

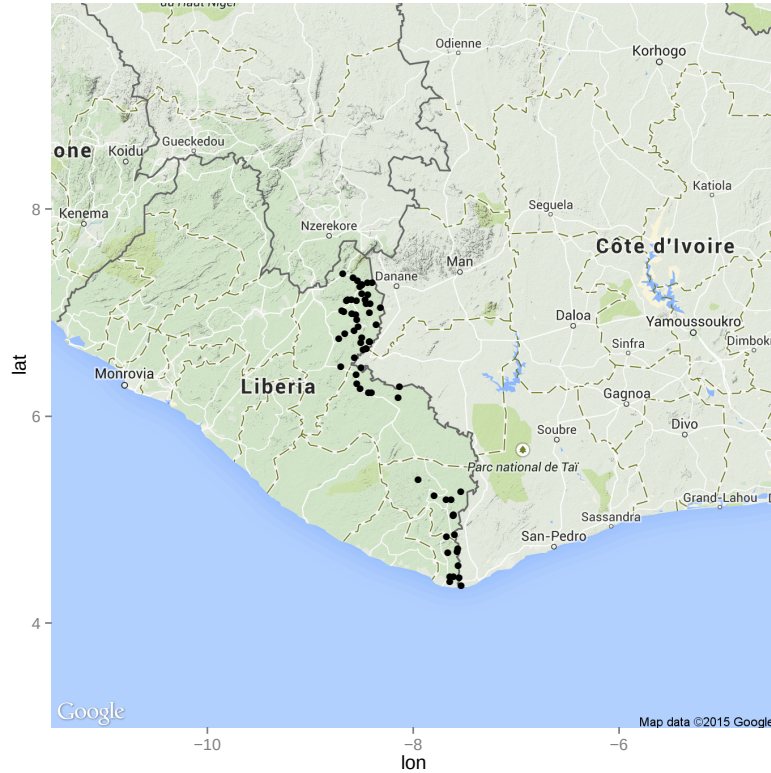
Violence, Empathy and Altruism: Evidence from the Ivorian Refugee Crisis in Liberia

Online Appendix

1	Border region and sample	2
2	Conjoint experimental design	4
3	Selection into violence and sensitivity analysis	5
3.1	Historical accounts of violence	5
3.2	Balance	5
3.3	Sensitivity to unobservables	6
4	Supplemental analyses	10
4.1	Main effects, controlling for index of prosocial behavior	11
4.2	Social norms and hosting	13
4.3	Selective migration	14
4.4	Generalizability	15
4.5	Non-association of violence with past refugee experience	16
5	Figures as Tables	16
5.1	Results from Figures 1 & 2	16
6	Qualitative interview guide	18

1 Border region and sample

Figure 1.1: Map of the border region and study communities



Our sample consists of i) 32 villages selected by the NRC to receive an economic recovery program after the crisis,¹ ii) 32 “comparison villages” selected using pre-treatment census data and Mahalanobis matching, and iii) 16 villages selected by the Government of Liberia to serve as “host villages”, where refugees were encouraged to settle and aid agencies were encouraged to channel their relief efforts while more permanent camps were being constructed.

Appendix Table 1 uses 2008 Census data to compare in-sample communities to the remaining population of rural communities in Nimba, Grand Gedeh, River Cess, and Maryland counties. In sample communities are larger on average than the full population of communities, probably because the census includes small communities as distinct communities. The sample is otherwise balanced.

We might be concerned that this program influenced hosting behavior, as host villages were exposed to greater, more diverse numbers of refugees and greater humanitarian services. Three points alleviate our concerns. First, the program began in Spring of 2011, roughly five months after the start of the refugee crisis (when the need for residents to host was most

¹The program, implemented between August 2013 and March 2014, sought to advance the economic recovery of host communities through the provision of agricultural tools and training.

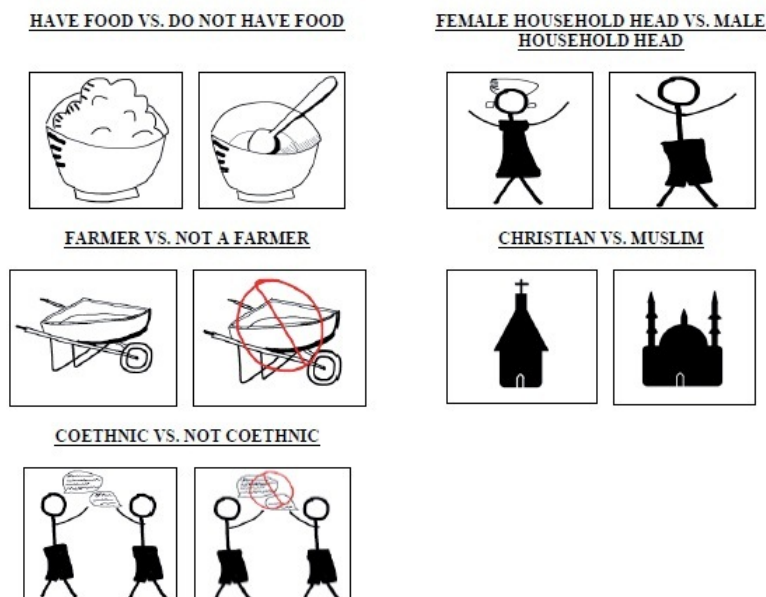
Table 1: Balance between insample communities and out of sample communities

	mean_insample	mean_outsample	mean_diff	pval
Population	775	312	462	0.00
Wealth Index	1.2	1.2	-0.0	0.75
Education	3.1	3.2	-0.1	0.43
Parents alive?	0.5	0.5	-0.00	0.9

In-sample communities are larger on average than the full population of communities, probably because the census includes small communities as distinct communities. The sample is otherwise balanced.

urgent). Second, the definition of hosting used in the survey (accepting refugees to “sleep under your roof and eat from your pot”) is distinct from the type of support offered in the 16 villages program, where separate structures were constructed for refugees. Third, our analysis uses village fixed effects, thereby ensuring that local or regional differences do not account for our results. Unsurprisingly, results are substantively unchanged when excluding host villages.

2 Conjoint experimental design



Pictographs were printed in black and white as 3 x 3 inch squares. Each attribute level was placed back-to-back with its conjugate, then laminated. Each enumerator thus had 10-double sided squares (5 for each profile), and would present them according to the instructions provided by the computer-assisted interview (CAI) device. The picture below displays practice survey enumeration during enumerator training (at the time, only four attributes were being used). After both profiles were laid down, respondents were asked to repeat both profiles to ensure full comprehension. Randomization was conducted via the survey software, and the order in which attributes were presented was randomly assigned to rule out ordering effects.



3 Selection into violence and sensitivity analysis

Underlying our claim that violence has a causal affect on altruistic behavior toward refugees is the assumption that exposure to violence was independent of potential hosting behavior conditional on covariates. In this section, we outline some reasons to believe this assumption holds. Second, we examine sources of selection empirically. Finally, we conducted a sensitivity analysis in the spirit of Imbens (2003).

3.1 Historical accounts of violence

The civil war was characterized by a high degree of indiscriminant violence by undisciplined fighters on all sides of the conflict (Ellis 2001; Lidow 2011). In addition, several factors limited civilians’ ability to avoid violence. First, the flow of information during the conflict was extremely limited due to the total absence of modern information technologies. The non-availability of reliable information limited the extent to which civilians could avoid violence. This was exacerbated by frequent changes in territorial control by various rebel groups (Ellis 2001; Lidow 2011).

Second, travel for civilians was done by foot, and was extremely hazardous. This raised the cost of flight, keeping citizens in dangerous territories. Often, civilians preferred to travel through the bush rather than use the roads, lest they encounter rebels notorious for their indiscipline.

To be sure, there was also a significant degree of targeted violence. Some individuals were targeted based on their ethnicity. Survival at checkpoints was often a matter of whether fluency in the rebels’ preferred dialect (Ellis 2001). Village chiefs and their kin, in particular, were targeted, as well as families of wealth or status. Having a “big belly” at a checkpoint was a distinct risk factor (Ellis 2001).

3.2 Balance

Table 2 examines the correlates of violence exposure empirically. Consistent with the discussion above, the results suggest that relation to the chief, prewar household livestock ownership and prewar formal sector employment are associated with higher levels of violence. However, because these variables are weak predictors of hosting behavior (See, they are unlikely to confound our analysis. We also control for these variables along with socio-demographic indicators in our analysis, and conduct sensitivity analyses to unobserved selection into violence that may correlate with contemporary outcomes.

Table 2: Determinants of Violence Exposure (0-6)

	Exposure to Violence
Intercept	2.07*** (0.36)
Age 25-35	0.25* (0.13)
Age 36-45	0.48*** (0.13)
Age 46-55	0.16 (0.17)
Age 55+	0.03 (0.17)
Related to chief (prewar)	0.24** (0.09)
Father edu	-0.00 (0.01)
Mother edu	-0.01 (0.01)
HH owned livestock (prewar)	0.22** (0.11)
HH had formal job (prewar)	0.44** (0.18)
HH had business (prewar)	0.06 (0.35)
Muslim	0.35 (0.35)
Gio Ethnicity	0.66 (0.43)
Grebo Ethnicity	-0.25 (0.40)
Krahn Ethnicity	0.24 (0.29)
Mano Ethnicity	1.07* (0.61)
Male	0.60*** (0.10)
Born in town	-0.23* (0.13)
R ²	0.16
Adj. R ²	0.11
Num. obs.	1298

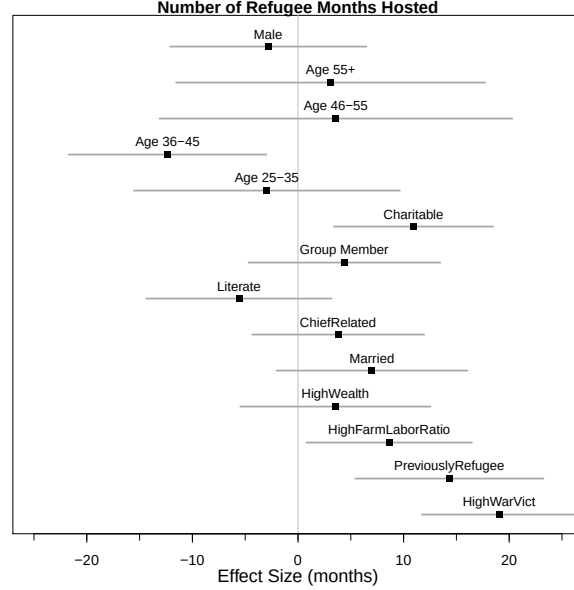
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

OLS estimation with village fixed effects (omitted); standard errors clustered by village

3.3 Sensitivity to unobservables

This section uses selection on observables to assess the potential bias from unobserved omitted variables, following (Oster 2017). The idea is to use the bias eliminated by observed covariates to assess the potential bias of unobserved variables. Formally, consider the following linear regression models:

Figure 3.1: Correlates of hosting (total # of refugee-months hosted)



$$Y = \beta X + \gamma \mathbf{W}_1 + \alpha \mathbf{W}_2 + \epsilon \quad (1)$$

$$Y = \tilde{\beta} X + \tilde{\gamma} \mathbf{W}_1 + \tilde{\epsilon} \quad (1)$$

$$Y = \dot{\beta} X + \dot{\epsilon} \quad (2)$$

where β , the effect of some treatment X , is the coefficient of interest, $\mathbf{W}_1$ is a set of observed control variables, and $\mathbf{W}_2$ is a set of unobserved control variables. Equation (1) refers to the true model and returns an unbiased estimate of β . Equation (2) consists of the full set of observed control variables — i.e. the main estimation equation in the paper. Estimates of $\tilde{\beta}$ will be biased unless $\mathbf{W}_2$ is uncorrelated with either X , Y , or both. Equation (3) is a naive model. Estimates of $\dot{\beta}$ will be more biased than those of $\tilde{\beta}$.

The Oster (2017) approach uses coefficient movements between the naive estimate ($\dot{\beta}$) and the controlled estimate ($\tilde{\beta}$) combined with movements in R-squared values to gauge the degree of potential omitted variables bias. Heuristically, estimates that move little with the inclusion of control variables that cause substantial increases in R-squared are indicative of limited omitted variables bias. The approach relies on two assumptions. The first assumption is the so-called “coefficient of proportionality”, δ , which is degree to which the observed controls ($\mathbf{W}_1$) determine treatment relative to the unobserved ($\mathbf{W}_2$). $\delta = 1$ allows the unobserved controls to be as influential as the observed controls. This assumption is likely to hold when the observed controls are among the strongest determinants of treatment.

The second assumption is the maximum R-squared value (R_{max}^2) from the hypothetical estimation of Equation (1), the true model. R_{max}^2 and $R_{controlled}^2$ (from Equation (2)) determine the explanatory power of unobserved omitted variables after accounting for the observed control variables. In the presence of measurement error or idiosyncratic variation in the outcome, $R_{max}^2 < 1$.

Oster (2017) shows that with assumptions about R_{max}^2 and δ it is possible to use coefficient movements in β between the naive and controlled regressions to calculate the potential bias from omitted variables. This results in an identified set, bounded on one side by the controlled estimate and on the other by the bias-adjusted estimate, that contains the unbiased estimate. A result is deemed robust if the identified set excludes zero.

Note that using coefficient stability between Equations (2) and (3) to argue for causality is equivalent to arguing treatment is *unconditionally* exogenous: $\dot{\beta}$ varies little from $\tilde{\beta}$ because $\mathbf{W}_1$ does not confound. And because $\mathbf{W}_1$ does not confound, $\mathbf{W}_2$ is also unlikely to confound (especially when we believe $\mathbf{W}_1$ constitutes the strongest determinants of treatment). The framework can easily be extended to the case where treatment is believed to be exogenous only after conditioning on a set of control variables, $\mathbf{M}$. In this case, the variables in equations (1)-(3) are first residualized with respect to $\mathbf{M}$ (equivalently, $\mathbf{M}$ is included in equations (1)-(3)).

How to select conservative values for R_{max}^2 and δ ? Oster (2017) re-analyzes experimental studies to identify conservative values of R_{max}^2 and δ under which a non-zero bias-adjusted effect would be consistent with exogenous treatment assignment. These parameter values are then recommended as a robust reporting standard. The intuition of this test follows from the discussion above: observational studies implicitly argue that the treatment is exogenous. Including controls should not change the coefficient because there is no confounding. In experimental studies, this assumption is known to hold. Control variables will still influence the coefficient estimate due to idiosyncratic imbalance across groups. Thus it is possible to use the stability of treatment estimates in randomized data as a guide to how much stability would be expected in observational data if the treatment were assigned exogenously. To do so, Oster (2017) draws on a large sample of randomized studies published in *American Economic Review*, *Quarterly Journal of Economics*, *Journal of Political Economy*, *Econometrica* and the *American Economic Journal Applied Economics* from 2008 through 2013.

Oster 2015 assumes the effects estimated in randomized data are causal and that they should therefore survive the bias-adjustment procedure. Robustness cutoff values are based on the value of R_{max}^2 and δ under which the bias-adjusted effect is distinct from zero in 90 per cent of experimental studies. This leads to the values of $R_{max}^2 = 1.3 \times R_{controlled}^2$ and $\delta = 1$. Substantively, this assumes unobservables explain as much of the variation in treatment as the observables and explain 30 per cent of the variation in the outcome explained by the included controls. For full details, see (Oster 2017).

In our set-up, *violvict* is the effect of interest, $\mathbf{W}_1$ includes the full set of covariates reported in Table 2, including village fixed effects, and $\mathbf{W}_2$ is the set of all unobserved confounders. Our test is conservative in two respects. First, we exceed Oster (2017)’s recommended standards for robustness by setting $R_{max}^2 = 2 \times R_{controlled}^2$ and $\delta = 1$ (rather than $R_{max}^2 = 1.3 \times R_{controlled}^2$ and $\delta = 1$). Substantively, this sets unobservables to be as influential as the full set of control variables (including fixed effects) in explaining both the outcome and treatment. Second, we test the unconditional exogeneity of *violvict* by excluding any baseline controls ($\mathbf{M}$, from above).

The results of this sensitivity analysis are presented in Appendix Table ???. The first column shows the “naive” effect of *violvict* on the outcome, estimated from a bivariate regression. The second column presents estimates of the fully controlled effect, reported in Tables 2 and 3 in the main article. The third and fourth columns show the bias-adjusted

effect and identified set under Oster (2017)'s recommended standards for robustness ($R_{max}^2 = 1.3 \times R_{controlled}^2$ and $\delta = 1$). Under this level of confounding, the identified sets exclude zero. The fifth and sixth columns show the bias adjusted effect and identified set assuming $R_{max}^2 = 2 \times R_{controlled}^2$ and $\delta = 1$. Even under this level of confounding, the identified sets exclude zero. Substantively, the results of this exercise indicate that omitted unobservables would have to be substantially more confounding than observables to reduce effect sizes to zero.

Table 3: Oster Sensitivity Analysis

	Naive effect (Std. Error) [R2]	Controlled effect (Std. Error) [R2]	R2max = 1.3 x controlled R2		R2max = 2 x controlled R2	
			Bias-adjusted effect:	Identified Set	Bias-adjusted effect:	Identified Set
# Refugee Months Host	6.82 (1.41) [0.02]	5.75 (1.43) [0.29]	5.2	[5.2, 5.75]	4.22	[4.22, 5.75]
Hosted Ethnic Outgroup	0.02 (0.01) [0]	0.02 (0.01) [0.28]	0.02	[0.02, 0.02]	0.03	[0.02, 0.03]
Hosted Religious Outgroup	0.01 (0.01) [0]	0.01 (0.01) [0.16]	0.02	[0.01, 0.02]	0.02	[0.01, 0.02]
# Refugee Families w/ Health Problems	0.09 (0.01) [0.03]	0.07 (0.01) [0.23]	0.06	[0.06, 0.07]	0.04	[0.04, 0.07]
# Refugee Families Food Insecure	0.08 (0.02) [0.02]	0.06 (0.02) [0.22]	0.05	[0.05, 0.06]	0.02	[0.02, 0.06]
# Refugee Families Fleeing Violence	0.09 (0.02) [0.04]	0.07 (0.02) [0.18]	0.06	[0.06, 0.07]	0.03	[0.03, 0.07]
% Refugee Months Not co-ethnic	0.00 (0.01) [0.03]	0.01 (0.01) [0.44]	0.02	[0.01, 0.02]	0.04	[0.02, 0.04]
% Refugee Months Muslim	0.01 (0.01) [0.03]	0.01 (0.01) [0.22]	0.01	[0.04, 0.04]	0.02	[0.01, 0.02]
% Refugee Families w/ Health Problems	0.05 (0.01) [0.03]	0.04 (0.01) [0.15]	0.04	[0.04, 0.04]	0.03	[0.03, 0.04]
% Refugee Families Food Insecure	0.01 (0.01) [0]	0.02 (0.01) [0.16]	0.02	[0.02, 0.02]	0.02	[0.02, 0.02]
% Refugee Families Fleeing Violence	0.05 (0.01) [0.03]	0.04 (0.01) [0.15]	0.04	[0.04, 0.04]	0.03	[0.03, 0.04]

Notes: The first column shows the “naive” effect of *violvict* on the outcome, estimated bivariate regression. The second column presents estimates of the fully controlled effect, reported in Table 2 in the main article. The third and fourth columns show the bias-adjusted effect and identified set under the standards for robustness ($R_{max}^2 = 1.3 \times R_{controlled}^2$ and $\delta = 1$) derived in Oster 2017s validation exercise. The fifth and sixth columns show the bias adjusted effect and identified set assuming $R_{max}^2 = 2 \times R_{controlled}^2$ and $\delta = 1$.

4 Supplemental analyses

4.1 Main effects, controlling for index of prosocial behavior

This section replicates Tables 2 and 3 from the main paper, controlling for an index of prosocial behavior (contributions to public goods in community in the past 30 days, community group membership and meeting attendance).

Table 4: Replication of Table 2 in main paper after controlling for prosocial behavior index

	# Refugee Months Hosted		Hosted Ethnic Outgroup		Hosted Religious Outgroup	
Violence index (0-6)	5.33*** (1.41)	5.04*** (1.52)	0.02*** (0.01)	0.02** (0.01)	0.01*** (0.00)	0.01*** (0.00)
Prosociality	9.74*** (2.97)	13.22*** (3.20)	0.05*** (0.02)	0.05** (0.02)	0.01 (0.01)	0.01 (0.01)
R ²	0.25	0.30	0.22	0.28	0.07	0.16
Num. obs.	1574	1300	1574	1300	1574	1300
Y Mean	61	61	.22	.22	.07	.07
Village FE	Y	Y	Y	Y	Y	Y
Ctrl Vars	N	Y	N	Y	N	Y

	Hosted family w/ health problems		Hosted family w/ food insecurity		Hosted family fleeing violence	
Violence index (0-6)	0.05*** (0.01)	0.04*** (0.01)	0.03*** (0.01)	0.03*** (0.01)	0.04*** (0.01)	0.04*** (0.01)
Prosociality	0.09*** (0.03)	0.09*** (0.03)	0.08*** (0.02)	0.08*** (0.02)	0.03 (0.03)	0.02 (0.03)
R ²	0.19	0.21	0.21	0.23	0.13	0.16
Num. obs.	1052	1035	1052	1035	1052	1035
Y Mean	.39	.39	.53	.53	.27	.27
Village FE	Y	Y	Y	Y	Y	Y
Ctrl Vars	N	Y	N	Y	N	Y

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 5: Replication of Table 3 in main paper after controlling for prosocial behavior index

	% Refugee Months (Not co-ethnic)		% Refugee Months Muslim		% Refugee Families Health Problems		% Refugee Families Food Insecure		% Refugee Families Fled direct violence	
Violence Index (0-6)	0.00 (0.01)	0.01* (0.01)	0.01** (0.00)	0.01** (0.01)	0.04*** (0.01)	0.04*** (0.01)	0.01 (0.01)	0.01 (0.01)	0.04*** (0.01)	0.04*** (0.01)
Prosociality	0.01 (0.02)	0.03 (0.02)	0.00 (0.02)	0.01 (0.02)	0.10*** (0.03)	0.09*** (0.03)	0.05** (0.02)	0.06** (0.02)	0.01 (0.04)	0.01 (0.04)
R ²	0.33	0.45	0.19	0.22	0.15	0.17	0.15	0.17	0.13	0.15
Ctrl vars	N	Y	N	Y	N	Y	N	Y	N	Y
Num. obs.	745	734	745	734	745	734	745	734	745	734

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

4.2 Social norms and hosting

Violence victimization is not associated with: whether the respondent sought out the refugee family herself (Column 1); hosting due to pressure from other community members (Column 3); and only weakly and negatively associated with hosting due to pressure by village authorities (Column 2).

Table 6: Individual agency and the decision to host refugees

	% of Families found Herself	% Families hosted due to pressure by Bigmen	% Families hosted due to pressure by neighbors
Violvict	0.00 (0.01)	-0.01 * (0.01)	0.00 (0.01)
R ²	0.18	0.11	0.18
Num. obs.	415	415	415
Ctrl Vars	Y	Y	Y

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Sample restricted to 32 communities covered in the endline sample (See Appendix Section 1).

4.3 Selective migration

Another source of potential bias is selective out-migration during and after the war. If out-migration correlates positively with violence and negatively with hosting refugees, our estimates would over-estimate the effect of past violence on the hosting of refugees. To address this concern, we replicate the main results after restricting the sample to the 80 per cent of respondents that were born in the town in which we surveyed them — a sample where concerns about selective migration do not apply. We find that the results are substantively unchanged, suggesting that selective migration is unlikely to be a confounding factor.

Table 7: Replication of Table 2 in main paper, sub-setting to those born in town

	# Refugee Months Hosted		Hosted Ethnic Outgroup		Hosted Religious Outgroup	
Violence index (0-6)	6.23*** (1.46)	6.56*** (1.56)	0.03*** (0.01)	0.02** (0.01)	0.01*** (0.00)	0.01* (0.01)
R ²	0.26	0.31	0.25	0.28	0.07	0.19
Num. obs.	1201	1005	1201	1005	1583	1005
Y Mean	61	61	.22	.22	.07	.07
Village FE	Y	Y	Y	Y	Y	Y
Ctrl Vars	N	Y	N	Y	N	Y

	Hosted family w/ health problems		Hosted family w/ food insecurity		Hosted family fleeing violence	
Violence index (0-6)	0.05*** (0.01)	0.05*** (0.01)	0.03*** (0.01)	0.03*** (0.01)	0.03*** (0.01)	0.03*** (0.01)
R ²	0.18	0.21	0.23	0.25	0.13	0.15
Num. obs.	831	815	831	815	831	815
Y Mean	.39	.39	.53	.53	.27	.27
Village FE	Y	Y	Y	Y	Y	Y
Ctrl Vars	N	Y	N	Y	N	Y

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 8: Replication of Table 3 in main paper, sub-setting to those born in town

	% Refugee Months (Not co-ethnic)		% Refugee Months Muslim		% Refugee Families Health Problems		% Refugee Families Food Insecure		% Refugee Families Fled direct violence	
Violence Index (0-6)	0.01 (0.01)	0.02* (0.01)	0.01* (0.01)	0.01 (0.01)	0.05*** (0.01)	0.04*** (0.01)	0.01 (0.01)	0.01 (0.02)	0.03** (0.01)	0.03* (0.02)
R ²	0.38	0.45	0.23	0.29	0.13	0.17	0.18	0.21	0.11	0.13
Ctrl vars	N	Y	N	Y	N	Y	N	Y	N	Y
Num. obs.	593	584	593	584	593	584	593	584	593	584

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

4.4 Generalizability

To assess generalizability, we draw on data collected in the same region in 2009 and 2010 by Blattman, Hartman and Blair (2014), which includes many of the same survey questions on wartime violence exposure. Their survey also asked whether the respondent contributed to sick families in their village in the past month, which fits our definition of altruism as action taken to benefit another at the expense of one's own material welfare. Finally, the survey includes the religion and ethnicity of the respondent. The 2009 and 2010 rounds of the data provide 40 randomly selected respondents per village. We aggregate individual level responses to ethnicity and religion to construct measures of village level ethnic diversity and consider whether the effect of violence exposure is attenuated in diverse settings. Appendix 3 displays the results, and shows that wartime violence is linked to altruism and that this effect is equally strong in diverse villages. This result suggests that the positive relationship between violence exposure and greater altruism toward ingroup and outgroup others generalizes beyond the Ivorian refugee crisis in Liberia.

Generalizability

	Contribute food or \$ to sick villagers in past 30 days			
violence_experienced (0-14)	0.021*** (0.002)	0.015*** (0.003)	0.020*** (0.003)	0.015*** (0.003)
Community ELF	0.511*** (0.048)	0.702*** (0.158)		
violence_experienced $\times$ c.ELF	-0.010 (0.009)	-0.010 (0.009)		
Community Religious ELF			1.271*** (0.088)	1.688*** (0.422)
violence_experienced $\times$ c.r.ELF			-0.005 (0.012)	-0.008 (0.013)
Village Fixed Effects	Y	Y	Y	Y
Controls	N	Y	N	Y
R ²	0.099	0.136	0.099	0.128
Num. obs.	4800	4466	4800	4466

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Covariates include: 5 age dummies, whether related to chief prior to war, parental education, whether family owned livestock before the war, prewar family occupation dummies, prewar family business ownership, gender, and ethnicity. Standard errors clustered by village.

4.5 Non-association of violence with past refugee experience

Table 9: Non-association of violence with past refugee experience

	Previously a refugee	Previously a refugee & stayed with co-ethnic family
warvict	0.01 (0.01)	−0.00 (0.01)
Y Mean	.73	.53
Individual controls	Y	Y
Village Fixed effects	Y	Y
R ²	0.34	0.24
Num. obs.	1289	1289

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Covariates include: 5 age dummies, whether related to chief prior to war, parental education, whether family owned livestock before the war, prewar family occupation dummies, prewar family business ownership, gender, and ethnicity. Standard errors clustered by village.

5 Figures as Tables

5.1 Results from Figures 1 & 2

Table 10: Results from Figures 1 & 2

	Refugee hosted		
woman_hh	0.04** (0.02)	0.00 (0.04)	0.04** (0.04)
farmer	0.05** (0.02)	0.03 (0.04)	0.04** (0.04)
hunger	0.10*** (0.02)	0.08** (0.04)	0.10*** (0.04)
coreligious	0.15*** (0.02)	0.20*** (0.04)	0.15*** (0.04)
coethnic	0.03 (0.02)	0.04 (0.04)	0.03 (0.04)
woman_hh x warvict		0.01 (0.01)	
farmer x warvict		0.00 (0.01)	
hunger x warvict		0.01 (0.01)	
coreligious x warvict		-0.02* (0.01)	
coethnic x warvict		-0.01 (0.01)	
woman_hh x prime			0.01 (0.01)
farmer x prime			-0.01 (0.01)
hunger x prime			0.07*** (0.01)
coreligious x prime			-0.07*** (0.01)
coethnic x prime			0.02 (0.01)
R ²	0.05	0.05	0.05
# of obs. (profiles)	3149	3149	6542
# of respondents	525	525	1091

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

6 Qualitative interview guide

1. Observation

Please briefly describe the context where you are having the interaction with the respondent.

2. Can you describe what happened when refugees from Côte d'Ivoire came to this community and needed to be hosted?

- *What was the role of the chief/leadership? What was the role of NGOs?*
- *What support did NGOs provide? How would you describe this level of support? Was it enough?*
- *Did the refugees come all at once, or did they come small small?*
- *Who were the refugees who came here? Did many different tribes come to your community or just some tribes?*
- *Did it ever happen that some tribes came here from CDI that did not get along?*
- *Did it ever happen that some people in your community did not want to host different different tribes? Why?*

3. Did you host refugees here during the Ivorian crisis? YES NO

4. If the respondent did not host refugees, WHY NOT? / If the respondent did host refugees, WHY?

- *Did you have a choice in hosting? Did you have a choice in who to host? Why or why not? Was there any pressure in these choices?*
- *What was the reward for hosting? What was the punishment for not hosting? (reward can even mean feeling good about yourself, or feeling like the other people in the community are praising you; or it can mean that the refugees who hosted worked your land and brought benefit to you)*
- *When your refugee family leave, why did they leave? Was it ever the case that you had to let them go because you could not manage to host them? Was it easy to let them go?*

5. If the respondent spoke about their own experience in exile as part of their decision to host, please ask them to explain more about why their own experience led them to host refugees / If the respondent did not mention their own experience in exile as a reason for hosting refugees, ask:

- *Did it ever happen that some people in your community did not want to host different different tribes? Why?*
- *Does it have anything to do with what happened here during the war? What about the kind of people they were (the way they talk, the way they eat, they way they practice their religion)?*

6. If you were a refugee during the war, or an IDP, can you talk about your living experience during exile.

- *Did you stay in the camps, with a host family, or both?*
- *Did your host family speak your dialect? Why made you decide to stay in a camp or a host family?*
- *Was life better in the camps or among host families?*
- *What was your biggest challenge living with a host family?*

7. Before the Ivorian election crisis started now, can you tell me about how you yourself and people in the community used to go back and forth between here and Côte d'Ivoire?

- *How often do people in this community go back and forth between here and there?*
- *Why do people do back and forth between here and there? (Buying, selling, family, marrying, visiting, medical care etc?)*

8. Since the election crisis, how has this changed?

- *How often do people in this community go back and forth between here and there?*
- *Why do people do back and forth between here and there? (Buying, selling, family, marrying, visiting, medical care etc*

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