that whereas a shock that doubles conflict deaths in one year increases outflows in that year by 40% on average, doubling conflict deaths in all years increases annual outflows by 100%. We further estimate an average of 30 refugees per conflict death (median 18), with higher rates for conflicts closer to an OECD country and possibly for ethnic wars and in lower income countries. The analysis illustrates a broader methodological point: It is hazardous to try to identify a causal effect using shocks to a presumed causal factor if the outcome variable is the result of decisions based not only on shocks but also on levels.

1 Introduction

According to the office of the UN High Commissioner on Refugees, the global stock of refugees more than doubled between 2010 and 2017, rising from about 10.6 to 22.4 million people. In the same period the number of internally displaced people (IDPs) is estimated to have grown from 14.7 to 39.1 million, thus by a factor of 2.7. These are staggering increases that associate with enormous welfare costs for the displaced. They have strained the international regime for refugee protection to multiple breaking points, and they have contributed to serious political conflict over asylum policies in some host countries.

* jfearon@stanford.edu, ashaver@alumni.princeton.edu. We thank Tunzaw Oo, Andrea Pellandra, and Kim Roberson at UNHCR for their generosity in providing and answering questions about the refugee data; Doug Rivers and Guy Grossman for helpful comments; participants of Stanford's Immigration Policy Lab lunch seminar for early suggestions; and Peninah Benjamin, Elizabeth Garrison, Jack Jacobs, Hugh McKenzie, Vanessa Smiley, and Andrew Tang for excellent research assistance.

† Draft

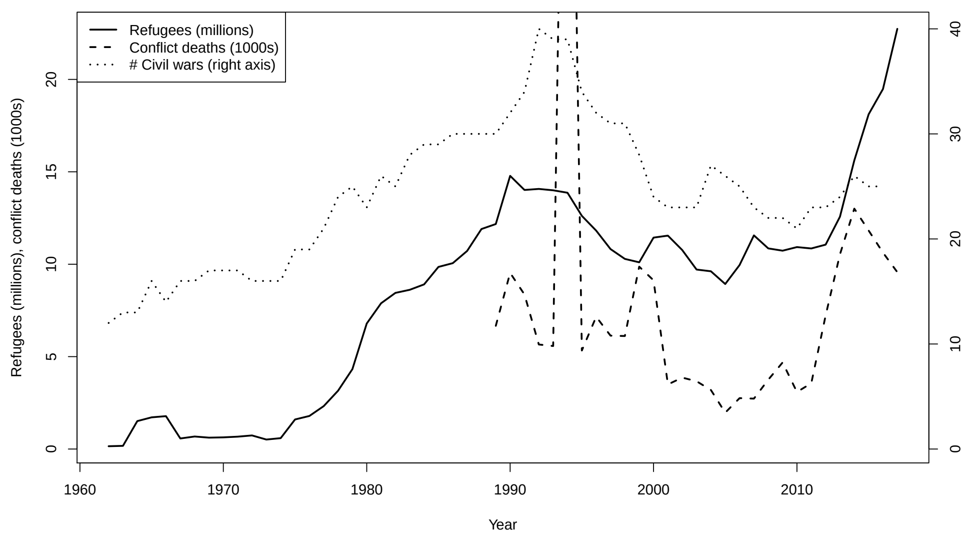
These massive increases in refugee numbers are driven far more by large-scale civil wars – such as the heavily covered war in Syria but also less well-covered debacles like the war in South Sudan – than by other causes of displacement, such as political repression absent civil war, or natural disasters. Figure 1 plots total refugees and asylum-seekers by year, along with measures of total conflict deaths and number of civil wars ongoing. Total refugees track the prevalence of civil war in the international system fairly well, and total conflict deaths perhaps even better. Using data to be discussed below, we find that the global refugee crisis is overwhelmingly the result of civil war and attendant state collapse. Almost 90% of refugee outflows between 1990 and 2017 were from countries that had a significant civil conflict, and about 80% of all refugees fled in years in which their home country experienced at least 100 fatalities from armed conflict. A mere eight countries account for half of all refugee outflows in this period, all of them wracked by major civil war and state dysfunction (Syria, Iraq, Rwanda, South Sudan, Somalia, Afghanistan, Liberia, and DRC). Half of the rapid global increase in refugees since 2010 is due to outflows from just three countries (Syria, Afghanistan, and South Sudan).[1]

In this paper we use new data on refugee outflows by country year to estimate the relationship between the intensity of armed conflict, as measured by conflict death estimates, and refugee outflows. We focus on two quantities of interest. First, the *rate* of refugee production: On average, how many refugees are produced per battle death? Our median and mean estimates come out at 18 and 30 for conflicts since 1990, with a large interquartile range across conflicts of 9 to 41.

Second, the *elasticity* of refugee production: On average, if conflict deaths double (say), by what percentage do refugee outflows increase? Here we find that the answer depends on how one asks the question. At the level of whole conflicts, refugee outflows show approximately constant returns to scale in the amount of violence. That is, double the magnitude of the war, double the

[1] On Figure 1: Refugee and asylum-seeker numbers are from the UNHCR's publicly available "stocks" data (`http://popstats.unhcr.org/`), and begin in 1961. Civil wars are based on an updated version of the list from Fearon and Laitin (2003), using the criterion detailed in that article (in particular, at least 1,000 battle deaths with an average of at least 100 per year). The annual conflict death estimates are based on UCDP (Pettersson and Eck 2018; Sundberg and Melander 2013), and begin in 1989; the large spike reflects the Rwanda genocide. We note that before around 1980, part of the similarity between the refugee and civil war numbers could be due to improved counting of refugees over time. That is, it seems likely that many refugees were not counted by the UNHCR system, or did not make it into the data set, in the 1960s and 70s. More discussion of the data below.

Figure 1: Refugees, civil wars, and conflict deaths



refugee crisis, on average.

Within a civil war, by contrast, we find that if conflict deaths double from one year to the next, refugee outflows increase on average by about 40%, considerably less than constant returns to scale (which is 100%). In other words, within conflicts there appear to be "diminishing returns" in the impact of violence on refugee outflows.

What explains these different impacts? We pose and estimate a simple structural model of refugee production that generates interpretable empirical specifications and helps makes sense of the results. In the model, the population share of refugees in year t is the product of the share of people directly affected by conflict violence in that year, times the share of those who decide to flee the country. We argue that the share who decide to leave depends in part on the level of destruction from past violence, as well as on expectations about violence levels in subsequent years. For example, the more violence and danger one expects in the next year, the worse the prospects for recovering and rebuilding from damage suffered "today," and thus the more attractive is the option to become a refugee.

An immediate implication is that the standard country-fixed effects approach to estimating the relationship between conflict violence and refugee outflows can dramatically understate the total impact, because it tends to "difference out" the effect of overall conflict intensity on decisions to leave the country. A given number of conflict deaths in one year can have very different implications for refugee outflows, depending on past and expected levels of violence in the conflict.

Our data and model also allow analysis of what sorts of countries and conflicts are most prone to generating large refugee flows (higher rates of outflow per conflict death). Proximity to an OECD country and lower income associate with significantly higher numbers of refugees per conflict death, which is consistent with a simple opportunity cost model for the decision to flee. Ethnic wars may associate with more refugees per death, possibly because these sometimes involve explicit ethnic cleansing campaigns. There also may be some tendency for civil wars in Asia to produce fewer refugees per death, for reasons that are not clear. Interestingly, these same factors are scarcely related to the elasticity of refugee outflows across countries (that is, they correlate with levels, but not sensitivity). This is consistent with an implication of the theory, that elasticities reflect "technological" features that scale the amount of damage to homes and livelihoods with the amount of killing, whereas distance to OECD (say) increases refugee numbers for any given number of deaths, by lowering opportunity costs of becoming a refugee.

From a policy perspective, the analysis and empirical estimates can inform expectations about the scale of a refugee crises *across* conflicts, and also about how refugee outflows are likely to respond to escalation or deescalation of fighting *within* an ongoing conflict. Combined with the statistics on how concentrated are the sources of growth in the global refugee crisis, the analysis here also suggests high returns to more international money and effort to prevent and end civil wars.

Quantitative studies of the determinants of refugee flows were pioneered by Schmeidl (1997), Davenport, Moore and Poe (2003), and Moore and Shellman (2004). More recent works in a similar vein include Iqbal and Zorn (2007), Melander and Öberg (2007), and Turkoglu and Chadefaux (2019). These typically estimate refugee flows by differencing the global "stock" numbers publicly reported by UNHCR or the US Committee of Refugees, in some cases including IDP estimates in total stocks.[2] A related line of work analyzes annual asylum applications (a directly measured

[2]The stock of refugees from country j in year t is the year t total reported by host countries and UNHCR or

flow) to western European countries (Neumayer 2005; Hatton 2016; Missirian and Schlenker 2017) or more broadly (Blair, Grossman and Weinstein 2019).

Our analysis differs in several ways. First, we employ direct measures of annual refugee outflows that were provided by the Field Information and Coordination Support Section (FICSS) of UNHCR for the purposes of this study. We discuss differences between the stocks-based estimate and these data in the next section.

Second, some studies include both refugees and IDPs in their dependent variable, whereas we focus exclusively on refugees (people who cross international borders). We do this because a number of policy implications attach specifically to refugee flows; because our theory and model imply different data-generating processes for IDPs and refugees; and because, even if we wanted to estimate elasticities and rates separately for IDPs, at present there is much less credible IDP flow data available.[3]

Third, whereas existing studies typically include categorical indicators for different types of conflict in a country year (e.g., civil war, riots, human rights abuses), we use the annual estimates of conflict deaths produced by UCDP for the years 1989-2017. This is the first time, as far as we know, that these measures of conflict magnitude have been used to predict refugee flows. Their annual variation allows us to estimate within-conflict effects more effectively than past studies have been able.[4] Relatedly, we analyze data for 1990-2017, whereas most of the existing studies of refugee flows are either entirely for data from the Cold War period or go to around 2000.

Fourth, we derive estimation methods and specifications from a theory of refugee outflows that

USCR for all countries other than j. In some cases the authors use stock as the dependent variable, controlling for lagged stocks (Schmeidl 1997; Davenport, Moore and Poe 2003) or not (Turkoglu and Chadefaux 2019).

[3]UNHCR and the Internal Displacement Monitoring Centre provide series for "new IDP displacements" that cover 2006-2017 and 2008-2017, respectively. However in both cases there are many missing observations and no way to distinguish missing values from zeroes. There is a growing and impressive literature on internal displacement (and also refugees) using individual-level data from specific conflicts. See in particular Balcells and Steele (2016) and Steele (2017) on IDPs in Colombia and Spain, and Braithwaite, Salehyan and Savun (2019) on micro-level studies more generally.

[4]Some analyses of refugee flows have used battle death estimates for years before 2009 due to Lacina and Gleditsch (2005), which in most cases provide totals for a conflict rather than year-by-year estimates (which are simply impossible for many conflicts before 1990 or so). Thus many observations are based on dividing a total estimate equally over years of the conflict.

we formalize in a simple structural model. This yields more interpretable and meaningful estimands – rates and elasticities – and greater clarity about the meaning of what we are estimating. Existing studies typically present multivariate models in which dichotomous indicators of different conflict types are included along with a large number of other covariates, and the dependent variables are negative-binomial or zero-inflated negative binomial rate parameters. While very useful for providing information on patterns of correlations, this approach can also make interpretation tricky. For example, some studies pit conflict indicators against economic indicators. But since armed conflict usually causes significant economic decline in the affected country, economic variables are "post treatment" so that the total impact of conflict on refugee flows is probably underestimated.

More important, the model helps us to see that a reduced-form, country-fixed-effects approach that uses year-to-year variation in violence levels within conflicts will be misleading, by "differencing out" the impact of levels of violence on decisions to flee. The analysis thus illustrates a more general danger of using short-run shocks to try to identify the impact of a causal factor X in observational data. If an outcome variable Y is influenced by peoples' choices, and if current choices depend in part on past or expected levels of X, then the "local average treatment effect" of shocks is different from the effect of variation in levels. This is more likely to be missed in a reduced-form approach that does not theorize the data-generating process.

Section 2 discusses the data. Section 3 develops the theory behind the estimations carried out in sections 4-6, which concentrate first on elasticities and then on rates. Section 7 concludes with a summary of the empirical findings and some broader implications, both methodological and regarding policy.

2 Data on refugee outflows and conflict deaths

The refugee-flow data obtained from FICSS are dyadic and specify for each year the number of refugees and asylum seekers newly arrived to a host from an origin country. We construct a panel dataset that sums refugee and asylum seeker numbers by origin to produce a measure of the total number of individuals forcibly displaced across international borders per year for every country for the period 1990-2017. In all analyses we omit countries in Western Europe, the "British offshoots," and Japan, which together account for one twentieth of one percent of all refugee outflows in our data (in addition, most of these probably stem from classification glitches).[5]

We focus on the post-Cold War period because these years are covered by UCDP's annual conflict death estimates, but also because the refugee estimates appear to be significantly less complete and reliable the farther back in time one goes, especially before 1980. For instance, conflicts known to have produced 100,000s or even many millions of refugees – such as Pakistan/Bangladesh 1971, the Vietnam war, Cambodia 1970-74, Ethiopia in the 1960s, Myanmar until 1991, and Biafra – are coded in the UNHCR data as missing or as having produced zero or very few refugees.

The global stock of refugees from a country can change year to year not only because of new outflows but also as a result of returns, births, deaths, and changes of citizenship. Changes in stocks are thus an inherently noisy measure of refugee outflows in a year, which is the relevant dependent variable if we want to understand determinants of refugee crises.[6]

Comparing our measure with the measure based on year-to-year change in stocks, we find that they are reasonably well correlated, at .49 in logs for all country years (N = 4,493), .82 for global totals by year (N = 55), and .92 for logged totals by country (N = 170). However, there are dramatic differences when we look at levels and variation within countries. Because refugee stocks from country j frequently decline from one year to the next due to mass returns and perhaps assimilation or change in citizenship status, fully 45% of country-year observations using the stocks-based measure are zeroes, as compared with just 4.7% of our flow measure.[7] The stock-

[5] An asylum seeker is a person who has formally applied for protection under the 1951 UN Refugee Convention, based on a "well-founded fear of persecution" were they to return to their country of origin. "Refugees" in the UNHCR and USCR statistics are people who have fled their home country and are in need of protection (Crisp 1999; Schmeidl 2000). In our data from UNHCR, 63% of all forced migrants are counted as "refugees" and the rest are asylum seekers. What determines whether forced migrants can and do formally apply for asylum status is idiosyncratic and dependent on circumstances specific to specific conflicts, in particular whether neighboring country policies allow for asylum applications. Both classifications are "forced migrants" so throughout the paper we combine "refugees" and "asylum seekers" and refer to the sum simply as "refugees."

[6] Another possible source of year-to-year change is variation in UNHCR's measurement, definitional, and coding practices. In all panel models examined below, we include year fixed effects to try to minimize the impact of any such changes. As it happens, year fixed effects account for very little variation and results are nearly identical whether or not they are included.

[7] We note that refugee returns can be significant even as conflict persists in a country. In 2017, for instance, tens of thousands Syrian refugees are estimated to have returned to the country even as hundreds of thousands more fled. In the case of children born to refugees, growths in refugee stocks can be substantial. For instance, more than

based measure thus tends to underestimate annual refugee outflows – the median is 487 with our measure versus 4 with differenced stocks (per country year). It also exhibits far greater variability within countries due to returns or other reductions. With our flow measure (in logs), 81% of the variation is accounted for by country dummies, whereas the same statistic is only 23% for the stocks-based measure. Refugee outflows are in fact *much* more steady than one would estimate from differencing global stocks.[8]

For our measure of conflict intensity, we rely on the Uppsala Conflict Data Program's annual "best" estimates of deaths from civil conflict, interstate war, one-sided violence (usually government or rebel group massacres of noncombatants), and non-state violence (what others sometimes refer to as "communal conflict") (Sundberg and Melander 2013; Högbladh 2019). About half of the 2.27 million total deaths for 1990-2017 are what UCDP calls "battle related deaths" in civil conflicts, which are "deaths caused by the warring parties that can be directly related to conflict." These include non-combatant deaths that result from collateral damage, while excluding "indirect deaths due to disease and starvation, criminality, or attacks deliberately directed against civilians (one-sided violence)" (Allansson and Croicu 2017*a*: 14). One-sided violence is "the use of armed force by the government of a state or by a formally organized group against civilians which results in at least 25 deaths," excluding extrajudicial killings in custody (Allansson and Croicu 2017*b*: 2). These comprise 35% of the total although only 16% omitting the Rwandan genocide. The rest are deaths in interstate conflicts (5.8%), almost all of which are in the Ethiopia/Eritrean war and Iraq (1991 and 2003), and non-state-violence (7.2%).[9]

UCDP's estimates are based on publicly available data from "global newswire reporting, global

300,000 children are estimated to have been born to Syrian refugees in Turkey in recent years, and approximately 60,000 children per year to Afghan refugees in Pakistan (UNHCR 2015).

[8]Let R_{jt} be the UNHCR-estimated global stock of refugees from country j in year t. As authors in this literature have typically calculated it, the differenced measure for refugee flow is then $r_{jt} = \max\{R_{jt} - R_{jt-1}, 0\}$. When calculating logged refugee and conflict deaths here and elsewhere, we are adding one to bring in the zero observations, unless otherwise noted.

[9]We used the UCDP georeferenced event data set to sum up conflict deaths occurring with each country year. Georeferenced data for Syria is only available for 2016 on, so we used the UCDP yearly data sets for Syria 1989-2015. We include conflict deaths from interstate wars, which are rare in this period relative to civil wars, but in a few cases associate with large refugee flows.

monitoring and translation of local news performed by the BBC, and secondary sources such as local media, NGO and IGO reports, field reports, books etc." (Allansson and Croicu 2017*a*: 13). Their procedures are conservative. They say "it is very unlikely that there are fewer [fatalities] than the UCDP best estimate" (14). Note that our rate estimates below – refugees per conflict death – should be literally interpreted as refugees per UCDP-recorded conflict death. If the true numbers are, say, 10% higher on average, then the true rates would be 10% lower, assuming the refugee numbers are not also systematically underestimated. On the other hand, our estimates of elasticities should not be little affected, in so far as the UCDP measure is coded by approximately common standards within conflicts.[10]

3 Estimating conflict-related refugee outflows

In this section we derive empirical specifications from a simple model of individual or household decisions concerning becoming refugees.

We will say that individuals or households have been *directly affected* by conflict violence if they had family members who were killed, injured, or abducted; they or their community were directly threatened with violence; or their livelihoods were destroyed or at least damaged. It is reasonable to assume that on average, the share of a country's population that is directly affected by conflict violence in a given year is positively related to our measure of the magnitude of conflict violence, the UCDP estimate of conflict deaths in that country year.

If an individual or household is directly affected by conflict violence, they make a decision about whether to try to flee the country and become refugees (in some cases, they may already be IDPs). Not all who are directly affected decide to leave. Indeed, it appears that most often, most people do not. Using our outflow data, the median and mean share of country population who flee in a conflict year are .0025% and .38%, respectively. Summing over war years, we estimate that the median civil war country lost about .4% of total population. The mean is 3.2%, pulled upward by

[10]To see this, note that if, say, UCDP's estimates are on average 10% too small year-to-year for a given country, this does not affect elasticity estimates since these concern how a percentage change in the death estimate relates to a percentage change in refugees (the 10% comes out in the constant, after logging).

a small number of extreme cases, for which it is clear that large majorities of the population were directly affected by conflict violence.[11]

Next, it is plausible that the decision of whether to become a refugee, conditional on having been directly affected, depends positively on both how damaging the conflict has already been, and how damaging it is expected to be. Suppose that a household's farm or business has been damaged or destroyed by conflict in year t. If they expect that the war will end in year $t + 1$, or that it will continue but at a low level that would allow rebuilding with high probability, then staying and rebuilding can make more sense than suffering the costs and uncertainties of becoming a refugee. By contrast, if they think that rebuilding their former life is likely to be very risky or impossible because they expect that the war will continue at a high level, then the option of becoming refugees looks less bad. Similarly, worse damage from previous years of the conflict may make the prospects for national rebuilding and recovery worse, thus making the exit option more attractive for affected households.

By this analysis, the share of the total population that exits as refugees in conflict year t will be (a) the share of total population who are directly affected in year t times (b) the share of (a) who decide to flee the country. The former depends positively on conflict violence in that year (and possibly the year before), while the latter depends positively on expectations about how bad the violence will be in the future, and also on prior intensity.[12]

We now formalize these considerations to draw out implications for how to set up an empirical model of refugee flows and what can be learned from the data. Consider conflict-affected country

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[11] There are a couple of ways to estimate share of population that became refugees over the course of a civil war with our data; different approaches tend to yield quite similar numbers. Here we sum the refugee share of total population for years with at least 100 conflict deaths, which implicitly assumes no returns during the war. For Liberia this results in a dramatic overestimate of 66% due to repeated waves of flight to Guinea and Ivory Coast and then returns. We drop Liberia for the numbers reported. The other very high percentage cases are Rwanda (40), Syria (35), and Somalia (21). Raleigh (2011) and Engel and Ibáñez (2007) also note the small percentage of people who exit even heavily affected areas, the latter based on a study of internal displacement in Colombia. Of course, small annual percentages add up, collectively, to millions of refugees, and a very large human toll.

[12] Of course, some people may decide to leave the country entirely due to anticipation of bad times to come, even if they were not directly affected. For example, Camerena (2018) analyzes refugee waves from Burundi in 1993, where tens of thousands of Hutus fled "in anticipation of violence, not in direct response to violence," that is, before it had reached their specific communities. Our main point about bias is unaffected (in fact, it is strengthened) by not explicitly modeling this subset in the formalization, since this group adds to the marginal effect of overall conflict magnitude.

j in year t. Let d_{jt} be total UCDP conflict deaths in year t, and let n_j be total population.[13] Let $D_{j|t}$ be a measure of both past and expected future conflict intensity, which we argued should be positively related to share of those directly affected by conflict who decide to flee in a given year. In the empirical analysis we will assume that $D_{j|t}$ is proportional to the realized average number of UCDP conflict deaths per year in the conflict.

We use a flexible functional form to model the share of the population directly affected when there are d_{jt} deaths, and the share of this group that flees when prior and expected intensity is $D_{j|t}$, as follows.

$$\text{share directly affected}|d_{jt} = (a_j d_{jt}/n_j)^{\beta_j}$$
$$\text{share of those who become refugees}|D_{j|t} = (b_j D_{j|t}/n_j)^{\gamma_j}$$

$a_j > 0$ and $b_j > 0$ are parameters that scale per capita deaths in year t and the prior and expected future scale of the conflict to the number of people directly affected by conflict violence, and the share who become refugees, respectively. β_j and γ_j will be estimated. As discussed below, they give the elasticity of refugee flows with respect to d_{jt} and $D_{j|t}$.

Let r_{jt} be the number of refugees fleeing the country in year t. Putting things together, and introducing a mean-zero error term ϵ_{jt}, we have

$$\frac{r_{jt}}{n_j} = \left(\frac{a_j d_{jt}}{n_j}\right)^{\beta_j} \left(\frac{b_j D_{j|t}}{n_j}\right)^{\gamma_j} e^{\epsilon_{jt}} \tag{1}$$

$$\log r_{jt} = \log a_j^{\beta_j} b_j^{\gamma_j} + \beta_j \log d_{jt} + \gamma_j \log D_{j|t} + (1 - \beta_j - \gamma_j) \log n_j + \epsilon_{jt} \tag{2}$$

An immediate implication of this analysis is that *the current-year effect of a one-time increase in conflict violence on refugee flows – a "shock" – is different from the current-year effect of a "permanent" increase*, in the form of greater overall conflict magnitude. To see this, consider what happens if violence is doubled from d_{jt} to $2d_{jt}$ in year t, but overall conflict intensity does not

[13] As noted, in all but a very small number of conflict-affected countries, the proportion of the population that flees each year during civil war is extremely small, so that it is an inconsequential simplification to use n_j rather than n_{jt}.

change ($D_{j|t}$). Then refugee outflows would be expected to increase by a factor of 2^{β_j}, on average (see (1)), because the number of people directly affected by conflict would increase. By contrast, if violence doubled in year t *and* was expected to be twice as large on average in subsequent years ($D_{j|t}$ to $2D_{j|t}$) then refugee flows would be expected to increase in year t by a factor of $2^{\beta_j+\gamma_j}$, because both the number directly affected and the share of these fleeing would increase. This is a straightforward consequence of conflict-affected individuals and households taking into account their prospects for safety and rebuilding.

A second implication, following from the first, concerns what will happen in a standard panel analysis if we do not take into account the impact of overall conflict intensity on the share of conflict-affected who flee. Suppose that in a sample of war years in conflict-affected countries we estimate

$$\log r_{jt} = \alpha_j + \beta_{panel} \log d_{jt} + \epsilon_{jt}, \tag{3}$$

where α_j is now a country-fixed effect. Write the process generating deaths as $\log d_{jt} = \log d_j + \delta_{jt}$, where $\log d_j$ is the average of log deaths per year in conflict country j, and δ_{jt} is a mean zero random variable.[14] Notice that β_{panel} will be identified based on year-to-year variation in conflict deaths, the δ_{jt} component. But in so far as past destruction and expectations of future violence affect decisions to flee, the impact of prior and expected *level* of violence in country j will be absorbed in the country fixed effect α_j. A more intense war – higher d_j – will be correlated with higher $D_{j|t}$, which is a component of the country-fixed effect α_j, in the $\gamma_j \log D_{j|t}$ term of (2). Thus the total effect of different levels of violence on refugee flows is larger than the effect of shocks estimated by (3).

Specifically, write $D_{j|t}$ as a stochastic multiple of annual average conflict deaths, $D_{j|t} = d_j e^{\tau_{jt}}$, where τ_{jt} is a random variable. Then the total effect of increasing log average conflict intensity, $\log d_j$, on $\log r_{jt}$ is $\beta_j + \gamma_j$ in country j.[15]

[14] In our data, the empirical distributions of log refugees per year within countries, and log conflict deaths per year within countries, are approximately Normal.

[15] α_j includes $\gamma_j \log D_{j|t} = \gamma_j \log d_j + \gamma_j \tau_{jt}$, so the effect of varying $\log d_j$ is $\gamma_j + \beta_j$.

Neither γ_j, nor a pooled "average" γ across countries, could be estimated from within-country evidence without a good measure of past and expected future damage that varies year-to-year within countries. We can, however, test a direct implication of this theory of refugee production by comparing the coefficient β_{panel} estimated with the data in (3), with the coefficient from a cross-sectional regression that compares averages across countries.

To see this, average the left- and right-hand sides of (2) over conflict years in each country j, yielding

$$\overline{\log r_{jt}} = \log a_j^{\beta_j} b_j^{\gamma_j} + \beta_j \overline{\log d_{jt}} + \gamma_j \overline{\log D_{j|t}} + (1 - \beta_j - \gamma_j) \log n_j + \overline{\epsilon_{jt}}.$$

Using $d_{jt} = d_j e^{\delta_{jt}}$ and $D_{j|t} = d_j e^{\tau_{jt}}$, and assuming either constant effects or "random coefficients" across conflict countries, we obtain the cross-sectional model[16]

$$\overline{\log r_{jt}} = \alpha_j + (\beta + \gamma) \log d_j + (1 - \beta - \gamma) \log n_j + \epsilon_j. \tag{4}$$

So under the plausible assumption that both past and future levels of violence influence the exit decisions of those directly affected by conflict in a given year of armed conflict, we have

Hypothesis 1. *The estimated coefficient on log conflict deaths in the country-fixed effects panel-data model (3) will be smaller than the estimated coefficient on mean log deaths in the cross-sectional model (4).*

The reason, in brief, is that average conflict intensity is expected to increase the share of directly affected people who decide to become refugees for a given amount of violence in a given year, and the cross-sectional comparisons take account of different levels whereas the within-conflict comparisons of the panel model hold these constant.

[16] Year-to-year variation in $\log d_{jt}$ and $\log D_{j|t}$ is averaged within countries and absorbed into the new constant α_j and error term ϵ_j. β and γ are means for β_j and γ_j; country-specific variation around these is likewise absorbed in α_j and ϵ_j. Note that essentially all regression models implicitly assume either constant effects ($\beta = \beta_j$ for all j) or that variation in the coefficients across units (heterogeneous effects) is random with respect to the covariates (Rivers 1988).

The model provides an additional testable implication for the cross-sectional regression, concerning country population.[17]

Hypothesis 2. *The estimated coefficient on log country population in model (4) should be approximately equal to 1 minus the estimated coefficient on mean log deaths.*

Because they derive from a theory of refugee production, the coefficients in the above models have substantive interpretations as elasticities. Let $\hat{\beta}_{panel}$ and $\hat{\beta}_{cs}(=\beta+\gamma)$ be the estimates from (3) and (4). As shown in the next section, these turn out to be about .42 and 1.08, consistent with Hypothesis 1. In the terms of the structural model, $\hat{\beta}_{panel} = .42$ implies that a 10% (say) upwards shock to conflict deaths in one year, without changing expected conflict magnitude, is estimated to cause, on average, a 4.2% increase in the share of the directly affected population. Likewise, using $\hat{\beta}_{cs} - \hat{\beta}_{panel} = .66$ as an estimate for γ, a 10% increase in overall conflict intensity is estimated to cause a 6.6% increase the share of conflict-affected who flee the country (on average), holding constant the number killed in year t.

Further, with (1) interpreted as a refugee production function $\hat{\beta}_{cs} = 1.08$ is an estimate of $\beta+\gamma$ and thus average "returns to scale" of refugee outflows with respect to average conflict deaths. Our estimates imply *approximately constant returns to scale* – double the size of the civil war overall implies, on average, doubling the total refugee outflows.

Are there reasonable alternative explanations for $\hat{\beta}_{cs} > \hat{\beta}_{panel}$? An immediate concern would be if there were plausible omitted variables that (a) cause refugee flows and (b) vary positively with average conflict magnitude but are not themselves caused by conflict violence.[18] The size of the difference between $\hat{\beta}_{cs}$ and $\hat{\beta}_{panel}$ suggests that these would have be large – at the very least, 1.5 times the size the effect of conflict deaths – if in truth $\gamma = 0$. Nonetheless, we can partly address this question by controlling for pre-war refugee flows in conflict-affected countries, thus conditioning on country-specific causes of refugee flows not directly caused by the subsequent violence.

[17] We measure population in 1990 or just prior to the start of the conflict, whichever comes later, in order to minimize effects of conflict on population. (At the cross-sectional level cross-country variation completely swamps within-country variation, so it does not really matter when population is measured.)

[18] For example, we should not control for, say, economic conditions during conflict, since these are a pathway for the effect of violence on refugee outflows (they would be a "bad control").

Another possibility is that measurement error in the UCDP's annual death estimates attenuates $\hat{\beta}_{panel}$, whereas there is less measurement error after we average across years in the cross-sectional model. It seems just as likely that there is comparable or worse attenuation bias in the cross-section that does not afflict the panel analysis – whereas media attention to, and information about, specific conflicts is fairly constant year to year and draws on the same set of sources, there are systematic differences in these across conflicts. Nonetheless, we show in the appendix that if the truth were that $\gamma_j = 0$ for all countries, then one has to add an implausibly large amount of noise (2.4 times the variance of the signal) to the annual death estimates to create this large a difference between $\hat{\beta}_{panel}$ and $\hat{\beta}_{cs}$.[19]

A last issue worth noting is the possibility that year-to-year variation in d_{jt} is correlated with the omitted variable $D_{j|t}$ in the panel model (3), making for some bias in $\hat{\beta}_{panel}$. If, for instance, positive shocks to deaths in year t correlate with increases public expectations of future damage, then $\hat{\beta}_{panel}$ will mistakenly pick up some of the impact of expectations on decisions to flee, thus leading to an underestimate of γ in the comparison to $\hat{\beta}_{cs}$. Regarding Hypothesis 1, as long as the correlation is not perfect, then $\hat{\beta}_{panel}$ understates the impact of conflict violence on refugee flows.

4 Elasticities: How does variation in civil war intensity affect refugee production?

Table 1 shows the results for the models just discussed, with the panel analysis (model 3) in columns 1 and 2, and the cross-sectional set-up (model 4) in columns 3-6. Following the theoretical model, we want to estimate the effect of variation in violence levels on refugee outflows during armed conflict, so we use years in conflict countries that had a significant amount of violence as measured by UCDP. The table shows results employing a threshold of at least 100 deaths in the year, for the 77 countries with at least one year with at least 100 conflict deaths between 1990 and 2017. Results are very similar if we restrict attention to the 61 countries that had at least 1,000 total conflict

[19] The differential measurement error explanation also implies that the true short-run elasticity of refugee flows with respect to violence in a year would have to be remarkably large; see the appendix.

deaths in this period, whether the annual threshold 100 or 25 or more deaths (see appendix).

Consistent with Hypothesis 1, we see that the cross-sectional estimates are much larger than the panel estimates. A shock that doubles conflict deaths from one year to the next, but with no change in expected overall conflict intensity, is estimated to increase refugee outflows by 42% on average (column 1). By contrast, the elasticity estimates in the cross-sectional specification run from about 2.3 to 3 times larger. Without the outlier of Papua New Guinea (columns 4 and 6), the estimated overall elasticity is approximately 1, or *constant returns to scale*: Double the war intensity, double the total refugee production, on average. This is a substantively large relationship that one might say, after the fact, makes intuitive sense. But it would be missed if we had focused on within-country variation without the aid of an analysis like that above.[20]

Column (2) adds log conflict deaths in the previous war year to the panel model. Decisions and the ability to flee after damage incurred in one year could carry over to the next. We see, however, that lagged deaths get an estimate that is positive but close to zero, and that the total impact (.394 + .046 = .44) is about the same. This is consistent with the expectation that the country-fixed effects model estimates an effect of violence that works mainly through increasing the number of conflict-affected in that year.[21]

Columns 5 and 6 add average refugee outflows in country years before the first conflict year, as a way of controlling for unmeasured country-specific determinants. While this variable clearly picks up the influence of powerful omitted factors – observe the increase in R^2 – the reduction in the estimates for average logged conflict deaths is quite small.[22] This suggests that it is not likely that the much of the difference between the panel and cross-sectional estimates is due to omitted variables (Altonji, Elder and Taber 2005).

[20]For the Bougainville conflict, UCDP has a single year (1996) with more than 99 deaths, and UNHCR records zero refugees in that year (1996); this outlier pulls the regression line down to a lower intercept, higher slope. Note also that including the control for pre-war refugee flows in country j in models 5 and 6 reduces the sample because a number of countries are already in war in 1990.

[21]If we add an additional (two-year) lag, the current year estimate is about .37 while both lags are .022 and .038, neither close to statistically significant.

[22]The reason is that there is almost zero relationship between prior refugee flows and average levels of conflict violence.

Table 1: Civil war and refugee flows. Elasticities.

	panel		cross-section			
	$\log r_{jt}$		$\overline{\log r_{jt}}$			
	(1)	(2)	(3)	(4)	(5)	(6)
$\log d_{jt}$	0.420***	0.394***				
	(0.060)	(0.066)				
$\log d_{jt-1}$		0.046				
		(0.036)				
$\overline{\log d_{jt}}$			1.232***	1.088***	1.088***	0.983***
			(0.179)	(0.163)	(0.197)	(0.178)
$\log(pop)$			0.200	0.155	0.051	0.064
			(0.131)	(0.117)	(0.160)	(0.142)
$\overline{\log r_{jt} \mid prewar}$					0.504***	0.408***
					(0.095)	(0.089)
constant			−2.686	−0.919	−2.411	−1.310
			(2.459)	(2.228)	(2.870)	(2.567)
N	766	763	77	76	43	42
R^2	0.733	0.756	0.402	0.387	0.663	0.620

Notes: $d_{jt} = \text{deaths}_{jt}$, $r_{jt} = \text{refugees out}_{jt}$. Overbars denote means within countries. Sample restricted to conflict years, defined by UCDP $d_{jt} > 99$, in countries with at least one such year. Panel models include country and year fixed effects, and cluster errors by country. Models 4 and 6 drop Papua New Guinea (outlier). All data is for 1990-2017. Population is measured in the year before start of the war, or 1990 if the war began earlier. Standard errors in parentheses. *p < .1; **p < .05; ***p < .01

We have also tested whether the immediate elasticity of refugee flows with respect to conflict violence varies over time within conflicts. On the reasonable argument that those who are more able to leave will leave sooner (Schon 2019), one might expect that the elasticity would decline over time. But on the other hand, more refugees abroad can lower the costs of leaving for subsequent refugees. Also, as noted above, with rare exceptions like Syria, the share of total population that actually leaves the country during a civil war is tiny, and it appears that most refugees are drawn from the (usually small) parts of the country that are most conflict-affected in that year. Empirically, we find that the elasticity of refugee flows with respect to conflict deaths declines on average by .0029 per year during war years, a very small effect that is not statistically distinguishable from zero (the estimated standard error is .0051).[23]

Recall that the simple structural model implied that the estimated coefficient on the log of country population in the cross-sectional model should be equal to $1 - \beta - \gamma$, or approximately $1 - \hat{\beta}_{cs}$. Table 1 shows good agreement with Hypothesis 2. Substantively, this has the interesting implication that the number of refugees is basically unrelated to size of the conflict-affected country (since $\hat{\beta}_{cs} \approx 1$).

5 Correlates of sensitivity of refugee flows to conflict violence

The data gives us some information about how the refugee response to violence varies across conflict-affected countries. In this section we use a mixed (or "hierarchical," or "random coefficients") model (Gelman and Hill 2007) to estimate different $\hat{\beta}_j$'s for different countries, and to ask if these are predictable from country features like income, type of conflict, and distance from an OECD country.

In principle we could estimate β_j from separate country regressions. But in practice many cases have only a few war years, which can make for extreme and high variance estimates. The mixed model estimates a weighted average of the "within" and "pooled" estimators, with weights determined by relative precision. In effect, within-country relationships are shrunk towards the

[23] See the appendix, Table A4. Melander and Öberg (2006) identify the competing effects issue and present an analysis that finds, contrary to prior studies, evidence of flows decreasing over time. They use differenced stocks data for 1976-96.

global mean, depending on precision of the within estimates and the overall relationship between deaths and refugee flows (Gelman and Hill 2007: chap. 12).

This approach also allows us to directly estimate γ, the elasticity of refugee flow with respect to conflict intensity, because it allows us to include average log conflict deaths for country j as a covariate in the panel analysis. These are perfectly co-linear with the country-fixed effects in model 3, but "random effects" models make an additional assumption that allows estimation of measured country-specific factors separately from unmeasured country-specific factors.[24]

As seen in Table 2, the results are very similar to Table 1. The estimates for $\log d_{jt}$ are the mean of the $\hat{\beta}_j$'s, and range from .42 to .45; the coefficients on $\overline{\log d_{jt}}$, which are estimates of γ in the structural model, are close to the corresponding cross-sectional estimate $(\hat{\beta}+\hat{\gamma})$ minus the estimate mean of $\hat{\beta}_j$. The difference between columns 1 and 2 is that the second allows random coefficients for $\log d_{jt}$, whose variation is examined next. Column 3 shows that the results are scarcely affected by controlling for pre-conflict refugee flows.

Figure 2 displays the $\hat{\beta}_j$ elasticities for the countries with more than 1,000 UCDP conflict deaths total, based on the model in Table 2, column 2. It shows that most conflicts have estimated elasticities not too far from the overall mean, and large ranges of uncertainty that include the global mean for all but a few cases.

In the structural model, the β_j's indicate how the share of country population directly affected by conflict violence varies with conflict deaths in a year. By this interpretation, we would expect the overall size of the country and covariates related to how a civil war is fought to affect β_j, but not covariates that are external to the country, like distance from an OECD country. The larger the country, the smaller the share affected by a given number of conflict deaths. And one might expect ethnic conflicts to produce more conflict-affected people per death, because some ethnic conflicts involve ethnic cleansing campaigns.

Table A5 in the appendix shows the mixed model results where we examine the effect of total

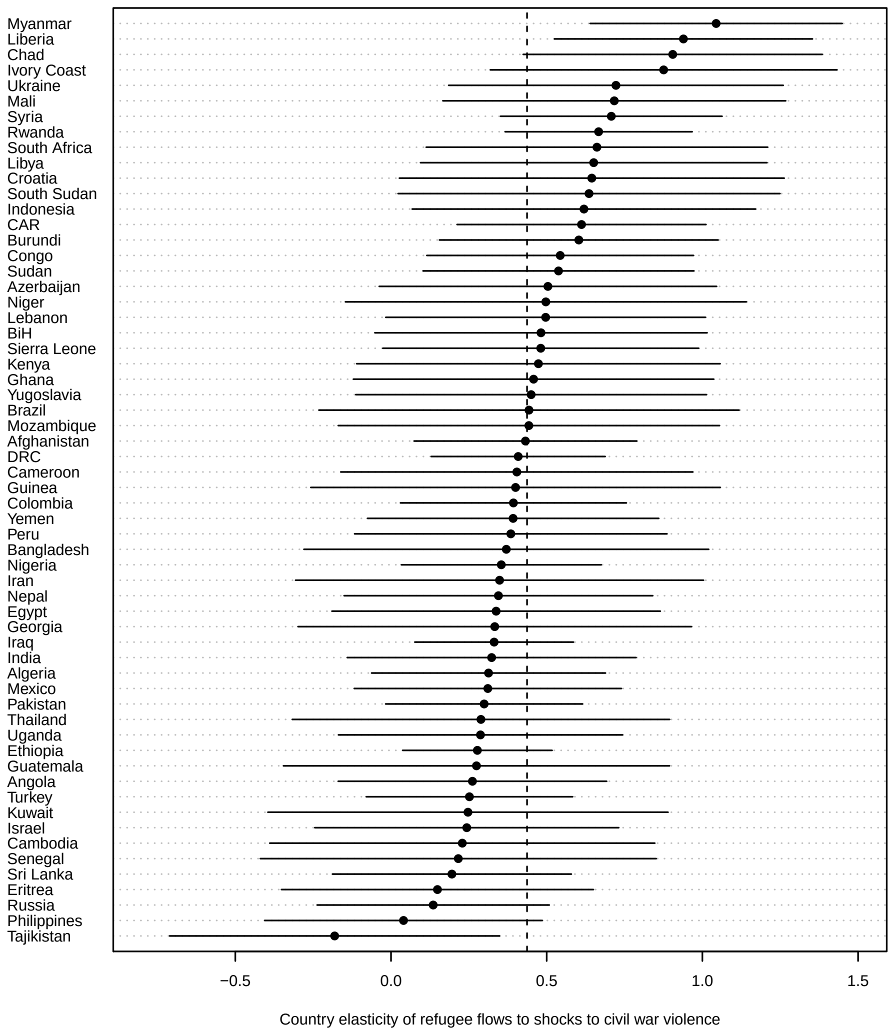
[24] The additional assumption is that unmeasured country-specific factors causing refugee flows (the α_j) are uncorrelated with the other covariates, here country-year deaths d_{jt} (e.g., Wooldridge 2010: 252). Our theoretical argument implies that the α_j are positively correlated with d_{jt} through the effect of conflict intensity on decisions to flee, so that we either need to use country-fixed effects as in Table 1 or include mean $\log d_j$ as a country-level covariate in the mixed model.

Table 2: Mixed model elasticity estimates

	$\log r_{jt}$		
	(1)	(2)	(3)
$\log d_{jt}$	0.416^{***}	0.451^{***}	0.419^{***}
	(0.043)	(0.070)	(0.104)
$\overline{\log d_{jt}}$	0.758^{***}	0.682^{***}	0.540^{**}
	(0.176)	(0.184)	(0.216)
$\log(pop)$	0.068	0.079	-0.066
	(0.125)	(0.126)	(0.166)
$\overline{\log r_{jt}\|prewar}$			0.488^{***}
			(0.096)
N	738	738	265
N(countries)	76	76	43

Notes: d_{jt} = deaths$_{jt}$, r_{jt} = refugees out$_{jt}$. Overbars denote means within countries. Model 1 has a random intercept; models 2 and 3 have both random intercepts and random coefficients for $\log d_{jt}$. Mean values are displayed. Sample restricted to conflict years, defined by UCDP $d_{jt} > 99$, in countries with at least one such year. All data is for 1990-2017. Population is measured in year t. Standard errors in parentheses. *p $< .1$; **p $< .05$; ***p $< .01$

Figure 2: Heterogeneity in refugee flow response to conflict (2 se error bars)



conflict deaths, per capita income, population, war types (center- versus autonomy-seeking rebels, and ethnic versus non-ethnic), distance from an OECD country, and region dummies, on the $\hat{\beta}_j$'s. Only log population comes out statistically significant, with a negative sign. Except for ethnic conflicts and subSaharan Africa – both have higher refugee sensitivity to conflict deaths – the other estimates are substantively close to zero. This could be because they are unrelated and these elasticities are fairly constant across conflict, or it could be that we just do not have enough information to tell (the variance of our estimates of $\hat{\beta}_j$'s is large because there are not many conflict years per country, on average).

6 How many refugees per conflict death?

The results so far suggest that conflict violence has a large total impact on refugee flows – roughly constant returns to scale – that is dramatically underestimated if one tries to identify the impact using shocks to violence levels within conflicts.[25] The theory of refugee production that underlies the structural model (1) holds that year-to-year shocks in violence affect the number of who consider flight, while past destruction and expected future conflict intensity additionally influence the share of directly affected people who choose to become refugees. Within-country variation in violence levels differences out the influence of the latter channel.

Elasticities measure the sensitivity of one variable with respect to another, in percentage terms. For some policy purposes it can be more useful to have estimates of raw *rates* of refugee outflows rather than elasticities. How many refugees will be produced, on average, per conflict death, and how do these rates vary with different kinds of conflicts and other observable factors?

Both within and across conflict countries, the number of refugees per conflict death is highly variable. Some years within a conflict have very high rates, and some conflicts have remarkably high average rates per year. We computed the median and average country-year rates for the conflict years in each of our 76 conflict-affected countries from 1990 to 2017. Table 3 summarizes the

[25] As noted in the Introduction, other sources of underestimation in the existing literature include controlling for factors that influence refugee outflows but are caused to a great extent by armed conflict, like economic decline during civil war; measurement error issues stemming from using differenced stocks to estimate outflows; and categorical indicators of conflict.

distributions of these 76 values, indicating, for example, that the average country had an average annual rate of 31 refugees per conflict death. By contrast, the median country had an average annual rate of 18 refugees per conflict death. Note also that the variation in rates is quite large. One quarter are estimated to have mean annual rates less than 6, while in the highest quartile there were more than 43 refugees per UCDP death. We consider correlates of this variation below.

Table 3: Refugees per conflict death

	25th pctile	Median	Mean	75th pctile
Median rate	4	9	22	25
Average rate	6	18	31	43
Avg. rate (mixed model)	9	18	28	41

Notes: Sample restricted to countries with at least one year with more than 99 UCDP conflict deaths, conflict years only. Number of countries is 76, with 759 total country-year observations.

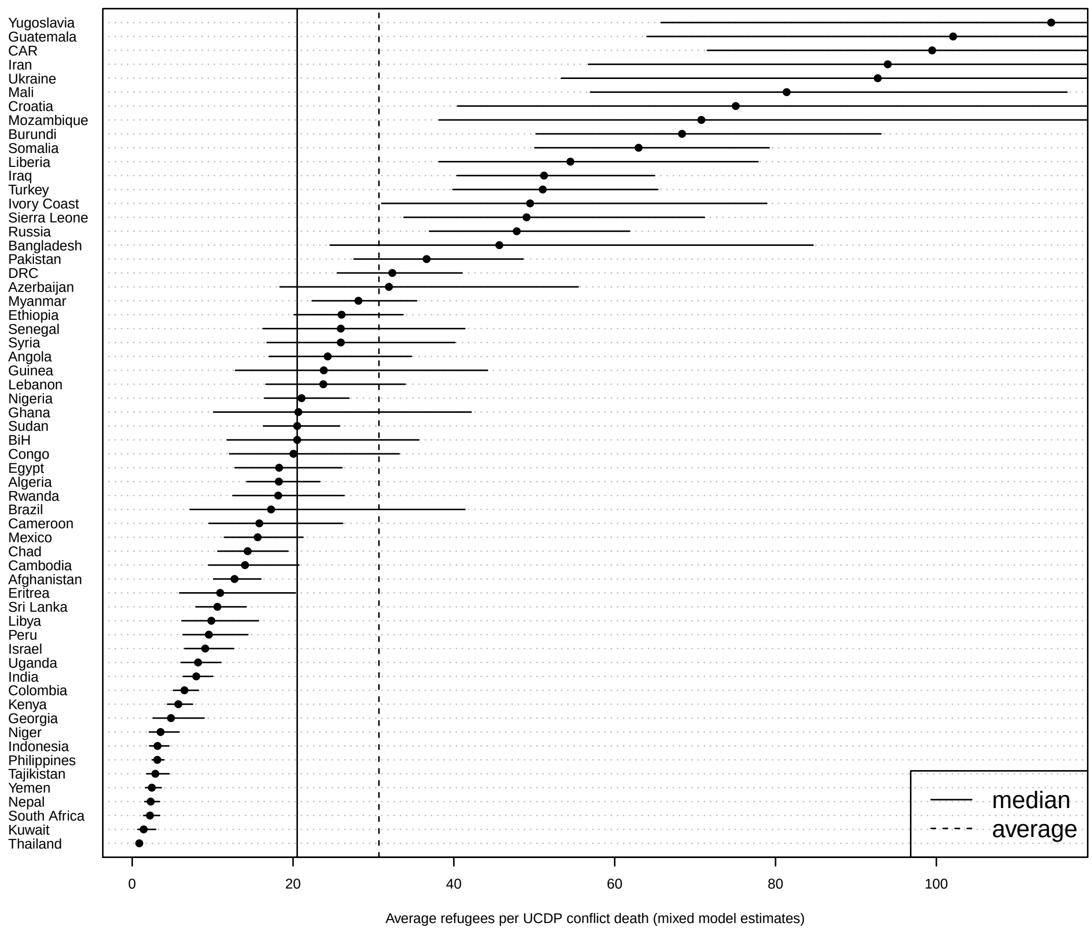
Whereas elasticity estimates are robust to systematic over- or under-counting of conflict deaths and refugees, this is obviously not the case for rates. As noted, if the UCDP method undercounts conflict deaths by, say, 10% on average, then the rate estimates will be about 10% too high. Nonetheless, if we take UCDP's criteria as a measurement practice that can be applied consistently across cases, these results allow for projections of how the scale of a refugee crisis will vary with the scale of the conflict.[26]

Figure 3 displays estimates of the mean rate for the 60 major conflict countries (at least 1,000 killed, total), ordered by total rates. These rates are estimated using a mixed model, again because some countries have few conflict years and thus extreme and high variance raw means.[27]

[26]South Sudan has the highest rates in our data, averaging 260 refugees per UCDP conflict death for 2011-2017. We believe that this is a substantial overestimate stemming from UCDP's laudably careful and conservative coding procedures, which may nonetheless tend to understate deaths for some large conflicts where journalists cannot travel safely and massacres are so common that reports describe them in vague terms rather than with specific numbers. UCDP currently has a total of just under 10,000 conflict deaths for 2011-2017, in contrast to 190,000 violent deaths for December 2013 through April 2018 estimated by Checchi et al. (2018) in a mortality study. Even if these are too high, the UCDP estimates could easily be off by an order of magnitude for this case. We drop South Sudan as an outlier for Figure 3.

[27]See the appendix for details. In brief, we use a random intercept model for $\log r_{jt}/d_{jt}$, whose distribution is approximately Normal, and then convert back to rate using $\exp(\hat{\alpha}_j + \hat{\sigma}_j^2/2)$, where $\hat{\sigma}_j^2$ is estimated from a mixed model for the within-country variance of log annual rates. This results in plausible smoothing for some extreme cases, although overall results below are the same if we just use the actual mean rates (the correlation with the mixed model estimate is .95).

Figure 3: Refugees per UCDP-conflict death, by country (1 se error bars)



What features distinguish the countries that had the largest numbers of refugees relative to conflict deaths? Drawing again on the theory of section 2, by subtracting $\log d_{jt}$ from both sides of model (2) and taking averages within countries as we did before, we can obtain an estimation model for average log rate by country j, as

$$\overline{\log r_{jt}/d_{jt}} = \alpha_j + (\beta_j - \gamma_j - 1)\log d_j + (1 - \beta_j - \gamma_j)\log n_j + \epsilon_j.$$

Thus factors that affect the mean log rate of refugee outflow in a conflict country are captured in the intercept α_j, while average conflict deaths and country population are predicted to have a statistically null relationship with log rates (since we have seen that $\hat{\beta} + \hat{\gamma} \approx 1$).

Table 4 estimates this specification with a mixed model, showing how several country-level attributes associate with different average rates of refugee outflows per conflict death. Rates are significantly lower the farther the conflict country is to a high-income country (omitting Japan, which accepts very few refugees). Proximity to an OECD country is likely a "pull" factor (Turkoglu and Chadefaux 2019; Hatton 2016), both in terms of refugee services and future economic prospects. It is interesting that proximity to a rich country matters for rates but not the elasticity β_j. This is consistent with the theory, according to which the latter reflects conflict technology in a country (sensitivity of number of conflict-affected to a given amount of killing). Proximity, by contrast, would have no effect by itself on conflict technology but does affect everyone's reservation values for leaving the country. We also see a weak tendency for rates to be higher for poorer countries, which might have a similar interpretation.

When a full set of region dummies is included (not shown), Asia has markedly lower rates on average while the estimates for the other regions are all similar, so we include only the Asia indicator in the Table. The coefficient is not meaningfully diminished when distance to OECD is also in the model, so it is not just a matter of being far from Europe.

There is some indication that ethnic and separatist wars have had higher rates (versus non-ethnic and center-seeking war, respectively), although the estimates are not statistically significant. A causal relationship would make sense given that ethnic wars sometimes involve wholesale ethnic cleansing campaigns, which can make for very large rates. For example, the high rate for Yugoslavia seen in Figure 3 is driven by the Kosovo war, which involved a mass expulsion of Albanian speakers, and the high rate for Myanmar is driven by the violent 2017 mass expulsion of Rohingya. Similar considerations may hold for Central African Republic, Ukraine, Croatia, Burundi, and Iraq.[28]

Table 4: Correlates of refugees per conflict death

	Dependent variable is $\log rate_{jt} = \log r_{jt}/d_{jt}$					
	(1)	(2)	(3)	(4)	(5)	(6)
Asia	-0.995^{**}					-0.924^{*}
	(0.449)					(0.504)
Dist. to 1990 OECD - Japan		-0.199^{**}				-0.209^{**}
		(0.082)				(0.098)
Ethnic war			0.249			0.173
			(0.221)			(0.234)
Separatist war				0.265		0.190
				(0.251)		(0.271)
$\log(gdp_{1990})$					-0.128	-0.366^{**}
					(0.142)	(0.146)
$\log(pop_{1990})$	0.077	0.007	0.038	0.055	0.011	-0.091
	(0.157)	(0.157)	(0.159)	(0.162)	(0.187)	(0.173)
$\overline{log(d_{jt})}$	0.208	0.153	0.073	0.054	0.090	0.256^{**}
	(0.128)	(0.118)	(0.115)	(0.122)	(0.120)	(0.126)
N(groups)	75	75	75	75	74	74
N	737	737	735	737	730	728

Notes: Mixed model displaying how the covariates shift the intercept $\hat{\alpha}_j$, by country. Distance is to closest 1990 OECD country (omitting Japan) in 1000 kms. Income and Population are for 1990 or first year with data in 1990-2017. Sample restricted to conflict years of countries with at least one year with UCDP deaths ≥ 100. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

7 Conclusion

An analogy to a very different setting helps to illustrate our main theoretical point. Suppose you learn that your salary will increase by 20% this year, to a new baseline for subsequent years.

[28]See appendix for more details on variables and estimation for Table 4. Distance is capital-to-capital distance to nearest 1990 OECD country omitting Japan (which is hard to reach and not very accepting of refugees). Results are little different if we use all OECD countries. Ethnic wars are coded based on updating the codings from Fearon and Laitin (2003); separatist (or autonomy-seeking) conflicts are coded using UCDP's "territorial" incompatibility indicator, based on the most lethal conflict for countries that had multiple types in a given year. Income and population are logged World Bank values (income in constant 2010 PPP dollars) for 1990 or the first year with data.

Anticipating this change in "permanent income" (Friedman 1957) you increase your consumption spending by some amount, say 15%, this year and going forward. By contrast, suppose you learn that you are getting a 20% income windfall this year, but without any implication for your permanent income. You decide to spread this over multiple years, so your consumption increases annually by less than 15%.

So if we try to estimate the effect of income on consumption by using random shocks to individuals' average incomes, we will estimate a local average treatment effect that does not generalize to the effect of a change in permanent income. This is because spending depends in part on anticipation of future income flows, and random year-to-year shocks do not capture the impact of changes in expectation of future levels.

Likewise, consider the decision of whether to become a refugee in a year when civil war violence has escalated by, say, 100% over the previous year. The impact on total outflows in that year will depend on whether people think this is idiosyncratic variation around an expected level that has not changed, or if the increase represents a change to a new, higher "permanent" level. The size of the refugee exodus will be greater in the latter case. The same logic applies to comparisons across conflicts. *The effect of violence shocks around the mean within a conflict is different from the effect of variation in the mean across conflicts*, or of changes in anticipated future violence within a conflict.

With this point in mind, our main empirical results can be summarized graphically by Figure 4. The x axis measures average conflict intensity using average annual UCDP conflict deaths (for years with at least 100), on a log scale. The y axis has average annual refugee outflows in conflict years. The solid line is the regression line from Table 1, model 4, showing the roughly constant-return-to-scale estimate for average level of conflict intensity (slope = 1.07). It implies, for example, that tripling the intensity of the civil war associates on average with about triple the number of refugees.[29]

Based on the country-and-year fixed-effects panel model of Table 1, model 1, the dashed line

[29] To make the graph easier to intepret, we label the axes with $\exp(y_j)$ and $\exp(x_j)$ instead of the logged values, which means that these are slightly lower than the actual values (by an amount that depends on variance of refugee and conflict death numbers year-to-year in the country).

shows the average impact of year-to-year, within-conflict variation in conflict intensity. Its slope is .42, indicating that a violence shock that, say, triples the number killed in one year relative to the average associates with an increase in refugee outflows in that year by a factor of about $3^{.42} = 1.59$. The plot maps the effect for a conflict with average intensity of 1,000 deaths per year, but the line could be shifted up or down to any average intensity level.[30] To illustrate in the case shown, a one-year escalation to 3,000 killed would associate with an one-year increase from about 10,000 refugees to about $10{,}000 * 1.59 = 15{,}900$ refugees that year.

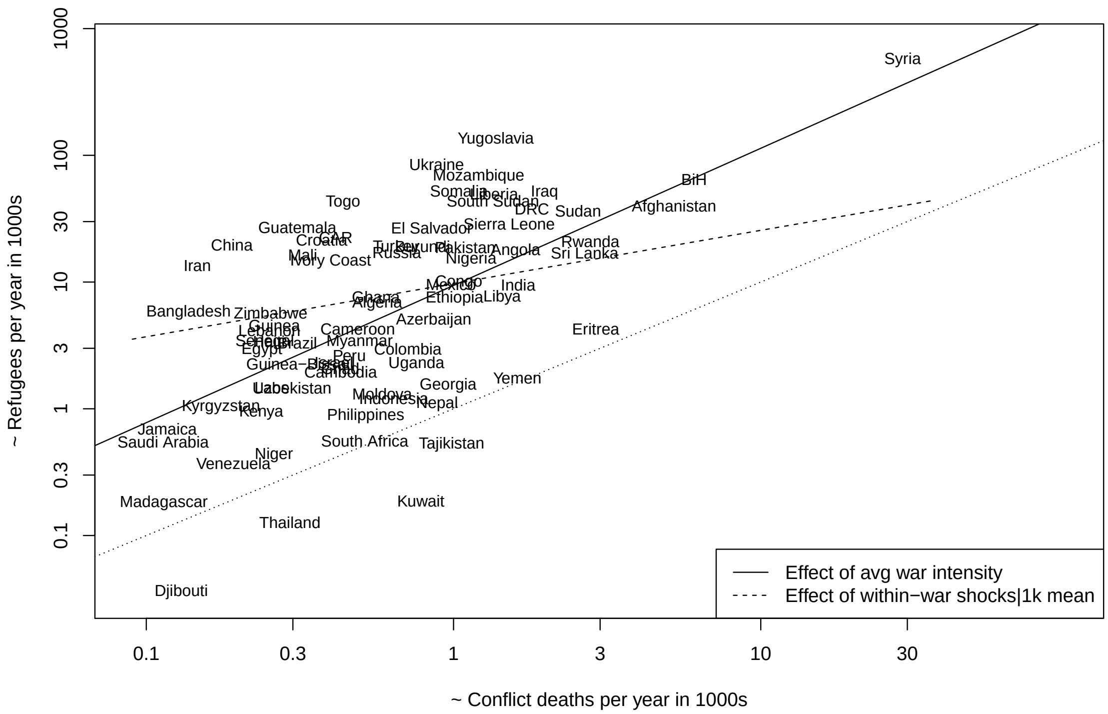
The analysis suggests a broader methodological point. It has become common to seek well-identified estimates of causal effects in social science settings by using plausibly random shocks to the value of possible causal factor. The example of civil war violence and refugee flows shows that average treatment effects estimated from shocks can be highly misleading. If the response variable (here, refugee flows) is the result of choices that depend on both prior and expected levels of the causal factor (here, violence) – or more generally if the causal effect of variation in average levels is different from the effect of shocks for whatever reason – then the reduced-form shock-based estimate may be misunderstood if we don't have a more structural model of the data-generating process.[31]

The global refugee crisis of the last 10 years is widely understood to be closely related to civil war and "state fragility" in developing countries. It is perhaps less well known, and less discussed, that the sources of the crisis are *highly* concentrated. As noted in the Introduction, just eight countries are responsible for half of all refugee outflows since 1990. Since 2010, in eight years when the total number of refugees more than doubled and refugees-plus-IDPs increased by a factor of 2.7, just three countries account for half of all new refugees – Syria, Afghanistan, and South Sudan. Improving policies concerning the treatment of refugees and the management of political and economic impacts in host countries should clearly be a high priority. But the concentration of

[30] There is a weak tendency for higher slopes in higher intensity conflicts, but the interaction effect is far from statistically significant. If we compare the estimated within elasticity for countries above and below the mean, both are almost exactly .42.

[31] Some examples: Estimating the effect of democracy on economic growth rates using idiosyncratic transitions; estimating of the impact of climate change or levels of climate variables on conflict using short-run random variation in rainfall or temperature; estimating effects of government repression using some source of random, short-run variation.

Figure 4: Impact of violence shocks and violence levels on refugee flows



the sources of the crisis should remind us that the marginal returns to international coordination on ending civil wars and peacekeeping in collapsed states may be very high (Doyle and Sambanis 2000; Fortna 2008; Fortna and Howard 2008).

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8 Online Appendix

8.1 Within-country measurement error in conflict deaths

Suppose that in truth the average β_j equals a number $b > 0$, the average γ_j equals zero, and the difference between the panel and cross-sectional estimates observed in the models of Table 1 is entirely due to measurement error within countries in d_{jt}. How large do b and the ratio of noise to signal in d_{jt} have to be to produce $\hat{\beta}_{panel} \approx .42$ and $\hat{\beta}_{cs} \approx 1$?

We simulate log refugee flows $\log \tilde{r}_{jt}$ using

$$\log \tilde{r}_{jt} = \alpha + [b + \hat{\beta}_j - \text{mean}(\hat{\beta}_j)] \log d_{jt} + \epsilon_{jt},$$

where

- the $\hat{\beta}_j$'s were those estimated from the mixed model in Table 2, column 2;[32]
- d_{jt} are the actual UCDP annual death estimates;
- ϵ_{jt} is drawn from a Normal distribution with the same variance as in the mixed model estimated on the actual data;
- and α is the mean of $\log r_{jt}$ in the estimation sample (that is, for all countries).

By construction, with this simulated data b is now the elasticity of refugee outflows with respect to conflict deaths, both within and across conflicts ($\gamma = 0$). Table A1 illustrates the estimates from one draw of ϵ_{jt} for this model, using $b = 1$. Notice that the estimates are close to 1 for both the panel and cross-section, which is the result of $b = 1$ and zero correlation between mean log levels of refugee flows and mean log levels of conflict deaths within countries (implied by $\gamma = 0$).

We now add random noise to $\log d_{jt}$, drawn from a mean-zero Normal distribution with standard deviation equal to $m \geq 0$ times the actual variance of $\log d_{jt}$. The share of the variation that is signal in the new, simulated deaths estimate is thus $1/(1 + m)$. Setting b at different initial values,

[32] To be specific, the model was estimate with the R package `lme4` using `t.betaj = lmer(lref ~ (1 + logdjt|ccode) + logdjt + meanlogdjt + log(wbpop) + factor(year),data=d,subset = i)` .

Table A1: Simulated model with $\beta_{panel} = \beta_{cs} = 1$ and $\gamma = 0$

	$\log \tilde{r}_{jt}$	$\overline{\log \tilde{r}_{jt}}$
	(1)	(2)
$\log d_{jt}$	1.069***	
	(0.057)	
$\overline{\log d_{jt}}$		0.981***
		(0.163)
constant		3.977***
		(0.461)
N	766	77
R^2	0.807	0.324

Notes: d_{jt} = actual deaths$_{jt}$, $\tilde{r}_{jt}$ = simulated refugees out$_{jt}$. Overbars denote means within countries. Model 1 is panel data with year fixed effects (not shown). Model 2 is the corresponding cross-sectional model. This is an example of a single draw. Standard errors in parentheses. *p < .1; **p < .05; ***p < .01

we do this 50 times (the estimates don't vary much so the number of simulations is not important), computing the models in Table A1 each time.

For any $b > 0$, we find that the share of signal in the deaths measure has to be about 30% ($m = 2.3$) to get a ratio of $\hat{\beta}_{cs}/\hat{\beta}_{panel} \approx 1/.42 = 2.4$, as in the observed results in Table 1. Table A2 shows simulation results with $b = 2$.

It is highly unlikely that the within-conflict (year-to-year) error in UCDP's death estimates is this large. It is also seems implausible that the true *average* elasticity for a one-year shock could be as large as 2, which would imply that a one-year doubling of conflict deaths causes refugee flows to increase by a factor of three. (Note that if measurement error explains a large part of the panel-versus-cross-section difference seen in Table 1, then it must also be that the true effect is being underestimated, and so is greater than 1.) Finally, this simulation has assumed zero measurement error in conflict deaths across countries, which would produce attenuation bias in the cross-section but not the country-fixed effects panel.

It is certainly possible, indeed likely, that there is some measurement error in the death estimates, both year-to-year within countries and affecting average levels across countries. So there may well be some downward bias in both the panel and cross-sectional estimates. But it is not likely that differential attenuation explains the observed gap between them.

Table A2: Measurement error simulation with $b = 2$

m	% signal	$\hat{\beta}_{panel}$	$\hat{\beta}_{cs}$	$\hat{\beta}_{cs}/\hat{\beta}_{panel}$
0.00	1.00	1.98	2.04	1.03
0.20	0.83	1.48	1.87	1.26
0.40	0.71	1.19	1.73	1.46
0.60	0.62	1.00	1.57	1.58
0.80	0.56	0.85	1.47	1.73
1.00	0.50	0.74	1.41	1.91
1.20	0.45	0.66	1.30	1.96
1.40	0.42	0.60	1.19	1.98
1.60	0.38	0.54	1.14	2.12
1.80	0.36	0.50	1.11	2.23
2.00	0.33	0.45	1.04	2.30
2.20	0.31	0.43	1.01	2.37
2.40	0.29	0.40	0.96	2.41
2.60	0.28	0.37	0.91	2.47
2.80	0.26	0.35	0.86	2.47
3.00	0.25	0.33	0.84	2.57

8.2 Different death thresholds

Table A3 reproduces Table 1 using two different thresholds for the inclusion of conflict countries and conflict years. Columns 1-4 show results when we consider countries with total UCDP conflict deaths of at least 1,000 between 1990 and 2017, a common (if necessarily somewhat arbitrary) threshold for coding "civil war." This reduces the number of countries in the sample to 61. Columns 5-8 use this same total threshold, but lowers the threshold for a "conflict year" to 25 or more UCDP-coded deaths, which substantially increases the number of conflict years included, but also reduces the number of countries for which we can code pre-war refugee flows (column 8). (Note that Bougainville conflict in Papua New Guinea had fewer than 1,000 deaths and so does not appear in these models.)

Table A3: Civil wars and refugee flows. Elasticities. Different death thresholds.

	$\log r_{it}$		$\overline{\log r_{it}}$		$\log r_{it}$		$\overline{\log r_{it}}$	
	panel		c-s		panel		c-s	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\log d_{it}$	0.417***	0.388***			0.328***	0.282***		
	(0.059)	(0.066)			(0.040)	(0.044)		
$\log d_{it-1}$		0.052				0.050*		
		(0.035)				(0.028)		
$\overline{\log d_{it}}$			0.935***	0.826***			0.950***	0.895***
			(0.195)	(0.235)			(0.136)	(0.184)
$\log(pop)$			0.011	−0.126			−0.079	−0.356*
			(0.144)	(0.215)			(0.125)	(0.204)
$\overline{\log r_{it} \vert prewar}$				0.395***				0.446***
				(0.110)				(0.119)
constant			2.509	2.919			4.314*	6.409*
			(2.880)	(4.142)			(2.257)	(3.657)
N	746	743	61	29	961	956	61	22
R^2	0.716	0.740	0.287	0.510	0.704	0.735	0.461	0.748

Notes: d_{it} = deaths$_{it}$, r_{it} = refugees out$_{it}$. Columns 1-4 are countries with at 1,000 total UCDP deaths, and conflict years with at least 100 deaths. Columns 5-8 are the same countries but including any year with at least 25 killed. Panel models include country and year fixed effects, and cluster errors by country. All data is for 1990-2017. Standard errors in parentheses. *p < .1; **p < .05; ***p < .01

8.3 Does $\hat{\beta}_j$ change over time as a conflict proceeds?

No. See Table A4.

Table A4: $\hat{\beta}_j$ over conflict duration

	$\log r_{jt}$	
	OLS	*mixed model*
	(1)	(2)
$\log d_{jt}$	0.44200***	0.44834***
	(0.08206)	(0.06329)
# previous conflict years	−0.01391	0.00181
	(0.04849)	(0.04073)
$\log d_{jt}$*# prev. conflict yrs	−0.00287	−0.00397
	(0.00510)	(0.00507)
$\overline{\log d_{jt}}$		0.77586***
		(0.18039)
$\log(pop)$		0.07799
		(0.12588)
constant		−0.70447
		(2.38843)
Observations	766	738
R^2	0.734	

Notes: d_{jt} = deaths$_{jt}$, r_{jt} = refugees out$_{jt}$. Overbars denote means within countries. Model 1 is OLS with country and year fixed effects. Model 2 is a linear mixed effects model with random intercepts and random coefficients for $\log d_{jt}$ (by country), with year dummies not shown. Mean values are displayed. Sample restricted to conflict years, defined by UCDP $d_{jt} > 99$, in countries with at least one such year. All data is for 1990-2017. Population is measured in year t. Standard errors in parentheses. *p < .1; **p < .05; ***p < .01

8.4 Covariates and refugee elasticity with respect to conflict deaths

Table A5 reports the effect of several covariates on $\hat{\beta}_j$, the estimate of elasticity of refugee flow in country j to conflict deaths d_{jt}, using a mixed model. The covariates are:

- war intensity, measured by average log UCDP deaths over the entire conflict;
- log of income per capita in 1990 (or first available year), using the World Bank's WDI constant dollar series;
- log of country population in 1990, using data from the World Bank;
- an indicator for whether the rebel group in the largest conflict in the country year sought to capture the central government or sought independence or greater autonomy in a region, using the UCDP "incompatibility" indicator;
- an indicator for whether the rebel group(s) in the largest conflict in the country year mobilize fighters primarily along ethnic or ethno-religious lines, or represent themselves as fighting on behalf of an ethnic or ethno-religious group;[33]
- Distance in kilometers between the country and the nearest 1990 member of the OECD, omitting Japan (which is remote and accepts very few refugees);
- region indicators.

The mixed model may be described as follows in the case of a single covariate.

$$\log r_{jt} \sim N(\alpha_{j[t]} + \beta_{j[t]} \log d_{jt}, \sigma_\epsilon^2) \tag{5}$$

$$\begin{pmatrix} \alpha_j \\ \beta_j \end{pmatrix} \sim N \left(\begin{pmatrix} \gamma_0^\alpha + \gamma_1^\alpha x_j \\ \gamma_0^\beta + \gamma_1^\beta x_j \end{pmatrix}, \begin{pmatrix} \sigma_\alpha^2 & \rho\sigma_\alpha\sigma_\beta \\ \rho\sigma_\alpha\sigma_\beta & \sigma_\beta^2 \end{pmatrix} \right), \tag{6}$$

where α_j and β_j are random intercept and elasticity coefficients estimated for country j; the γ^α's allow the α_j's to vary with the covariate x_j; and the γ^β's likewise estimate the relationship between the covariate x_i and the elasticity β_i for country i. The model also needs to estimate three variance terms (σ_j^2, $j \in \{\alpha, \beta, \epsilon\}$), and the α-β correlation ρ, for a total of 10 parameters.[34]

[33] We updated the coding in Fearon and Laitin (2003) to 2017, applying it to the UCDP conflicts in this period.

[34] Two of the covariates, for center-seeking and ethnic wars, are in fact time varying, so the term should be written x_{jt} for these. In fact there is very little within-country variation in these measures.

The coefficient of interest, and what is reported in Table A5, is γ_1^β, the marginal effect of the covariate x_i on the country-specific elasticity.

The specific estimation is done with the `R` package `lme4` using `lmer(lref` $\sim$ `(1 + logdjt|ccode) + logdjt*`x_j `+ meanlogdjt + log(wbpop) + factor(year),data=d,subset = i)`.

Table A5: Country/conflict features and refugee elasticity with respect to civil war deaths

	DV: $\hat{\beta}_i$ = est. elasticity of refugee outflow wrt to conflict deaths							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\overline{\log d_{it}}$	-0.024							-0.089
	(0.072)							(0.102)
$\log(gdp_{1990})$		-0.085						-0.094
		(0.055)						(0.080)
$\log(pop_{1990})$			-0.096^{**}					-0.133^{**}
			(0.049)					(0.064)
Center war				0.036				0.157
				(0.134)				(0.160)
Ethnic war					0.192			-0.008
					(0.121)			(0.186)
Dist. to 1990 OECD - Japan						0.008		-0.012
						(0.037)		(0.048)
E. Europe							-0.143	
							(0.266)	
L. Amer./Carib.							-0.135	
							(0.289)	
MENA							-0.005	
							(0.215)	
subSah. Africa							0.248	
							(0.186)	
N(groups)	76	75	76	59	76	76	76	58
N	738	731	738	628	736	738	738	621

Notes: Hierarchical model estimates of covariate(s) effect on elasticity of refugee outflow wrt to UCDP death estimate. GDP per capita and population are from 1990 or first year with data. Center war means war over central government (vs. autonomy-seeking conflict). Distance is to closest 1990 OECD country (omitting Japan) in 1000 kms. Sample restricted to civil war countries. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$.

8.5 Estimating rates of refugees per UCDP conflict death

For Figure 3, we first estimate a mixed model for log rate, with just a random intercept:

$$\log r_{jt}/d_{jt} = \alpha_{[j]t} + \epsilon_{jt},$$

from which we can extract "regularized" estimates of the mean log rate (refugees per conflict death) in each conflict country. To convert these to simple rates (not logged), we cannot just exponentiate, since $\exp(E(\alpha_j)) \neq E(\exp(\alpha_j))$. Instead, we use the fact that if $\log y$ has Normal distribution with the mean $\overline{\log y}$ and variance σ^2, then $E(y) = \exp(\overline{\log y} + \sigma^2/2)$. Let $e_{jt}^2 \equiv (d_{jt} - \overline{d_{jt}})^2$. We estimate a regularized $\hat{\sigma}_j^2$ with the mixed model

$$e_{jt}^2 = \sigma_{[j]t}^2 + \delta_{jt},$$

and use the $\hat{\sigma}_j^2$'s to compute a rate for each conflict country. The standard error bands shown in the Figure are from the country-specific se's for the random intercepts in the model for $\log r_{jt}/d_{jt}$. Figure A1 shows that there is just a little smoothing at the tails going on. This is not relevant for the analysis of correlates of log rates, but relevant for a few of these tail cases for estimated unlogged rates as in Figure 3.

Table 4 is estimated by

$$\log r_{jt}/d_{jt} = \alpha_{[j]t} + \beta_0 x_j + \beta_1 \overline{\log d_{jt}} + \beta_2 \log pop_{1990} + year_t + \epsilon_{jt},$$

where x_j is the covariate in question; β_0 is the average effect on the log rate (shown in table); $\alpha_{[j]}t$ is a random intercept by country; and $year_t$ are year dummies.

Figure A1: Mixed model log rates vs. raw log rates