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Refugee or Internally Displaced Person?

To Where Should One Flee?

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This study investigates the circumstances that lead some countries to produce a large number of refugees and relatively few internally displaced persons (IDPs) as opposed to a large number of IDPs and relatively few refugees. The authors develop the hypothesis that refugee flows are greater in the face of state (sponsored) genocide/politicide than they are in response to other state coercion, dissident campaigns of violence, or civil wars. They also argue that countries surrounded by poor, authoritarian regimes will produce fewer refugees (relative to IDPs) than those surrounded by wealthy, democratic neighbors. A sample selection model is employed to conduct statistical analyses using data on a global sample of countries for the period from 1976 to 1995. The results support many of the authors' hypotheses and suggest that the choice-centered approach produces useful answers to new questions that other scholars have yet to ask.

Keywords: refugees; forced migration; internally displaced persons

The literature on the decision to abandon one's home in the face of violence is dominated by scholarship that emphasizes a lack of choice: People flee because they have to. Yet we know this is not the case. For all those who flee, others stay behind, some choosing to take up weapons, others simply believing that they can "ride out the storm." When faced with extraordinary circumstances, people still make choices.

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And the choices people make reveal themselves in patterns at the aggregate level. Consider the Kosovo region of the former Yugoslavia in 1998 and 1999. In the spring and summer of 1998, more than 40,000 Kosovars abandoned their homes and hid in forests and the mountains, setting up their own makeshift camps or seeking shelter with relatives, whereas virtually no Kosovars crossed an international border in search of refuge from the fighting (U.S. Committee for Refugees, 1999, pp. 247-248). The following spring and summer, an estimated 857,000 fled Kosovo, whereas another 580,000 abandoned their homes but remained in the province (Reliefweb, 2000). Thus, in 1 year the Kosovo region produced a far greater number of internally displaced persons (IDPs) than refugees.[1] Yet in the following year, the same region produced far greater numbers of refugees than IDPs. What can account for the dramatic shift in the destinations of Kosovars between 1998 and 1999? More generally, what can account for the likelihood that a country will produce more refugees than IDPs or vice versa?

Although there are no studies that focus on this specific question, Schmeidl (2000) expresses what seems to be something of a conventional view when she asserts that "refugees and IDPs flee from similar root causes rather than responding to completely different occurrences" (p. 152). We offer an alternative view, suggesting that (a) different patterns of conflict behavior by states and dissidents have differential impacts on refugee and IDP flows and (b) the characteristics of neighboring countries also have an effect.

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1. A forced migrant is one who, owing to a reasonable fear of persecution—by which we mean deprivation of life, liberty, or physical being—abandons her or his possessions and/or domicile and relocates either within her or his own country or seeks asylum abroad. A refugee is a forced migrant who seeks asylum abroad, whereas an internally displaced person (IDP) is a forced migrant who relocates within the borders of her or his country of origin.

Two questions motivate the inquiry. First, what leads some countries to produce forced migrants (FMs)? Second, what characteristics of countries will affect the proportion of FMs who become refugees versus IDPs? To answer the first question, we build on a small group of studies that argue that forced migration flows are a function of peoples' responses to expectations of coercive and/or violent behavior (Davenport, Moore, & Poe, 2003; Moore & Shellman, 2004a; Neumayer, 2005). The second question leads us to extend the argument and develop new hypotheses about peoples' response to expectations of coercion and violence as well as hypotheses about the impact of neighboring environments on aggregate flows. Recent work by Azam and Hoeffler (2002) and Valentino, Huth, and Balch-Lindsay (2004) suggests that states sometimes have incentives to directly target civilians (in addition to insurgents). We build on this work to take issue with the claim that all coercion/violence has a uniform effect on refugees and IDPs. We argue, instead, that the targeting strategy of the state plays a major role in determining whether a country that produces FMs produces relatively few or relatively many refugees. We also develop hypotheses about the impact the characteristics of neighboring countries have on the relative number of refugees.

To test our hypotheses we estimate a selection model, the first equation of which distinguishes those country-years that produce forced migration flows from those that do not. That equation focuses on the "push" factors of the country of origin. Our second equation accounts for the proportion of FMs who seek refuge abroad, and it takes into account both "push" and "pull" factors. Our results are consistent with several of our hypotheses.

We present the remainder of the study in four sections. We lay out the argument and hypotheses in the following section. Then we discuss the sample, statistical model, and operational indicators. Next, we discuss our estimation and results. In the conclusion, we discuss implications and sketch ideas for future analyses.

The Argument

We develop a stylized rationalist account of the decision to become a refugee or an IDP that has two stages and contends that the decision hinges on one's expectations of (a) victimization and (b) socio-politico-economic opportunities. The first stage is the decision whether to abandon one's home and is a function of expectations of victimization. The second stage is the decision where to relocate and is a function of both expectations of victimization and opportunities in the different locations.

We are interested in identifying the characteristics of countries that make them more likely to produce a greater proportion of refugees relative to IDPs. As such, we are interested in explaining a macro-level observable. Yet because these observables are the outcome of decisions made by individual people, we ground our explanation in the choices of individuals.[2] More specific, we assume that individuals are purposive, that they value their liberty, their physical person, their property, and their lives. And we sketch a stylized rationalist model of decision making that emphasizes peoples' formation of expectations based on information about the threat that people face with respect to those values. We further assume that there is a single set of relevant public information in countries: People have access to roughly the same information when they form their expectations.[3]

To begin, consider a female head of household deciding whether to abandon her home (and property, if she owns it) to relocate elsewhere, taking only the possessions she can carry. We argue that individuals form expectations about the probability of becoming a victim of persecution, denoted P. As P rises in value toward 1, we suggest that there is some threshold at which most people will choose to abandon their homes. As long as P is below that threshold—and for the vast majority of human beings on the planet, we submit that it is—forced migration will remain rare: Although there are some 20 million FMs, they represent .3% of the global population (United Nations High Commissioner for Refugees, n.d.; United Nations Population Fund, 2001). However, when an individual's expectation of P is above her or his threshold, the individual will flee, even though it means abandoning her or his property.

Those people who flee face a second choice to either cross an international border or stay within their country. To make that choice, they must determine the probability of persecution associated with remaining inside their own country, P_O, or crossing a border, P_A. We use the subscript $_O$ to indicate the country of origin and the subscript $_A$ to indicate countries of potential asylum, and P continues to represent the probability of victimized persecution. When $P_O > P_A$, an individual is more likely to seek refuge abroad than seek refuge within his or her origin country.

Our assumption that people use information to assign values to P is important for making the micro-macro link: It suggests that as information

2. See Little (1991, chap. 2) for a useful discussion of the value of micro foundations in theory.

3. Anderson, Regan, and Ostergard (2002) provide evidence to support the claim that people have access to a single information set about human rights violations. Note that because preferences and resource allocations will vary across individuals, not everyone will respond the same way to that information set. However, large numbers of people—those whose preferences and resources are near the high point of the joint distributions—will respond to the information set in the same way. We wish to account for the migratory behavior of these individuals.

changes, peoples' expectations of P will shift in response and, thus, forced migration flows will change as well. To develop hypotheses, then, we must identify both the information people will monitor to develop their expectations of P and for those whose expectation of P is sufficiently high, the information they will monitor to assign expected values to P_O and P_A. Our hypotheses about the sources of information relevant to P are developed in greater detail elsewhere (Moore & Shellman, 2004a).[4] Our hypotheses about the information that influences expectations of P_O and P_A require development.

Information Relevant to P_O and P_A

There is some speculation in the literature on IDPs that push factors have the same impact on refugee and IDP flows (e.g., Schmeidl, 2000). Yet recent work suggests that not all states adopt the same targeting strategies when facing protest and insurgency. Scholars such as Mason and Krane (1989) argue that whether a state adopts an indiscriminate or focused targeting strategy will influence the mobilization prospects of dissident groups. But recent work by Azam and Hoeffler (2002) and Valentino et al. (2004) focuses attention elsewhere with respect to the impact of government targeting strategy. Both studies begin with the observation that civilian deaths in civil wars are often too high in frequency to be explained as "collateral damage." They develop similar arguments about why the state is the likely culprit.

Azam and Hoeffler (2002) develop a game theory model that shows that states can have an incentive to target civilian noncombatants to deny dissidents their base of support. Then they examine refugee flows in sub-Saharan African civil wars as a test of a portion of their model. Valentino et al. (2004) briefly review classic guerrilla theory and suggest that in those civil wars where the insurgents successfully mobilize popular support, the state has an incentive to "drain the sea" by directly targeting the civilians who provide the insurgents with succor.

We build on these claims to develop hypotheses about the coercive/violent behavior of states, dissidents, and foreign soldiers on the push factors that will influence the value of P_O. To begin, Mao Tse Tung (1938/1966) argues that "we must rely on the force of the popular masses, for it is only thus that we can have a guarantee of success" (p. 57). When a state responds to this reliance on the local population by targeting civilian noncombatants, we expect people to revise their expectations of P_O upward. We expect that the state's targeting strategy will have a significant impact on peoples' expectations about the likelihood of victimization and that we will be able to

4. We provide a brief overview in the online appendix that accompanies this study (see http://garnet.acns.fsu.edu/~whmoore/research/OnlineAppendix.CPS06.pdf).

observe such an effect in larger proportions of refugees relative to IDPs in cases where states adopt targeting strategies that fail to discriminate between combatants and noncombatants.

In much the same way, as dissidents increase the commission of violent acts, individuals should revise upward their beliefs about P_o. However, this effect may be conditional on government behavior. Mao (1938/1966) suggests that dissidents and guerrillas need the support of the masses to maintain their movements. As a result, we suspect that dissidents do not target the masses, but instead primarily target the state. Moreover, in civil wars, rebels often provide protection to individuals and their families in exchange for support. This leads us to hypothesize that as both dissident violence and state violence increase, migrants are more likely to remain within their own borders than cross an international border because dissidents will seek to provide pockets of safe haven within the country. As a result, civil war (the violent interaction of states and dissidents) should decrease the proportion of refugees to FMs.

As just noted, because dissidents and foreign states seek to replace the incumbent regime, they must court popular support. As such, the targeting strategies of dissidents and foreign soldiers will exhibit a strong tendency to target combatants. States, on the other hand, have an incentive to target civilian noncombatants (Azam & Hoeffler, 2002; Valentino et al., 2004). We submit that these strategic considerations make states considerably more likely to target noncombatants than dissidents. We thus expect that the level of state violence will condition the impact of dissident violence on refugee flows. That is, if state violence is low, higher levels of dissident violence will raise peoples' perception of P_o. However, as state violence rises and dissident violence is high, the state is more likely to target civilian noncombatants, and the dissidents are more likely to try to offer refuge. Thus the impact of higher levels of dissident violence on P_o should decline as state violence rises.

Having discussed sources of threat, we now turn our attention to other variables that influence forced migration. Leveraging the same arguments that we use to develop hypotheses with respect to FMs (Moore & Shellman, 2004a), we expect that institutions that provide for freedom and the rule of law, high wages, and the absence of diaspora populations abroad will reduce the number of people who will seek refuge abroad for any given information set that influences individuals' expectations about P_o. That is, we expect people to value these three things and, thus, be less likely to seek refuge abroad when their country has democratic institutions, high wages, and little diaspora population abroad.

What influences P_A? Recall that this term represents an individual's expectation that she or he will be a victim of persecution in the country where

she or he seeks asylum. To develop hypotheses from our framework, we need to identify the information that people would use to develop expectations about the value of P_A. To create comparability across cases, we focus on the characteristics of neighboring countries. Although some refugees seek asylum in countries that do not border their country of origin, the vast majority seek refuge in neighboring countries. Thus we develop our hypotheses with respect to information about the characteristics of the surrounding neighborhood.[5]

Given our assumption that people value lives, liberty, physical person, and property, we anticipate that they will avoid leaping from the frying pan into the fire: P_A drops as the violence level rises in neighboring countries. Stated as hypotheses, we expect higher levels of coercive/violent behavior by neighboring states, neighboring dissidents, and foreign soldiers in neighboring countries to be associated with lower proportions of refugees relative to IDPs.

In addition, we submit that the political institutions, wage opportunities, and cultural opportunities available in neighboring countries will have an impact on the proportion of refugees relative to IDPs. More specifically, we hypothesize that the more democratic the institutions, the higher the wages, and the greater the size of a diaspora culture, the greater will be the proportion of refugees to IDPs from a given country of origin.

Finally, we submit that transaction costs will influence the number of refugees and IDPs a country is likely to produce and argue that these costs are a positive function of distance and difficult terrain on the border. To produce specific hypotheses, we assume that relocating within one's own country requires less travel than relocating abroad.[6] This leads to the hypothesis that, ceteris paribus, people are more likely to relocate within their own country than abroad. With respect to difficult terrain, we hypothesize that countries surrounded by mountains will produce fewer refugees and more IDPs. We briefly sketch our research design below. A full discussion of our operationalization and research design can be found in the online appendix that accompanies this study.

5. In a different study (Moore & Shellman, 2004b), we examine a refugee's choice of asylum country and in that study we drop the assumption that people seek asylum in neighboring countries.

6. We recognize that this assumption is violated for people who live closer to a city, town, or village across the border than the nearest city, town, or village in their own country.

Research Design

We study all countries for which we could assemble data for the period from 1976 to 1995 and use the country-year as our unit of observation. Our hypotheses require us to measure 19 concepts, and we use measures that have been widely used by others studying violence. The United Nations High Commissioner for Refugees (n.d.) database is the source for our indicator of the number of refugees who fled a given country in a given year, and Schmeidl and Jenkins (1999) provide the data for the number of IDPs in a given country-year. We use Harff's (2003) list of genocides and politicides to create a binary measure of the presence of a genocide/politicide event. The Political Terror Scale (PTS; Gibney & Dalton, 1996) is a 5-point scale of the extent to which a state violates the physical integrity of persons' rights, and we use it as another indicator of state violence. To measure dissident violence, we create a count of violent demonstrations and guerrilla attacks from Banks (n.d.). We also interact the PTS score with dissident violence. Both our measure of civil war and our indicator for war on the territory of the country are taken from the Correlates of War project (Sarkees, 2000).

In addition to measuring these concepts in each country, we need to develop measures of the level of violence in surrounding countries. We use the same sources of data described above to develop measures of the levels of violence in neighboring countries. More specific, we calculate the mean (for ordinal and integer indicators) or proportion (for binary indicators) of each variable over the country's neighbors.

We also have several concepts that do not involve violence. The Polity IV data (Marshall & Jaggers, n.d.) provide an indicator of both the level of institutional democracy and the absence of a functioning government (a variable that we call transition). The latter is a binary indicator that is scored 1 whenever the Polity IV project does not assign values on its 21-point scale of democracy. We use the 21-point scale as our measure of institutional democracy. To measure wages, we use GNP per capita. The GNP data are from both the World Bank (2002) and Banks (n.d.), and the population data come from Fearon and Laitin (2003). To measure the size of the diaspora community, we use the stock of FMs produced by the country in the preceding year, employing the United Nations High Commissioner for Refugees (n.d.) data and the Schmeidl and Jenkins (1999) data. We also create a measure of the previous year's value of the proportion of FM stock from a given country that are refugees. As with the violence indicators, to get at pull factors, we need to calculate neighborhood scores using the same approach.

Finally, we measure transaction costs by creating a binary variable that codes whether a given country has mountainous borders (Shellman, 2001).

Table 1
Descriptive Statistics

	Mean	Standard Deviation	Minimum	Maximum
Origin genocide	0.07	0.25	0	1
Origin dissident violence	0.79	2.14	0	27
Origin civil war	0.14	0.34	0	1
Origin IWOT	0.02	0.13	0	1
Origin political terror (PTS)	2.85	1.04	1	5
Origin Dissident Violence × PTS	2.70	7.45	0	92
Neighborhood genocide	0.06	.13	0	0.67
Neighborhood dissident violence	0.93	1.91	0	27
Neighborhood civil war	0.14	0.20	0	1
Neighborhood IWOT	0.02	0.08	0	1
Neighborhood political terror (PTS)	3.00	0.69	0	5
Origin democracy	−1.32	6.98	−10	10
Origin transition	.046	0.21	0	1
Neighborhood democracy	−1.26	5.10	−10	10
Neighborhood transition	.046	0.12	0	1
Origin GNP/capita	3,375	6,114	43	41,293
Neighborhood GNP/capita	3,481	4,092	83	22,897
Mountains	0.36	0.33	0	1
Lag refs/FMs stock	0.21	0.39	0	1
Lag forced stock	135,964	488,348	0	4,945,130

Note: FM = forced migrant; IWOT = international war on territory; PTS = Political Terror Scale.

We calculate the neighborhood score as the proportion of neighboring countries with mountainous borders. We report the descriptive statistics for all our variables in Table 1. The data we use in this study are available as a replication data set deposited with the Inter-University Consortium for Political and Social Research's Publication Related Archive.

To estimate parameters, we use a Heckman sample selection model where the selection equation regresses a binary indicator of whether the country produced any refugees or IDPs in a given year on the independent variables that represent our hypotheses for that process. The equation of interest regresses the proportion of FMs who are refugees on the variables that represent our hypotheses regarding that process. We used Stata's Heckman procedure to estimate the parameters.

When analyzing pooled data, one must concern oneself with the possibility that the error terms are heteroscedastic and contemporaneously correlated

Table 2
Proportion of Refugees to Forced Migrants, 1971 to 1996

Value	Frequency	Percentage
0	126	15.4
0.01 to 0.09	57	6.9
0.1 to 0.19	15	1.8
0.2 to 0.29	12	1.4
0.3 to 0.39	8	1.0
0.4 to 0.49	11	1.3
0.5 to 0.59	10	1.2
0.6 to 0.69	4	0.5
0.7 to 0.79	2	0.2
0.8 to 0.89	1	0.1
0.9 to 0.99	2	0.2
1	571	69.7
0 to 1	819	100

Note: This shows values for cases in which there is a nonzero forced migrant flow (i.e., only the selected sample). For ease of presentation, we truncate the values at the first significant digit. Percentage column does not total 100% due to rounding.

across panels. Heteroscedasticity and autocorrelation do not bias estimates, but they inflate the standard errors and, thus, hinder hypothesis testing. We correct for the problems associated with autocorrelation and heteroscedasticity by including lagged stock variables in our models and calculating robust standard errors using the Huber/White/sandwich estimator.[7] Having presented the research design, we turn to the results.

Results

Before turning to parameter estimates, we can test one of our transaction cost hypotheses by examining the frequency distribution of the proportion of refugees to FMs for those country-years with a nonzero FM flow. Table 2 records that distribution and to our surprise, is strongly inconsistent with our hypothesis. We argue that because of the costs of relocating, people are more likely to travel a short distance than a long distance, which leads one to expect the proportion of refugees to FMs to be skewed toward 0; but instead, the distribution is strongly skewed toward 1. It is conceivable that this distri-

7. We use the cluster option in Stata 8.0 to calculate the standard errors. The method relaxes the independence assumption and requires that only the observations be independent across countries.

bution is an artifact of the relative difficulty of identifying IDPs relative to refugees (Schmeidl, 1998). Yet although it is certainly true that IDP populations are more likely to go unidentified than refugee populations, two issues mitigate against our invoking that defense. First, doing so would mean defending our hypothesis in the face of contrary evidence. Second, Schmeidl (1998) notes that by the mid-1980s, the United Nations High Commissioner for Refugees and other organizations considerably improved their efforts to identify IDP populations and record their presence. Yet the distribution of the proportion of refugees to all FMs does not change appreciably when one examines the years 1980 to 1995 or 1990 to 1995. This strongly suggests that the distribution is not an artifact of poor data collection prior to the mid-1980s. For those reasons, we accept the inference that our transaction cost hypothesis is rejected with respect to the decision to seek refuge abroad as opposed to at home.

What might account for this pattern? One possibility is that although the transaction costs of seeking refuge abroad do outweigh the transaction costs of relocating as an IDP, most people who cross the threshold P^* do not find the threat reduced sufficiently as an IDP. In other words, the reduction of threat from seeking refuge abroad might—ceteris paribus—be greater on average than the reduction of threat from seeking refuge within one's own country. Such an account is consistent with Table 2.[8]

Turning to our other hypotheses, we report our findings in Table 3. The model in Table 3 includes the PTS variable. Because the PTS variable is available for fewer countries than a number of our other variables, our sample size is not as large as it might be. However, when we estimate a model that excludes the PTS variable, by and large, the inferences we draw across the two models are the same.[9] The origin variables appear toward the top of

8. A second possibility is that most transaction costs of seeking refuge abroad for people who become refugees are—on average—lower than those for seeking refuge within country. That would be the case if the violence that leads to exodus is skewed toward bordering areas of countries. In other words, there might be strong tendencies such that people who become refugees might have lived near a border and people who become IDPs might have lived far from borders. Such a pattern would be consistent with a transaction costs story and also consistent with the results in Table 2. Unfortunately, our data contain no information about geographic location. As we discuss briefly in the conclusion, adding geographic information is a direction for future inquiry.

9. One can replicate the model that excludes the Political Terror Scale (PTS) variable using the replication data set and command files. Although the magnitudes of the coefficients differ in places, the signs remain the same across the two models. With respect to significance, origin dissident violence and neighborhood civil war are no longer statistically significant, and origin democracy, neighborhood GNP per capita, and mountains become statistically significant. This sample covers the years from 1971 to 1995.

Table 3, whereas the neighborhood variables appear toward the bottom of the table. Recall that we include only the neighborhood variables in the outcome equation. In addition to the coefficient estimates, we report the marginal effects of the variables that were included in both equations. We do so because the coefficients for these variables do not fully represent the effects of the variables. Sigelman and Zeng (2000) point out that "when the errors of the selection and the regression equations are correlated ($\rho \neq 0$), it is incorrect to interpret β_k as the marginal effect of x_k on y, unless x_k does not enter the selection equation" (p. 177). The impacts of the independent variables that appear in both equations are a compound of their impacts on the selection and outcome equations. Therefore we compute the marginal effects of those variables using the formula given in Sigelman and Zeng (p. 177). One can interpret the coefficients of the other variables as if one had estimated a single equation regression model on the proportion variable.

Because we report the findings of a more extensive inquiry into the covariates of forced migration elsewhere (Moore & Shellman, 2004a), we do not discuss the coefficients and substantive effects from our selection model at length. The parameter estimates are largely consistent with our previous work; and where they are not, we note the exception. This allows us to focus our attention on the outcome equation.

Before considering those coefficients and substantive effects, however, note that the selection parameter, ρ is statistically significant, which indicates that the residuals are correlated across equations. When $\rho \neq 0$, standard OLS regression techniques applied to the outcome equation yield biased results. The estimated coefficient for ρ confirms that we should be using a sample selection model on these data.

We now turn our attention to the main equation of interest and the marginal effects of the variables on the proportion of refugees to FMs. Our major hypothesis concerns the effect of genocide/politicide in the country of origin. The parameter estimate for this variable is positively signed and statistically significant. That is consistent with our hypothesis that the proportion of refugees will rise in response to a genocide/politicide event. Furthermore, the marginal effect .05 indicates that a country that experiences state (sponsored) genocide will increase its proportion of refugees to FMs by that amount, holding all other independent variables constant. That might initially appear to be an inconsequential substantive effect, but recall that our dependent variable ranges from a minimum of 0 to a maximum of 1. Given that range of values, an increase of .05 is nontrivial.

Civil war and the interaction of PTS and dissident violence produce negatively signed, statistically significant parameter estimates, whereas the PTS and dissident violence component variables are positive and significant. Dis-

Table 3
Proportion of Refugees to Forced Migrants With Sample Selection,
1976 to 1995

Variable	Forced Migrant	Refugee Flow/ Forced Flow	Migrant Marginal Effect[a]
Origin genocide	0.30†† (0.17)	0.12††† (0.05)	0.05
Origin dissident violence	0.17† (0.11)	0.08†† (0.04)	0.04
Origin civil war	0.83††† (0.16)	–0.22††† (0.05)	–0.42
Origin IWOT	0.28 (0.24)	–0.08 (0.10)	–0.14
Origin political terror (PTS)	0.27††† (0.06)	0.08††† (0.03)	0.012
Origin Dissident Violence × PTS	–0.04† (0.02)	–0.02††† (0.01)	–0.014
Origin democracy	1.9e-03 (0.01)	–0.004 (0.01)	–0.004
Origin transition	–0.03 (0.25)	–0.06 (0.08)	–0.05
Origin GNP/capita	–8.67e-05††† (1.64e-05)	1.35e-05†† (8.38e-06)	7.61e-06
Lag forced stock	1.99e-07††† (7.21e-08)		
Neighborhood genocide		–0.46††† (0.18)	
Neighborhood dissident violence		0.01 (0.01)	
Neighborhood civil war		–0.19†† (0.12)	
Neighborhood IWOT		0.32†† (0.14)	
Neighborhood political terror (PTS)		0.03 (0.03)	
Neighborhood democracy		–0.01††† (0.00)	
Neighborhood transition		0.11 (0.16)	
Neighborhood GNP/capita		4.96e-06 (6.36e-06)	
Mountains		0.04 (0.05)	
Lag refs/FMs stock		0.09†† (0.04)	
Constant	–1.69††† (0.21)	0.06 (0.15)	
Rho—Selection effect	0.70††† (0.09)		
Sigma	0.44††† (0.03)		
N (uncensored)	1972 (417)		

Note: Robust standard errors appear in parentheses. FM = forced migrant; IWOT = international war on territory; PTS = Political Terror Scale.

a. The effects of variables contained only in the proportion equation can be interpreted as the marginal effect of a 1-unit change in that variable on the proportion. However, if the variable enters both equations, the coefficient for the proportion equation is affected by its presence in the selection equation. The formula used to calculate the marginal effect for each variable is

$$\frac{\partial E(y \mid z^* > 0, x)}{\partial x_k} = \beta_k - \alpha_k \sigma_u \delta(w - \alpha).$$

Sigelman and Zeng (2000, p. 179) note that this calculation produces an estimate for each observation and that one must take the mean of all estimates to produce the "average" impact of the independent variable.

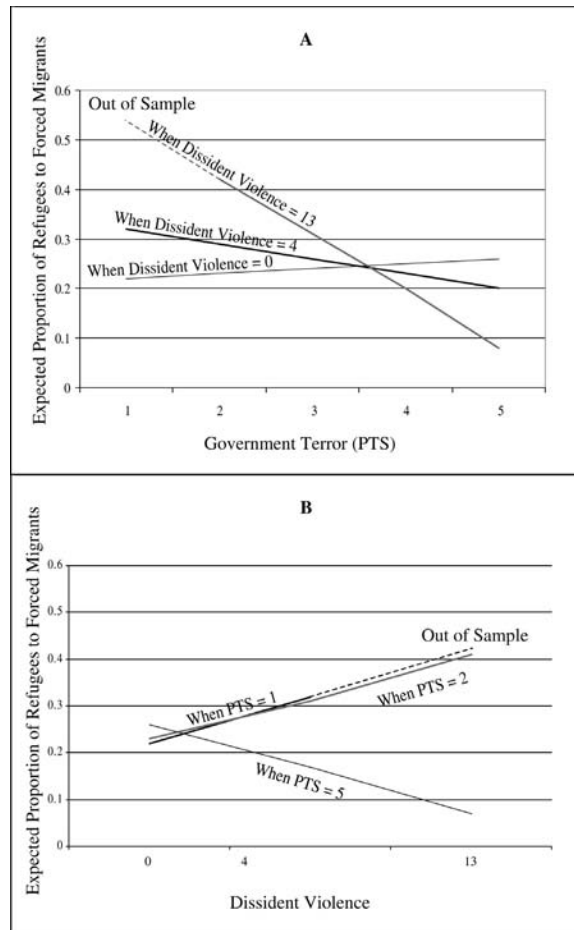†$p < .10$. ††$p < .05$. †††$p < .01$.

cussion of civil war is straight forward, but interpretation of the other effects requires additional attention. A civil war reduces the proportion of refugees to all FMs by .42. This is a very large effect, and it is consistent with our expectations: Although genocide/politicide events increase the proportion of refugees to IDPs by .05, civil war decreases the proportion of refugees to IDPs by .42. That said, it is important to note that this does not mean that countries with civil wars will produce more IDPs than refugees. This result should be interpreted within the context of the skewed distribution of the dependent variable as reported in Table 1. If we take a random draw from the population of forced migration cases, we will usually get a case with a high proportion of refugees to IDPs. When we add the information that this case has a genocide/politicide event, we are usually going to draw a case with an even greater proportion of refugees to IDPs. Yet if we take a random draw but require that the case have a civil war, we are likely to draw a case with a considerably lower proportion of refugees to IDPs.

We also probe whether a different measure of the interaction between government and dissident behavior would produce the same effect as civil war. The component coefficients (i.e., dissident violence and PTS) represent the impact of the component when the other component has a value of 0 and proper interpretation of the interaction effect requires additional manipulation (Friedrich, 1982). With respect to the latter, we provide two graphs to display the effects across different values of the other variable. To interpret the components, note that PTS ranges from 1 to 5 and, thus, the coefficient estimate for dissident violence is out of sample: It does not make sense to speak of the impact of dissident violence when PTS equals 0. What about the impact of PTS when dissident violence is 0? Table 3 indicates that when there are no acts of dissident violence, a unit increase in PTS increases the proportion by .012. To analyze the impact of PTS at other values of dissident violence, we turn to Figure 1.

Panel A in Figure 1 depicts the degree to which the slope of the relationship between government terror (PTS) and the proportion of FMs who are refugees changes as a function of dissident violence. We consider three values of interest for dissident violence: a high value of 13 events, a middle value of 4 events, and a low value of 1 event. We also choose these values because they are representative of the sample values. Points on the solid lines are in-sample effects, whereas the dotted lines represent out-of-sample effects (i.e., these observations are not in our data). With that as background, Panel A in Figure 1 shows that the impact of PTS on the proportion of refugees is strongly influenced by the value of dissident violence. When dissident violence is low, it does not matter what value PTS takes: The variables do not appreciably affect the proportion of refugees, holding all dummy vari-

Figure 1
Interactive Effect of Dissident Violence and Government Terror

Note: The expected proportions are calculated by setting all dummy variables to 0 and all other variables to their means. We use the average marginal effects calculated for variables, entering both equations that appear in Table 3, and the coefficient estimates for the variables only, entering the regression equation. Dotted lines represent effects outside the estimation sample. PTS = Political Terror Scale.

ables at 0 and all other independent variables at their means.[10] However, when dissident violence has high values (e.g., 13 events), then the expected proportion of refugees to all FMs shifts from 0.08 when PTS is at its maximum value of 5 to 0.42 when PTS is at a lower value of 2.[11] This is a very strong effect. Combining this information with the coefficient on the component term, we find that when dissident violence is absent, PTS slightly increases the proportion of refugees. However, as dissident violence increases, that positive effect quickly dissipates and the slope between PTS and the proportion of refugees to FMs becomes negative.

Turning our attention to Panel B, we find that the impact of dissident violence on the proportion of refugees is also strongly conditioned by the value of PTS.[12] When states respect human rights (PTS = 1), dissident violence has a positive slope; but as we increase the value of PTS, the slope of the relationship between dissident violence and the proportion of refugees to FMs becomes negative. For clarity of presentation, we present only three lines in the graph, and one can see that the impact of dissident violence on proportion of refugees is negative when a state uses human rights violations against all members of its population (i.e., PTS = 5). In sum, our hypothesis about high levels of government terror and high levels of dissident violence garners support across both the civil war variable and the interaction between dissident violence and government terror.

That said, we did not expect to find a strong, positive effect for dissident violence, yet there is such an effect conditioned on the absence of government terror. What might explain this result? One possibility is that high dissident violence coupled with low government terror is the result of dissidents targeting civilians instead of the state. If dissidents target civilians, we would expect people to be more likely to seek refuge abroad than at home. Unfortunately, our measure of dissident violence is an event count that measures intensity rather than categorizing the target of the violence. As such, we cannot draw a conclusion about dissident targeting of civilians. Nonetheless, our finding hints that a future direction for investigation is the measurement of dissident targeting of noncombatants.[13]

10. Note that we calculate the expected values using the marginal effects for the origin variables and the coefficient estimates on the neighborhood variables and setting all dummy variables to 0 and all other variables to their means.

11. The observation where PTS equals 1 and dissident violence equals 13 does not exist in our data.

12. We recognize that the information contained in Panel B is redundant. We present both panels because it is easier to see the effects that way.

13. Stathis Kalyvas (1999) makes arguments about both governments and dissidents targeting the population.

Returning to the other measures of violence, international war on territory produces a negative sign but is statistically indistinguishable from 0. To properly interpret this null finding, we must also consider the coefficients in the selection equation. If those estimates indicate that a variable has a positive, statistically significant impact on the probability that a country produces FMs, then a null finding in the outcome equation implies that the variable increases both refugee and IDP flows equally. However, if the parameter estimate in both equations is not statistically significant, then that suggests that the variable influences neither the refugee flow nor the IDP flow. The latter inference is suggested for this variable.[14] This is something of a surprise, as international war on territory produces a positively signed statistically significant parameter estimate in some models in our previous research (Moore & Shellman, 2004a), and Schmeidl (1997) finds that participation in international war is positively associated with forced migration.

Having discussed the effects of local coercion/violence, we turn our attention to the levels of coercion/violence in the neighborhood. Not as many variables produce statistically significant effects, but those that do have a considerable impact. For example, the results show that the greater the number of neighboring countries experiencing genocide/politicide, the lower the proportion of refugees. In a similar manner, the greater the frequency of civil war in neighboring countries, the lower the proportion of refugees. Both findings are consistent with our hypotheses, but the estimate for neighboring countries experiencing international war on their territory runs contrary to our expectations: The greater the frequency of foreign soldiers fighting in neighboring lands, the higher the proportion of refugees.

To consider substantive effects, a shift from zero neighboring countries with genocide to all countries with genocide/politicide decreases the proportion of refugees by a whopping .3, although such a proportion of genocides/politicides does not exist in our sample. To consider a more realistic example, consider a country with five neighbors. A shift from zero genocides/politicides in the neighborhood to one reduces the proportion of refugees to FMs by .09, holding all else constant. If we increase it from zero to two neighbors experiencing genocide, the proportion of refugees to FMs will drop by an average of .18. A country with all of its neighbors at civil war will produce a .19 lower proportion of refugees, on average, than one with none of its neighbors at civil war. Although neighbors at civil war are nowhere near as rare as neighbors experiencing genocide, we can create a more realistic example by considering a country with four neighbors. As each of its neighbors engages in civil war, the proportion of refugees will drop by .048,

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14. We also tried lagging this variable and it too was positive and not significant.

holding all other variables constant. Finally, a country surrounded by neighbors with foreign soldiers fighting on their territory will, ceteris paribus, produce a .32 larger proportion of refugees. If we consider a country with four neighbors, as each is invaded by foreign soldiers, the proportion of refugees will, on average, grow by .08.[15]

The finding that international war on territory produces a positive coefficient is unexpected but might be explained by Weiner's (1996) notion of "bad neighborhoods." He argues that not only do bad government, war, and civil war tend to cluster geographically and, thus, produce refugee flows, but also refugee flows tend to reinforce bad government, war, and civil war. Salehyan and Gleditsch (in press) study a portion of the system about which Weiner speculates and report countries that host refugees from neighboring countries have greater incidence of civil war. Lischer (2000, n.d.) reports that although refugees are involved in less violence than is commonly believed, some refugee populations are drawn into civil wars in their host countries whereas others mobilize militarily to launch raids against their country of origin. Moreover, Hegre, Ellingsen, Gates, and Gleditsch (2002) find that civil wars in neighboring countries increase the probability of a country experiencing civil war. Our study cannot speak directly to Weiner's idea or the Salehyan and Gleditsch; Lischer; or Hegre et al. findings, but it does hint at the possibility of endogenous relationships that we have not captured.[16] In the conclusion, we discuss the implications of this for future work.

Having discussed the impact of the coercive/violent behavior of actors on the proportion of refugees, we wish to briefly consider the results for our other variables. Origin democracy has a negative effect on the proportion measure but is not statistically significant. The dummy variable that mea-

15. A reviewer suggested that we lag this variable to examine whether refugees were indeed attracted to neighboring international wars or caused them. When we lag the neighborhood war on territory variable, it is still positive and significant. That said, we do not think that lagged values of the neighborhood indicators get at the issue. Instead, one wants to regress neighborhood international war on territory on a lagged value of refugee proportion. We estimate a plain vanilla regression, and the estimate for lagged proportion value is .006 and almost statistically significant. This finding is close to but not quite consistent with Weiner's (1996) conjecture that refugees are not so much attracted to wars as they are contributing factors to wars.

16. When we lag neighborhood civil war, the variable is insignificant. As above, we do not think this is the way to get at the relationships and, thus, we again estimate a regression model. This time the model contains neighborhood civil war as the dependent variable and the lagged value of the proportion as the independent variable. This time the coefficient estimate is statistically significant with a coefficient of .05, which is a nontrivial effect. The inference is that refugees are not attracted to ongoing neighboring civil wars but may be contributing to them as Salehyan and Gleditsch (in press) contend.

sures transition regimes does not produce a statistically significant parameter estimate in either model.

Although the origin GNP per capita coefficient is negative in the outcome equation, the "average" marginal effects of origin GNP per capita are positive.[17] However, the coefficient is insignificant. Next, we consider the substantive significance of the neighborhood variables.

We begin with the lagged stock of the proportion of refugees to FMs, which is positively signed and significant. To consider substantive effects, moving from .10 to .50 in the lagged stock of the proportion increases the proportion of refugee flows to FM flows by .036. Thus consistent with our argument, as the refugee diaspora grows, the proportion of refugees produced by a country producing FMs will grow with it.

Neighboring democracy, however, unexpectedly decreases the proportion of refugees to FMs, whereas neighborhood GNP per capita has the hypothesized positive impact on the proportion of refugees to FMs. To consider the effect that is contrary to our expectations first, a country with four borders and two democratic neighbors will produce a refugee proportion .0025 lower than one with four borders and one democratic neighbor. Although it appears that larger neighborhood values of GNP per capita, on the other hand, attract refugees, the coefficient is insignificant.[18] This finding sheds light on the "bogus refugees" argument in that our evidence suggests that not all refugees are seeking wealthy markets across their borders.

Finally, our measure of transaction costs—the proportion of borders with mountainous terrain—is positively signed but insignificant. Recent work by Fearon and Laitin (1999, 2003) may account for this finding. Building on the same insurgent literature we reference above, they observe that insurgents need rough terrain to succeed and include a measure of the proportion of all territory that is mountainous in their studies of civil war. Perhaps our measure of mountainous borders is better understood as a proxy for guerrilla violence rather than as a measure of transaction costs.[19] To the extent that this is the case, it suggests that a multiple equation model (where rough terrain is an

17. The marginal effect for each observation ranges from −.000012 to .00000461, but the mean is in fact positive. Remember that in computing the marginal effects, we compute a marginal effect for each observation and take the average.

18. In a model, we estimate with a larger sample not including the PTS variable; neighborhood GNP per capita is positive and significant.

19. In a discussion at a scholarly meeting, Steve Saideman (personal communication, April 2004) suggested an alternative interpretation based on a year he spent in the Pentagon. Saideman observed that a conventional wisdom about mountain ranges is that they provide excellent cover for smuggling routes and, thus, contra our hypothesis, facilitate rather than deter clandestine travel.

independent variable in an equation with guerrilla violence as a dependent variable) would provide a better specification than our single equation.[20]

What do these findings suggest about why several hundred thousand Kosovars abandoned their homes and relocated inside Kosovo, whereas virtually none sought refuge abroad in 1998, or about why in 1999, nearly 1 million Kosovars sought refuge abroad, whereas several hundred thousand sought safety within Kosovo? The question is interesting in part because it leads one to think about the applicability of general findings to a specific case. In the Kosovar case, if our results are applicable, we should find a genocide campaign in 1999 but not 1998, as our genocide variable had a positive impact on the production of refugees relative to IDPs. Harff (2003) records a "politicide with communal victims" in Serbia (i.e., Kosovo) beginning in December 1998 and ending in July 1999. Although the 1st month of the event occurred in 1998, we submit that this is consistent with our statistical findings.

This brief sketch of Kosovo in 1998 and 1999 suggests that our cross-national model is at least partially consistent with one case study. What we find particularly interesting about the exercise is what it suggests about the usefulness of conducting statistical time-series case studies. Some of the variables in our cross-national model will drop out because of lack of variance (e.g., rough terrain). Others may not vary enough with time to play much of an explanatory role (e.g., democracy, size of the economy), especially if the unit of temporal observation is the week or month. That said, we are also likely to be able to produce more valid measures of our concepts than we have done in the cross-national study (e.g., dummy variables can be replaced with event counts). Thus a useful direction for future analyses is to conduct time-series case studies to probe the extent to which the cross-national results can be replicated in more specific analyses.

Conclusion

The dominant approach to the study of forced migration suggests that people always leave in the presence of high-level violence. We chart a different course and ground our hypothesis development in the decision to abandon one's home as a choice. Doing so opens new avenues for inquiry, such as

20. Another potential possibility brought to our attention recently is that democracies are better able to police their borders such that increased neighboring democracy might decrease refugees. In a similar manner, mountains might make the borders harder to police and present opportunities to cross borders without being apprehended.

the question of whether one should seek refuge abroad or in one's country of origin.

Our study produces two major findings. First, we find support for our primary hypothesis that refugee production relative to IDPs is a positive function of state targeting of civilians, as measured by genocide in our study. Second, we produce the expected result that both civil war and high levels of dissident violence coupled with high levels of government terror have large negative effects on the proportion of refugees to FMs.

We fail to find support for some of our hypotheses, chief among them our arguments about transaction costs. First, contrary to the notion that distance traveled is an important determinant of forced migration decision making, refugee flows outweigh IDP flows in the vast majority of cases. Second, mountainous borders do not operate as a deterrent but instead, as a stimulant to refugee flows. Future work will do well to take these results and integrate them into new theories of forced migration.

We find support for other hypotheses, however. Several neighborhood variables have the hypothesized effects: Large refugee diasporas, high wages, and the absence of genocide in neighboring countries each spur refugee flows relative to IDP flows. On the other hand, our findings for neighborhood democracy and international war on territory run contrary to expectations, and the latter suggests that it may be useful to develop more complex theoretical and statistical models that specify interdependencies that produce bad neighborhoods.

Our study, then, charts a new path and illuminates both fruitful directions for theory development and empirical anomalies that require explanation. We conclude with a discussion of a handful of directions for further analysis that we believe warrant investigation.

Perhaps the biggest weakness of global analyses such as this is the lack of more specific data about (a) population locations (and, thus, distance from borders), their size, and composition; and (b) information on countries' immigration and emigration policies and practices. Our study effectively assumes that the probability of persecution or victimization is uniform across the population of each country, and this surely is not so. Furthermore, we do not have any information about the location of violence, and it presumably makes a difference because (a) people presumably walk away from, not toward violence; and (b) the size and location of the population that is proximate to violence vis-à-vis borders will presumably affect the size of refugee flows. Moreover, although we have some information about the geography of borders, we know nothing about the terrain within countries, and this may affect decisions as well. Finally, origin and neighboring countries' immigration practices likely influence forced migration and whether countries pro-

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duce more refugees or IDPs.[21] The collection of information on populations and migration practices is prohibitively costly for a global sample but is quite feasible for smaller time-series case studies.

Finally, both the cross-national model reported here and time-series case study models should be studied to determine their out-of-sample forecasting properties. If our designs can produce short-run forecasts, that would be of great interest to contingency planners. One finding of interest from this study is that civil wars tend to produce more IDPs, whereas genocides tend to produce more refugees.

Given the shortcomings noted above, one might ask why we would begin with a global analysis. The reason is simple. We are interested in determining what—if any—general patterns can be established. Doing so is useful for several reasons. First, the data we use are relatively easy to collect; if they prove useful for out-of-sample forecasting (something we have not yet investigated), then we may have a rather crude yet simple tool that will have some utility for contingency planning. Second, it gives us a baseline of knowledge against which we can assess the findings from time-series case studies. Without the global analyses, one would be hard pressed to assess the extent to which ones' findings were the result of selecting unusual cases. Third, these sorts of patterns can help illuminate new questions and ultimately, answers as well as shortcomings not revealed by less systematic empirical investigations. Yet we need more fine-grained analyses with both variables that cannot (yet) be measured in a global analysis, as well as more reliable and valid measures of variables we can measure in a global analysis.

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