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Risk Factors for Forced Migrant Flight

JACQUELINE H. RUBIN
WILL H. MOORE

Department of Political Science
The Florida State University
Tallahassee, USA

An important type of medical study seeks to establish the risk factors for contracting various diseases. A similar, but very small, vein of research exists in peace and conflict studies, and we seek to contribute to it. Our study evaluates whether variables shown to explain variance in numbers of forced migrants can serve as risk factors that might aid contingency planning for such humanitarian crises. We study a cross-national sample of cases over the period from 1985 through 1994. Our findings indicate that annual, country-level indicators of civil war, a forced migrant episode, and human rights violations are candidate risk factors for forced migration in the following year. Interestingly, when using country-years as the unit of observation genocide is not a useful risk factor for forced migration.

Keywords early warning, forced migration, refugees.

Introduction

In early 2003, fighting erupted in Sudan's western region of Darfur. By late 2004, an estimated 1.6 million people were displaced within the region, which is approximately the size of France. An additional 200,000 had fled their homeland for neighboring Chad. Across the border, refugees found themselves surrounded by desert, in a remote area where resources, particularly water, were scarce. They constructed makeshift shelters to protect themselves from cross-border raids and the isolation of the rainy season, when aid deliveries are nearly impossible.

In response, the United Nations High Commissioner for Refugees (UNHCR) "in early 2004 mounted a major logistics operation to move the vast majority of the refugees to camps at a safer distance from the volatile border" (UNHCR, 2005). Primarily due to security concerns, launching operations in Darfur itself proved more difficult. Following a request from the UN country team in Sudan, UNHCR become operational within Darfur in June 2004. Nonetheless, on October 15, 2004, the World Health Organization (WHO) estimated that up to 70,000 of the displaced people in Darfur had died "as a direct result of the conditions in which they [were] living" since March 1, 2004 (WHO).

The United States Mission to the UN defines humanitarian emergencies as conditions under which "large numbers of people are dependent on humanitarian assistance . . . from sources external to their own society . . . and/or . . . are in need of physical protection in

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Address correspondence to Jacqueline H. Rubin, Department of Political Science, The Florida State University, Tallahassee, FL 32306. E-mail: jrubin@fsu.edu

order to have access to subsistence or external assistance" (1996, p. 1). Väyrynen (1996, pp. 16–19) notes that humanitarian emergencies have four aspects: warfare (primarily within states), disease, hunger, and refugee flight. Of these, the last has the clearest international consequences. Refugee flight is, quite literally, the spread of domestic unrest across international borders. Forced migrants who cross international borders often do so without sufficient water, food, or shelter. Like Blanche DuBois in Tennessee William's epic *A Streetcar Named Desire*, these people depend entirely on "the kindness of strangers."

What could have lessened the plight of those 200,000 fleeing to Chad with only their lives? Contingency planning is one important option available to the international community. The UNHCR (1996, Section 1) defines contingency planning as:

> a forward planning process, in a state of uncertainty, in which scenarios and objectives are agreed, managerial and technical actions defined, and potential response systems put in place in order to prevent, or better respond to, an emergency or critical situation.

Contingency planning requires risk assessment: "In order to anticipate, assist, or prevent refugee flight, we need to identify and monitor those causes and triggering events of flight" (Apodaca, 1998, p. 81).[1] The UNHCR's primary document for contingency planning suggests that "there is no hard and fast rule" to determine when one should draw up such plans:

> Often it is simply a question of intuition mixed with experience that prompts one to recognize the need. A number of attempts have been made to "scientifically" determine when an influx or some other event requiring contingency planning is likely, but all have their limitations. Neither extreme, the intuitive nor the scientific, are adequate. The best approach to early warning lies somewhere between (UNHCR, 1996, Section 1).

This study asks whether the variables scholars have used to explain the annual-level variance in forced migrants across large numbers of countries have any potential to serve as risk factors to contribute to the scientifically based portion of risk assessment. Put simply, can statistical models identify risk factors?

Although we base our model on existing work, this effort is distinct in its goal. Previous scholars have concentrated their efforts on hypothesis testing, asking when forced migrants will leave their homes, or where they will go once they've left. These questions have led to empirical specifications designed to highlight contemporaneous activity. Contemporaneous factors are theoretically interesting, but are unlikely to help policymakers and humanitarian groups anticipate forced migration. To fill the lacuna, we ask an entirely different question: What (if any) are the warning signs of an impending forced migration event? This is a common question in medical studies: What risk factors change the probability of contracting a disease (Davies and Gurr, 1998)? Rather than test hypotheses or infer causality, risk factor models aim to identify observables that influence the probability that a given country experiences a forced migrant event.[2]

A brief empirical exercise highlights the distinction between hypothesis testing and risk factor analysis. In this paper, we examine the influence of a series of events on the

[1] We adopt the conventional definition of a forced migrant as one who, due to a fear of persecution, has abandoned his or her home in favor of an uncertain future elsewhere.

[2] Rather than focus on the probability of observing a forced migration event one could focus on the intensity of such an event. Doing so would certainly be useful. However, like those in the medical field who focus on the probability of contracting a medical condition rather than the severity of that condition, we believe that we are considerably more likely to find useful risk factors for the *probability* of observing a forced migration event than for the *intensity* of such an event.

probability of forced migration. A classic hypothesis-testing model would estimate the impact of those events at the time forced migration is observed; on the other hand, a risk factor model seeks leverage on the impact of the events *before* forced migrants emerge. The correlation between forced migration at time t and forced migration one year later, at t+1, is 0.52; although the two are related, a forced migration event in one year neither guarantees nor rules out another forced migration event in the following year. It follows that the factors that identifiably predate forced migration events may well be different from those that contemporaneously predict it. The current effort draws upon, but is distinct from, existing hypothesis testing work.

To place this effort into the important operational context, in the 1990s several projects, such as ReliefWeb (see Rusu, 1998) were launched that sought to create operational early warning systems of the type we are exploring here. A number of academic projects supported these efforts (e.g., Adelman, 1998; Davies and Gurr, 1998; Schmeidl and Adelman, 1998). Today one such effort, the FAST Early Warning Unit at Swisspeace (www.swisspeace.org/fast/), leads the field and has developed a number of clients who rely on their service. Thus, our effort is not merely an academic exercise: contingency planning supported by theoretically motivated and empirically based risk assessment is a reality, and we expect it to grow in importance. This effort seeks to contribute to that development.

Our study proceeds in four parts. In the next section, we review the recent literature on forced migration. In the second section we discuss our research design. We present the results of our empirical analysis in the next section, and discuss some of its implications. Finally, we conclude with a discussion of the limitations of this effort and some fruitful areas of future research.

A Model of Forced Migration

We are not the first to consider whether statistical research can help identify risk factors that could contribute to contingency planning for humanitarian crises.[3] Unfortunately, of the existing systematic work, only Schmeidl (1997), Schmeidl and Jenkins (1998) and Apodaca (1998) deal specifically with refugee flight; other efforts tend to treat humanitarian emergencies as a general category (Jenkins and Bond, 2001; Davies and Gurr, 1998; Harff and Gurr, 1998) or deal specifically with other crises (Tellis et al., 1997; Esty et. al., 1998).

The UNHCR (1996, Annex B) identifies a list of 30 risk factors to forced migrant flight, a number of which have subcategories.[4] One option is to collect data on indicators of the items on that list and then search for correlations, much like Esty et al. (1998) did in their

[3]See Gurr and Harff (1998) and Davies and Gurr (1998). Adelman (1998) and Schmeidl and Jenkins (1998) provide a concise and comprehensive discussion of the concepts underlying this type of effort, and the potential problems it presents.

[4]The list includes 14 factors prompting departure, including ethnic/racial tensions, social tensions, religious tension, human rights abuses, political instability including opposition movements, external factors (e.g., influence of foreign groups and governments), relations with neighboring countries, demographic factors, ecological devastation and other natural events, economic instability (including labor disputes), corruption and drug trafficking, military intervention and interferences, historical probability, and a favorable situation in neighboring countries. It includes seven intervening factors, including alternatives to international flight, international relief in place of origin, international protection force in place of origin, obstacles to flight, unfavorable asylum policies in nearby countries, closed borders, and uncertain living conditions in asylum country. Finally, it includes nine triggering events, including new types of people affected, problems spreading to new geographic regions, significant increases in the intensity of a situation, changes in the viability of flight (including open borders and new neighboring governments), the departure of key political figures or changes in political party, increased peer group pressure, natural disasters, mass demonstrations or riots, and seasonal factors.

effort to identify correlates of state failure. One advantage that a statistical model has over a seat-of-the-pants analysis like this is that the former produces specific estimates about the changes in probabilities associated with changes in risk factors. Additionally, in this paper we are specifically interested in the ability of theoretically grounded academic efforts to contribute to contingency planning efforts. With these considerations in mind, we adopt a different approach and turn to published studies of the correlates of forced migrant flows. In the conclusion, we revisit the UNHCR's list of risk factors, and compare it to our findings here.

Schmeidl (1997), Davenport et al. (2003), Moore and Shellman (2004) and Neumayer (2004) are the published studies from which to choose, and we selected Moore and Shellman. Their model is very similar to the Davenport model, and we prefer it largely for operational reasons with respect to the dependent variable (discussed below). Both Schmeidl and Neumayer restrict their samples (for various reasons), and we prefer the global coverage of Moore and Shellman. That said, the decision here to restrict our attention to the variables in the Moore and Shellman study should not prohibit others from examining different variables in other analyses.[5]

We briefly describe the arguments that Moore & Shellman invoke to motivate their specification, but first we must better rationalize the general exercise. The large-n statistical analyses of forced migration use annual, country-level data, and these data are rather coarse in terms of measurement relative to the forced migrants themselves and the field workers who serve them (neither of whom make decisions in annual chunks). Nevertheless, because annual data are churned out by governments, IGOs and NGOs, finding systematic patterns in such aggregated data means that those same, widely available, data might have utility as risk factors. However, they will only be useful as risk factors here if they have an impact *a year in advance*. Published studies examine the contemporaneous effects of the variables: the impact of genocide, civil war, etc. in year t on forced migrant flow in year t. To be useful as risk factors those same variables must have an impact in year $t-1$. As Apodaca explained, we need to know whether we can find indicators available today that can provide us with cause to plan for tomorrow. To that end our study examines the ability of the variables used in Moore and Shellman to serve as risk factors.

Davenport et al. (2003), Moore and Shellman (2004), Neumayer (2004) and Shellman and Stewart (this volume) develop their arguments about the covariates of forced migration by starting at the micro-level. They ask: Why would an individual leave his home and belongings in favor of an uncertain future elsewhere? Constructing an answer begins with the assumption that every individual is presented with a lottery where she is going to be the victim of persecution with some probability, $p \varepsilon [0, 1]$. As p rises from 0 to 1, there is for most people some threshold value above which they will prefer leaving to staying.

The general challenge in building a forced migration model, then, is to identify the factors that will influence the individual's perceptions about p. Details can be found in Moore and Shellman (2004), but the thrust of the argument is that people monitor their environments to develop expectations about whether the probability of becoming a victim of

[5]For example, Schmeidl (1997) finds that ethnic rebellion and foreign intervention into civil wars are significant predictors of refugee exodus. In addition, she argues that economic development may serve as an accelerator in the presence of political conflict, so that forced migrant flight is more likely in conflicting areas with low levels of development than in areas of greater development and equivalent conflict. Neumayer's (2004) study includes a series of economic variables absent from Moore and Shellman's model, of which growth and discrimination against ethnic minorities exerted a significant impact on asylum-seeking in Western Europe. He also found geographic proximity of the destination state to be an important determinant of asylum-seeking. Any of these variables might be considered as potential risk factors for forced migration.

persecution is sufficiently high that relocation is warranted. More specifically, they contend that there are three primary sources of threat: the state, dissidents, and foreign soldiers. We focus on the first two groups.[6]

Government violence is expressed through violations of human rights and acts of genocide and politicide. We expect human rights violations to increase the probability of a forced migration event in the following year, but offer the counter-intuitive expectation that a genocide event will *lower* the probability of observing forced migration in the following year. That expectation is driven by [a] research that shows that forced migration is a risk factor for a genocide event (Harff & Gurr, 1998; Harff, 2003) and [b] the fact that we use an annual unit of temporal aggregation. To elaborate, Harff's work shows that people tend to flee in response to anticipation of genocidal killing, not reaction to it: though they are contemporaneously correlated (when measured in annual units), forced migration is a risk factor for genocide, but not vice-versa. Further, because the threat of genocide is death, individuals who feel threatened from an ongoing genocide should be *less* likely to leave their homes than other individuals: Victims of genocide or politicide cannot translate that threat into forced migrant status: they are dead.[7] The annual temporal unit of aggregation is important because genocide events tend to last less than one year. Were we using daily data, we would anticipate that genocidal killing at time t would lead people in nearby locations to anticipate similar killings in their villages and towns at times $t+1$, $t+2$, etc., and flee in response. Using weekly data one would similarly expect the anticipation described by Harff to be revealed in a positive effect of genocidal activity at time t on forced migration activity at time $t+1$. However, because most genocides tend to last less than one year, when we move to annual aggregation genocidal activity at time t should be negatively associated with forced migration at $t+1$ as those who fled in response to the anticipation of killing will have done so in year t: people do not wait a year to flee.

Of course, governments are not the only source of individual threat. Dissidents engage in guerilla attacks or other armed activities that threaten citizens. In some cases either the government or the dissidents are the major source of violence, but in other cases both sides are able to mobilize sufficient people under arms to engage in prolonged fighting, which we call civil war. Thus either the state alone, the dissidents alone, or the interaction of the states and the dissidents might provide a source of threat. Davenport et al. (2003), Moore and Shellman (2004), Neumayer (2004) and Shellman and Stewart (this volume) find variables measuring these concepts have a statistically significant impact on forced migration.[8]

In addition to the set of publicly available information that contributes to each individual's expectation about the probability of persecution, these studies suggest that other factors will also have an impact on forced migrant flows. For example, Moore and Shellman (2004, p. 728) argue that "people live in cultural communities that are critically important to them," in part because they provide people with information about migration possibilities, but also due to the intrinsic value of one's culture. They use the stock of forced migrants from a given country as a proxy to measure those concepts, and their findings (and those of others) indicate that it affects the flow of forced migrants.

[6]Moore and Shellman (2004) construct a dichotomous measure of foreign troops on territory to capture this possible influence on p. The variable never gains significance in their analyses, and we expect the same here. Nonetheless, we did run models including the measure. As expected, it was statistically insignificant and did not change any of our other results.

[7]We are grateful to Barbara Harff for drawing our attention to the fact that, as she put it, "post genocide people do not move because they are dead."

[8]Schmeidl (1997) also has variables that measure some of these concepts, but not all of them. The ones she measures are also supported in her data.

In addition, Moore and Shellman submit that, "ceteris paribus, people prefer to be able to share their political views with others without fear of retribution and that they prefer transparent government to corrupt government" (p. 729; see also Schmeidl, 1997; Davenport et al., 2003). Institutional design may influence a government's ability or willingness to repress its citizenry. For example, institutions that can channel citizen discontent may mitigate a desire to leave, or an independent military may be unwilling to follow executive orders to restrict human rights. Countries with democratic institutions, then, should experience less forced migration than autocratic polities.[9]

Finally, the voluntary migration literature focuses largely on income as a key causal force of migration. Moore and Shellman (2004) and Neumayer (2004) contend that expected income likely plays a role in forced migrants' decisions to stay or go. In the absence of a direct measure of wages, these studies have included GNP per capita in their statistical models, and the results have supported the hypothesis that expected wages influence forced migration.[10]

The theory and results found in this literature provide a candidate list of variables that we can explore as possible risk factors. As noted above, to be useful as risk factors they will need to be able to contribute to the probability of observing a forced migration event in the following year. That suggests the following model of probability of forced migration:

$$\begin{aligned}\Pr(\text{Forced Migration})_{i,t+1} &= \alpha + \beta_1(\text{human rights abuse})_{it} + \beta_2(\text{genocide})_{it} \\ &+ \beta_3(\text{dissident violence})_{it} + \beta_4(\text{civil war})_{it} + \beta_5(\text{forced migrant stock})_{it} \\ &+ \beta_6(\text{institutional freedom})_{it} + \beta_7(\text{expected wages})_{it} + \varepsilon_i\end{aligned}$$

Having identified a model to estimate, we turn our attention to research design, data, and estimation issues.

Research Design, Data, and Estimation

Ideally, risk factors would be measured at the smallest level of temporal-spatial aggregation possible. Thus Harff and Gurr (1998, p. 569) propose "daily monitoring of high-risk situations." The trade-off, of course, is that such fine-grained information is difficult to come by, especially across a large spatial-temporal domain. For this effort, we probe the usefulness of annual, country-level data. This means that we measure our potential risk factors the year prior to the forced migrant observation. Significant results given this specification would suggest that our independent variables have some usefulness as risk factors for forced migrant events.

Since we are primarily using the Moore and Shellman (2004) data, our spatial domain is all countries in the world included in their study, which is most countries, excluding the micro-states. Our temporal domain is 1981–1994, a range defined by the availability of data across all variables in our models.

[9]The literature also proposes an inverted U-shape for the relationship between institutional freedom (opportunities) and political dissent (Jenkins, 1983; Tilly, 1978). We estimated models that included such a specification; the nonlinear variable, democracy^2, was not significant in either model. Including it also failed to change the significance or substantive implications of the model's other variables.

[10]Schmeidl (1997) is an exception. She finds that economic underdevelopment and population pressures have little impact on subsequent refugee migration.

Dependent Variable: Forced Migration

Past studies of forced migration have concentrated on the relative magnitude of that migration. Thus Gibney et al. (1996) measure the number of refugees in the international system, and Schmeidl (1997) focuses on the stock, or number, of displaced refugees originating from a given country. Davenport et al. (2003) expand the stock measure to include internally displaced persons (IDPs), and then calculate *net* forced migrants as the difference between IDPs plus refugees abroad and refugees hosted. Moore and Shellman (2004) take the first differences of the stock of forced migrants to create a "flow" of the number of displaced persons originating from a given country in a given year.[11] Like Davenport et al. (2003) they define forced migrants as the sum of IDPs and refugees abroad, though they do not calculate the *net* figure. Finally, Neumayer (2004) counts the number of asylum applications to Western European countries. These measures were developed to test contemporaneous models of forced migration. We have a different goal: our interest lies in predicting the future probability of a forced migrant event. Having discussed this point above, we begin this study with a probability model, and thus require a dichotomous dependent variable.

Given our interest in a binary measure of forced migration we use Moore and Shellman's (2004) "flow" data to create a variable that equals 1 when forced migrants emerge from a given country in a given year, and 0 when no forced migration is observed. Forced migration is a relatively rare event: 1,464 out of 1,781 country-years (around 82%) in our data experienced no forced migration, while 317 country-years (around 18%) produced refugees and/or internally displaced persons.[12]

Independent Variables

Our model requires measures of seven concepts. Moore and Shellman (2004) operationalize each of the concepts, and we adopt their data with one exception. They use the Political Terror Scale (PTS; Gibney and Dalton, 1996) to measure human rights violations. We also use PTS, but in addition we explore the usefulness of a new measure of human rights abuses developed by Cingranelli and Richards (online).

Nearly all empirical human rights research is focused on one category of human rights: physical integrity rights, or "the entitlements individuals have in international law to be free from arbitrary physical harm and coercion by their government" (Cingranelli and Richards, 1999, p. 407). Violations of these rights include extrajudicial killing, disappearances, torture, and political imprisonment. The most commonly used measure of physical integrity abuse is the political terror scale (PTS; Gibney and Dalton, 1996).

The PTS is coded from annual Amnesty International and U.S. State Department reports of cross-national human rights practices, using an ordered scale ranging from 1 to 5 where higher values represent higher levels of physical integrity abuse by a government. The scale assumes unidimensionality of human rights abuse; that is, it assumes that physical integrity abuse is scalable across its types, and therefore that it can be accurately measured by level alone.

[11] They bound the lower value at zero by recoding all negative scores.

[12] Although our current research interests have lead us to a dichotomous dependent variable, one might be interested in the distribution of the count data that generated our current dependent variable. Given forced migration (e.g., forced migrants 0), we observe a range from 10 to 3.5 million refugees and IDPs in a given unit. The average country-year produced 175,649 refugees and IDPs (standard deviation = 416,480). 16 country-years experienced forced migration in excess of 1 million refugees and IDPs.

Although PTS and the Cingranelli-Richards Human Rights Database (CIRI) physical integrity scores are both drawn from the same information, PTS makes an assumption that CIRI problematizes: that physical integrity violations form a unidimensional index. This difference results in conceptually different measures of physical integrity abuse. More specifically, the CIRI index coders began with disaggregated information about government respect for specific physical integrity rights. They then use Mokken scaling analysis to produce "an easily replicable, unidimensional scale of overall government respect for physical integrity rights" (Cingranelli and Richards, 1999, p. 408). Finally, the results are summed across all four physical integrity rights. This produces an ordinal scale of government respect for physical integrity, ranging from 0 (no government respect) to 8 (full government respect).[13] This approach allows them to problematize (and ultimately demonstrate) the unidimensionality of physical integrity abuse.

Because PTS assumes unidimensionality, a score on the PTS provides information solely about the level of physical integrity abuse in a country. Beginning with categorical data grants the CIRI scale leverage on more nuanced information about that abuse. Specifically, a single score on the CIRI scale provides information about the level, pattern, and sequence of government respect for particular physical integrity rights. Thus a CIRI score contains information about "the different combinations of human rights that governments choose to violate" (Cingranelli and Richards, 1999, p. 411). For example, a score of three represents no government respect for the rights against imprisonment and torture, partial respect for the right not to be extra judicially killed, and full respect for the right not to be disappeared. A score of four predicts that the right against torture is partially respected *before* the right against political imprisonment (Cingranelli and Richards, 1999, p. 413).

For measurement purposes, the main advantage of the cumulative scaling technique used to produce the CIRI physical integrity scale is that "knowing the resulting *pattern* of government respect, given a single scale score for any country-year, one can predict with great accuracy which particular rights a government respects and which ones it violates" (Cingranelli and Richards, 1999, p. 415–416). For our purposes, knowing the patterns associated with scale scores may shed light on how patterns of physical integrity abuse affect the decision to leave one's home and become a forced migrant.

In our sample the two measures correlate at 0.69. Because of these differences, we estimate two models, one that uses PTS to measure physical integrity abuse, and a second that uses CIRI. If the measures perform differently across the models, we expect the difference will be due to CIRI's systematic treatment of types of abuse. In particular, this attention to method might result in a less noisy and more accurate measure of human rights abuse, yielding in turn less noisy and more accurate parameter estimates.

The next concept we need to measure is genocide or politicide. We employ data collected by Barbara Harff for the State Failure Project (2003; also see Harff & Gurr, 1988). Harff provided an ordered scale of the annual number of deaths for each instance of genocide or politicide, available for all countries and years in our sample.[14]

To measure dissident violence we use Banks' (online) cross-national time-series archive data set to generate an event count measure of the number of times dissident groups used

[13]To aid in interpreting results, we recode the physical integrity index so that higher values indicate higher levels of human rights abuse.

[14]The data are available at the State Failure project website: www.cidcm.umd.edu/inscr/stfail/. We adopt Moore & Shellman's revised scaling of this variable: 0 = 0 deaths; 1 = 1 to 999; 2 = 1,000 to 1,999; 3 = 2,000 to 3,999; 4 = 4,000 to 7,999; 5 = 8,000 to 15,999; 6 = 16,000 to 31,999; 7 = 32,000 to 63,999; 8 = 64,000 to 127,999; 9 = 128,000 to 255,999; 10 ≥ 256,000.

violence in a given country-year. More specifically, the variable we use is the sum of two variables in Banks' data: the number of guerilla attacks and the number of riots.

To measure civil war in a country we use the Correlates of War intrastate and extra systemic war data (Sarkees, 2000). Extra-systemic wars include wars of national independence fought against a colonial power. This is a binary measure coded one when a civil war is present and zero when it is not. A conflict must record at least 1,000 battle deaths to be considered a war.

Next, we need a measure of the presence of a diapsora culture abroad and network that can provide information to potential forced migrants. This variable should be designed to measure the dispersion of a society: How many members of a community have already become forced migrants? Can the depletion of a native population be an indicator of subsequent displacement? To that end, the forced migrant stock variable is an aggregate of all past forced migrants from the relevant country. Stock is conceptually distinct from flow, which is the increase in total forced migrants from year t to year $t+1$, or the number of forced migrants who left their homes in year t. It is, in essence, the first difference of the stock. In this case, because our interest lies in identifying risk factors that predate population movements, we use a dichotomized lag of the origin country's forced migrant stock equal to one if a country has a non-zero number of forced migrants through the preceding year, and zero if it does not.

We use the Polity project's measure of institutional democracy (Jaggers and Gurr, 1995) to measure institutions that produce freedom. Taking the difference between the democracy and autocracy scores creates a single variable ranging from -10 to 10, with higher values indicating increasing levels of democracy.[15] Because regimes in transition or flux often cannot be coded, the Polity data contain a number of missing values on this measure. Rather than drop these regimes from the analysis, we assign missing values a score of zero and code a dummy variable that equals one when the Polity measures are missing due to transition, and zero otherwise (see Moore & Shellman, 2004, p. 732).

Finally, expected income may play a role in the decision to stay or go. Moore and Shellman's GNP indicator is taken from the World Bank, with missing values augmented with Banks data (online). They use Fearon and Laitin's (2003) population data to create a GNP per capita variable, which we employ here.

[15]Polity's democracy measure is an additive 10-point scale derived from codings of the competitiveness of political participation and executive recruitment, the openness of executive recruitment, and constraints on the chief executive. For our purpose, the democracy scale captures the extent to which citizens can channel discontent through political institutions. On the other hand, the autocracy scale is derived from the lack of regulated political competition and political participation, the lack of competitiveness and openness in executive recruitment, and the lack of constraints on the chief executive. This second measure captures the extent to which citizens are isolated from their government. As measured, both democracy and autocracy may directly affect the ability or willingness of a regime to repress its citizens. More importantly for this discussion, they are clearly different measures. Empirics support this point. The Polity IV democracy and autocracy measures correlate at -0.86; although similar, they are not opposite sides of the same coin.

When the autocracy score is subtracted from the democracy score, the resulting -10 to 10 variable captures the government's ability to imbibe discontent through nonviolent means, adjusted for its autonomy and lack of accountability: its ability to repress. This is what we want to capture when we hypothesize that institutional design may influence a government's ability or willingness to repress its citizenry, and as a result we use the -10 to 10 measure rather than the 0 to 10 democracy option.

Statistics and Issues

The data cover a global sample of 202 countries over the 14-year period from 1981 through 1994. Because the data contain observations across both time and space, we must address some potential estimation problems.

Studies widely agree that forced migration levels in one year affect forced migration levels in subsequent years. We suspect that this is so for the probability of a forced migration event as well. Systematic temporal dependence in the dependent variable over time biases estimates and causes them to be inefficient. To capture temporal dependence, we employ the method suggested by Beck et al. (1998). The key idea is that "annual [binary time-series cross-section] BTSCS data are equivalent to grouped duration data with an observation interval of one year" (1998, p. 1265). In this understanding, the dependent variable equals 1 if there was a failure (i.e., forced migrant event) during that year, and 0 in successful years. This allows the researcher to conduct BTSCS analysis by including temporal dummy variables or a natural cubic spline in their model.[16] Significant coefficients on the dummy or spline variables simultaneously diagnose and accounts for temporal dependence. We employ logit analysis with three spline knots.

We must also consider the fact that BTSCS data allow for multiple failures in the same unit; this differs from most event history analyses, which model time until the first or only failure (Beck, Katz, and Tucker, 1998:1271). The possibility of multiple failures problematizes the logit assumption that the probability of failure in any year is the same as that in any other year. Instead, it seems likely that the probability of a forced migration event in one year is in part dependent on citizens' past experience with the phenomenon. We model this dependence on past forced migrant events with a duration variable. The measure counts the time, in years, since a country last produced forced migrants.

A second hazard when using time-series cross-sectional data to estimate parameters is unit-level heterogeneity. While it is not obvious that the baseline probability of a forced migrant event is, ceteris paribus, different across countries, it is certainly possible. To account for the possibility of systematic variation in baseline probabilities across countries, we use a conditional fixed effects specification to estimate our models' parameters.[17]

We thus use a logit specification with conditional fixed effects, a duration variable counting the years since a country last experienced forced migration, and three temporal splines[18]

[16]Natural cubic splines fit cubic polynomials to a predetermined number of subintervals of a variable (Beck et al., 1998, p. 1270).

[17]There are at least two ways one can employ fixed effects to account for unit-level heterogeneity. First, we can specify our model to vary across units by including a dummy variable for each unit in the analysis: $Y_{it} = \alpha_{it} + \beta`X_{it} + \varepsilon_{it}$. This is the least-squares dummy variable (LSDV) approach. Second, we can remove country-specific effects by reducing each observation by its country-specific mean: $Y_{it} - Y_{i(\text{mean})} = \beta`(X_{it} - X_{i(\text{mean})}) + (\varepsilon_{it} - \varepsilon_{i(\text{mean})})$. This is the conditional fixed-effects specification. We estimated parameters using both specifications, and the results were not meaningfully different. However, the LSDV specification failed to report the Wald chi-square statistic, while the conditional fixed-effects option produced all statistics of interest. Therefore, the models reported in this paper use the conditional fixed-effects specification. We used Stata 8 to estimate the models (see the `clogit` command).

[18]Many of the most important international events seldom occur. For example, revolutions, economic shocks, and wars are rarely observed, but are of great interest to international scholars. One consequence of the rarity of these events is that our data often contain many more 0 s (nonevents) than 1 s (events). King and Zeng's (2001a; 2001b) demonstration of the dangers of rare events data call on a dataset of national dyads with 303,814 observations. Of those, 1,042 dyads (0.3%) were at war. The rarity of war in the data is accurate; the problem arises with the consideration that "the substantive information in the data lies much more with the 1's than the 0's" (King and Zeng, 2001a, p. 695). With the data overwhelmed by the absence of war, meaningful covariates of war's presence are lost. As a result, logit analyses underestimate the probability of war, while the probability of peace is overestimated.

to estimate our model. The following section presents the results of our model estimation, and discusses the substantive implications of those results.

Findings and Implications

Of the 1,781 country-years in our data, forced migrants were observed in 317, or nearly 18%. They emerged from 90 countries, or about 45% of our spatial sample, and were observed in each of the ten years covered by the data.

We report the results for our two conditional logit models in Table 1. The models are nearly identical, differentiated only by their varying expressions of physical integrity abuse (which produces slightly different sample sizes). Model 1 employs the political terror scale (PTS), while Model 2 uses the CIRI index of physical integrity abuse.

Because of the difficulty inherent in interpreting logit coefficients, we report both the coefficient estimates and the odds ratios for each model. The odds ratio for an independent variable represents the *ceteris paribus* factor change in the odds of observing forced migration, given a unit change in that variable.[19] Odds ratios greater than 1 indicate an increase in the odds of forced migration and odds ratios less than 1 represent a decrease in the odds.

We begin with a discussion of how the expectation of persecution influences the probability of future forced migration. Although positive, as expected, physical integrity abuse is insignificant in Model 1 (PTS); however, it is positive and significant in Model 2, which uses the CIRI scale. When physical integrity abuses, as measured with CIRI, increase in the preceding year, the odds of forced migration are increased by a factor of 1.3.

We can also examine changes in the expected probability of forced migrant outflow given unit changes in the independent variables of interest.[20] We depict the effects of perceived threat in Figure 1. The CIRI scale is coded over a nine-point range, so we use a line graph to portray the substantive effects (panel 2). Because CIRI's coding captures patterns and sequences of physical integrity abuse, interpreting the effects of changes in this variable requires understanding the relationship between values of the index and patterns of abuse. To that end, Table 2 replicates Table 3 of Cingranelli and Richards (1999, p. 414), and reports the pattern of government respect for physical integrity inherent in the CIRI coding scheme.[21]

In the CIRI scale freedom from torture is the first integrity violated, followed by unlawful imprisonment, then extrajudicial killings, and finally, disappearances. Each 1-unit increase in CIRI's 0 through 8 scheme represents a decrease in government respect for one

For two reasons, we are confident reporting logit results rather than employing the rare-events variant. First, there is no obvious threshold above which events are considered rare. In King and Zeng's data, war occurred 0.3% of the time. In our data, forced migration was observed in 317 out of 1,781 country-years (17.8%). With 17.8% of the data capturing our positive outcome, it is unclear whether forced migration can or should be considered a rare event.

Second, King and Zeng note that where small samples and rare events do not bias logit estimates, the rare-events method gives the same answer as logit: "When the results make a difference, our methods work better than logit; when they do not, these methods give the same answer as logit" (King and Zeng, 2001a, p. 702). We estimated our model with both logit and rare-events logit techniques, and found virtually identical results. Consequently, we are confident that the estimates reported in this paper are robust to rare-events biases.

[19]The odds ratio is the exponentiated coefficient multiplied by the degree of change in the independent variable. Thus, for a change of δ in x, the odds are expected to change by a factor of $e^{(\beta^*\delta)}$.

[20]In all cases, variables other than the specified variable of interest are held at their modes (for dichotomous and ordinal variables) or means (for continuous variables).

[21]Because we reversed the coding of this variable so that higher values represented more serious physical integrity abuse, the contents of the table are similarly reversed from Cingranelli and Richard's original table.

TABLE 1 Logit analyses of the probability of forced migration

	Model I				Model II			
Variable	Odds ratio	B	r. s.e. (B)	Z	Odds ratio	B	r. s.e. (B)	Z
PTS_{t-1}	1.275	0.243	0.158	1.54	–	–	–	–
$CIRI_{t-1}$	–	–	–	–	1.270	0.239**	0.077	3.10
$Genocide_{t-1}$	0.730	−0.314**	0.119	−2.65	0.773	−0.257*	0.123	−2.08
Violent $dissent_{t-1}$	1.028	0.027	0.06	0.48	0.981	−0.019	0.060	−0.31
Civil War_{t-1}	2.717	1.000*	0.473	2.11	3.836	1.344**	0.430	3.13
Institutional $freedom_{t-1}$	1.071	0.069*	0.036	1.91	1.045	0.044	0.035	1.26
$Transition_{t-1}$	0.512	−0.669	0.427	−1.57	0.698	−.359	0.501	−0.72
GNP_{t-1}	1.0	$-7.80e^{-14}$	$1.69e^{-13}$	−0.46	1.0	$-7.06e^{-14}$	$3.72e^{-13}$	−0.19
FM Stock $Dummy_{t-1}$	3.434	1.233**	0.452	2.73	3.449	1.238**	0.457	2.71
Years since FM	1.097	0.092	0.241	0.38	1.101	0.096	0.241	0.40
Spline 1	0.830	−0.187*	0.110	−1.70	0.829	−0.188*	0.110	−1.70
Spline 2	1.013	0.013	0.062	0.21	1.018	0.018	0.063	0.29
Spline 3	1.475	0.389	0.295	1.32	1.471	0.386	0.294	1.31
N	568				546			
Wald chi-square	57.73***				64.66***			
(df)	(11)				(11)			
Log pseudo-likelihood	−206.805				−192.073			
Pseudo-R^2	0.164				0.187			

*$p \leq 0.1$.
**$p \leq 0.01$.
***$p \leq 0.001$.

of those rights, from full respect to partial, or from partial respect to none. A government program of repression and human rights abuse is likely to have specific goals. In that spirit, Moore (2000) argues that states substitute repression for accommodation, and vice versa, in response to dissident protest. Similarly, Davenport (1995) suggests that repression is a response by regimes to domestic threats. If it is indeed the case that human rights abuse is goal-driven behavior, it makes sense that physical integrity violations are applied with some consistency over time.

Figure 1 indicates that the partial introduction of torture to a society formerly free of physical integrity abuse increases the probability of forced migration by 0.06, from 0.5 to 0.56. Increases of similar magnitude are associated with the partial introduction of imprisonment and killings, and the full violation of freedom from imprisonment.

This effect tapers off slightly as CIRI's scale increases from 5 (full freedom from disappearances, partial freedom from killing, and no freedom from imprisonment or torture) through 8 (no government respect for any physical integrity right). Thus a change from 7 (partial freedom from disappearance and no freedom from any other right) to 8 corresponds

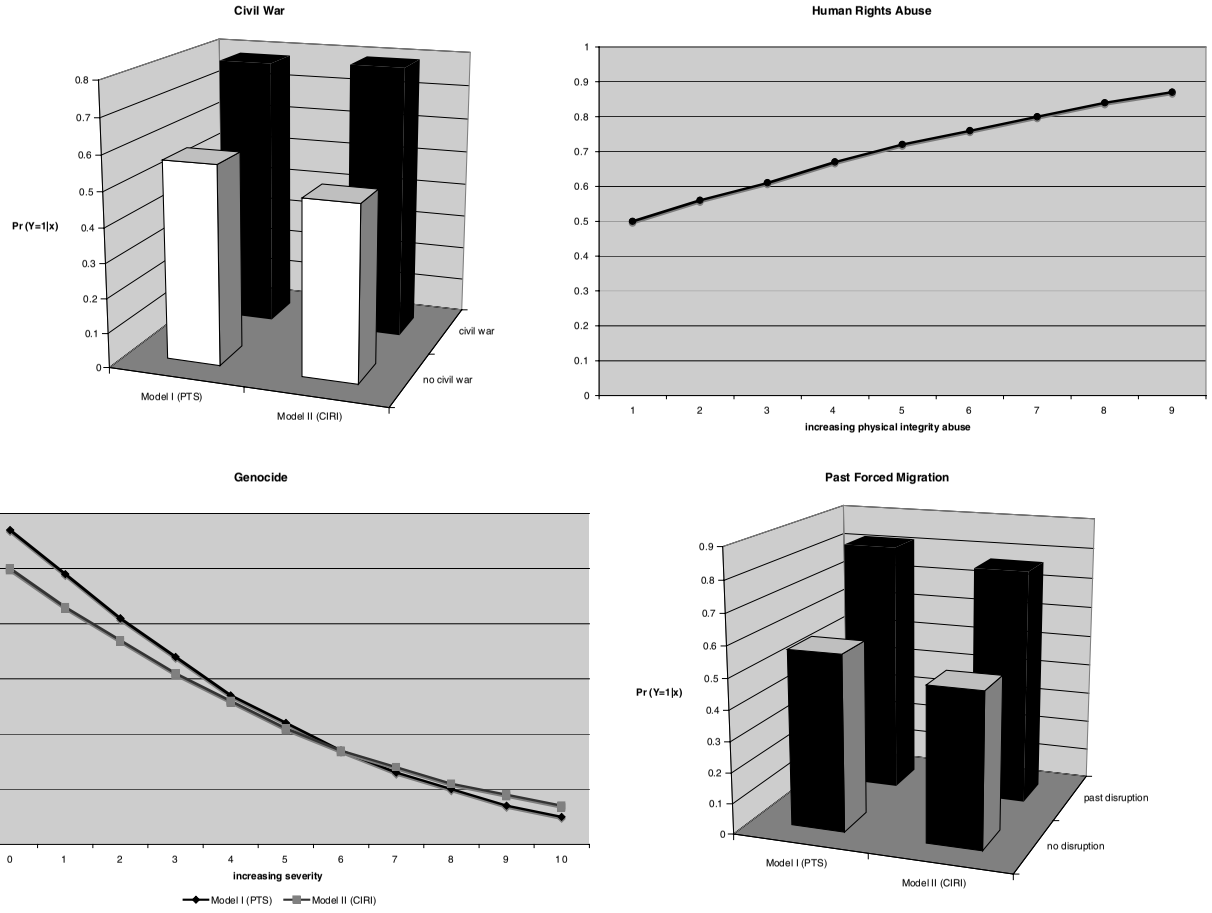

FIGURE 1 Effects of perceived threat on the probability of forced migration in the next period.

TABLE 2 Physical integrity scale scores and Mokken scale predictions of patterns of government respect for particular physical integrity rights: the pattern of respect

CIRI Scale Score	Our Scale Score	Government Respect for Physical Integrity Rights			
		Disappearances	Killing	Imprisonment	Torture
8	0	Full	Full	Full	Full
7	1	Full	Full	Full	Partial
6	2	Full	Full	Partial	Partial
5	3	Full	Partial	Partial	Partial
4	4	Full	Partial	None	Partial
3	5	Full	Partial	None	None
2	6	Partial	Partial	None	None
1	7	Partial	None	None	None
0	8	None	None	None	None

This table replicates Cingranelli and Richards' (1999) Table 3. However, because we reverse their coding of the physical integrity scale, our scale scores are inverted.

to a 0.03 increase in the probability of forced migration in the following year, from 0.84 to 0.87. While these are rather modest increases in probability, the cumulative effect of physical integrity abuse is substantial: A change from full government respect for human rights to no respect yields a 0.37 increase in the probability of forced migration, from 0.5 to 0.87. When physical integrity abuse reaches a maximum (and other factors are average), forced migration is expected with nearly a 0.9 probability. The results suggest that the CIRI measure of human rights abuse is a useful risk factor for assessing the probability of a forced migration effect.[22]

Moving on, Table 1 shows that civil war has a positive effect on the probability of observing a forced migrant event in the following year. More specifically, the odds ratios indicate that when civil war was present in the preceding year, the odds of forced migration are increased by a factor of 2.7 (in Model 1) or 3.8 (in Model 2). Because civil war is a binary variable, we use a bar chart to depict the expected change in the probability of forced migration given the introduction of a civil war in the previous year (see Figure 1, panel 1). Where civil war was present in the preceding period, the probability of forced migration is 0.78 in Model 1, and 0.79 in Model 2. In the first case, introducing civil war and holding all else constant produces a 0.21 increase in expected probability; in the second, the increase is 0.29.

Over the period from 1945 through 1991, civil wars broke out at a rate of about 2.3 per year, and ended at a rate of about 1.85 per year (Fearon, 2004; see also Fearon & Laitin, 2003; Collier et al., 2004). Thus, the average duration of civil wars has been steadily increasing over the postwar period. The fact that civil war is observed this year makes it likely that civil war will be observed in the coming months, and that individuals will increasingly feel threatened, making them more likely to become forced migrants.

[22]We have argued that the different findings across measures of human rights abuse are attributable to CIRI's systematic treatment of abuse. Both the Political Terror Scale and the CIRI physical integrity index used here combine an inherently categorical index into a single variable. Rubin is currently working on a paper that investigates the multidimensional implications of physical integrity abuse, including the possibility that the contemporaneous consequences of that abuse vary across repressive method. In the future, extending that possibility to a risk factor model could provide further insight into the phenomena that precede or predict refugee flight.

In addition to civil war and human rights abuse, we explore the usefulness of genocide as a risk factor for forced migrant events. In both models, genocide produces a statistically significant coefficient in the expected direction. Specifically, and all else equal, a unit increase in the magnitude of genocide *decreases* the odds of future forced migration by a factor of 0.73 in Model 1, and 0.77 in Model 2.

The expected change in the probability of future forced migration as a function of increasingly severe genocide is depicted in the third panel in Figure 1. Not unlike the effect of physical integrity abuse, the impact of genocide diminishes as it becomes increasingly intense. Thus, the introduction of genocide involving less than 1,000 deaths makes forced migration in the next period less likely, decreasing the probability from 0.57 to 0.49 in Model 1 and from 0.50 to 0.43 in Model 2, and increasing the death toll from 128,000 to 256,000 decreases the same probability by 0.02 (so that the probability declines from 0.07 to 0.05 in Model 1, and from 0.09 to 0.07 in Model 2).

Past studies have demonstrated that genocide is significantly, positively, and *contemporaneously* related to forced migration (Schmeidl, 1997; Davenport et al., 2003; Moore & Shellman, 2004). Our findings may therefore lead some readers to question the accuracy of our theory or its specification. However, as we noted in an earlier section, the temporal dimension of risk factor analysis leads us to expect this changing sign. Here, we briefly explain our logic.

There is a strong literature suggesting that war and political upheaval are familiar predecessors to ethnic conflict (Gurr, 1994) and genocide (Fein, 1993; Licklider, 1995; Rummel, 1995; Krain, 1997). "Political upheavals and internal wars. . .are antecedents of most humanitarian emergencies" (Harff and Gurr, 1998, p. 556). These are the same influences we believe threaten individuals, inspiring them to flee their homes and become forced migrants. When juxtaposed, our argument and the literature's implications make several points.

First, genocide and forced migration should be contemporaneously correlated. This is the effect reported in the literature. Second, and relatedly, genocide should *not* be a systematic predecessor to forced migration. Instead, we suggest that forced migration may actually be a response to the anticipation of genocides and the early killings in the event. People relocate *prior to* and *during*, but not *after* genocide.

The coarse temporal units of available data restrict our ability to find support for this argument. Nonetheless, it seems useful to consider what we would expect to find given such data. Consider the following general model, where FM is forced migration, G is genocide and t is time:

$$FM_{t+1} = f(G_{t-3}, G_{t-2}, G_{t-1}, G_t, G_{t+1}, G_{t+2}, G_{t+3})$$

Genocide tends to be a short-lived event. Thus given weekly data, we might expect G_{t-1}, G_t, G_{t+1}, and perhaps G_{t+2} to produce statistically significant positive effects, capturing forced migration in anticipation of (G_{t-1} and G_t), along with (G_{t+1}), and immediately following (G_{t+2}) the onset of genocide. Yet, given data aggregated to the monthly level, we would expect only G_t and G_{t+1} to produce statistically significant effects, since when genocide occurs at t it is likely over by the time the data observe forced migration at $t+1$.

When temporal aggregation reaches the annual level, genocide events last for a fraction of an observation. In other words, annual genocide data likely lumps the anticipation, execution and termination of genocidal activities into a single observation. Given this coarse aggregation, genocide occurring at t is unlikely to lead to a delayed forced migration event a year later at $t+1$. Thus the only positive and significant effect one would expect to find when using annual data is the contemporaneous one reported in the literature. Moreover, it is unlikely that a genocide event will continue for two years. Empirically, when G_t is coded

1, G_{t+1} will generally be 0. Given that FM_t is likely to equal 1 when G_t equals 1, then, the relationship between FM_{t+1} and G_t should be—and is—negative. The key point for this analysis is that, rather than using genocide as a risk factor for forced migration, refugee and IDP flows may actually predict genocide and politicide.[23]

We turn next to our proxy measure of diaspora culture and information networks. The parameters for the past migration variable is positive and significant in both models. Substantively, past experience with forced migration increases the odds of current forced migration by a factor of 3.43 in Model 1, and 3.45 in Model 2. This effect is the strongest among all the variables in Model 1, and is second only to civil war in Model 2. The final panel in Figure 1 depicts the expected change in the probability of forced migration given the introduction of past experience with the same. In Model 1, the consequence of past forced migration is a 0.25 increase in the probability of forced migration in the current year, from 0.57 to 0.82. In model 2, the expected increase is 0.27, from 0.50 to 0.77. In general, past forced migration is a strong predictor of a current forced migration event.

Next, we turn our attention to the structural variables we expected might serve as risk factors. The variables we examined were institutional freedom, regime transition, and our income proxy, GNP per capita. Of these, only the first produced a statistically significant estimate.

Institutional freedom, measured with the level of democracy, is positive and statistically significant in Model 1. This is the opposite effect of what we expected. The institutional freedom variable is coded ordinally from −10 to 10, with higher values indicating increasingly democratic institutions. *Ceteris paribus*, a 1-unit move toward more democratic institutions increases the odds of future forced migration by a factor of 1.07. Each unit increase in democracy corresponds to a roughly 0.015 greater probability of future forced migration. Finally, a change from fully autocratic (−10) to fully democratic (10), accomplished over a 12-month period, would make forced migration more likely, increasing the probability by 0.29 from 0.52 to 0.81. This effect is depicted graphically in Figure 2.

What might account for the unexpected positive effect? Zolberg et al. (1989, pp. 16–17) note that while authoritarian regimes often impose restrictions that limit migration, democratic polities are more likely to limit entry than exit (see also Larrabee, 1992). It seems possible that shifts toward democracy proxy change in policies where borders that were formerly closed became open. Perhaps our result indicates that an increase in democracy in one year presents individuals with the "opportunity" to become forced migrants in the following year, thus predicting an increase in the probability of forced migration. A change toward democracy, by capturing policy changes that include border openings, may be associated with a short-run increased probability of forced migration (Newland, 1993). While plausible, this explanation does not sit well with reported findings of a negative contemporaneous impact of democracy on the number of forced migrants, so additional analyses are needed to sort out the usefulness of democratic institutions as a risk factor.[24]

[23]Several anonymous reviewers questioned whether the genocide finding is a consequence of multicollinearity among the variables on the right side of our equations. We examined the possibility in two ways. First, we looked at the correlations among our independent variables. Only 6 two-way correlations in a 9×9 matrix exceeded 0.28, and the highest of these was between the Political Terror Scale and civil war, which correlated at 0.54. Second, we examined the variance-inflation factor (VIF) for each independent variable: $\text{VIF} = \frac{1}{1-R_k^2}$. "The VIF for a variable shows the increase in [the variance of any independent variable] that can be attributable to the fact that this variable is not orthogonal to the other variables in the model" (Greene, 2003, p. 57). No variable in our analyses had a VIF greater than 1.74, and the mean VIFs were 1.3 (for model 1) and 1.27 (for model 2). On these grounds, we are as confident as we can be that our findings are not statistical artifacts, but meaningful estimates from which we may draw inference.

[24]See footnote 13.

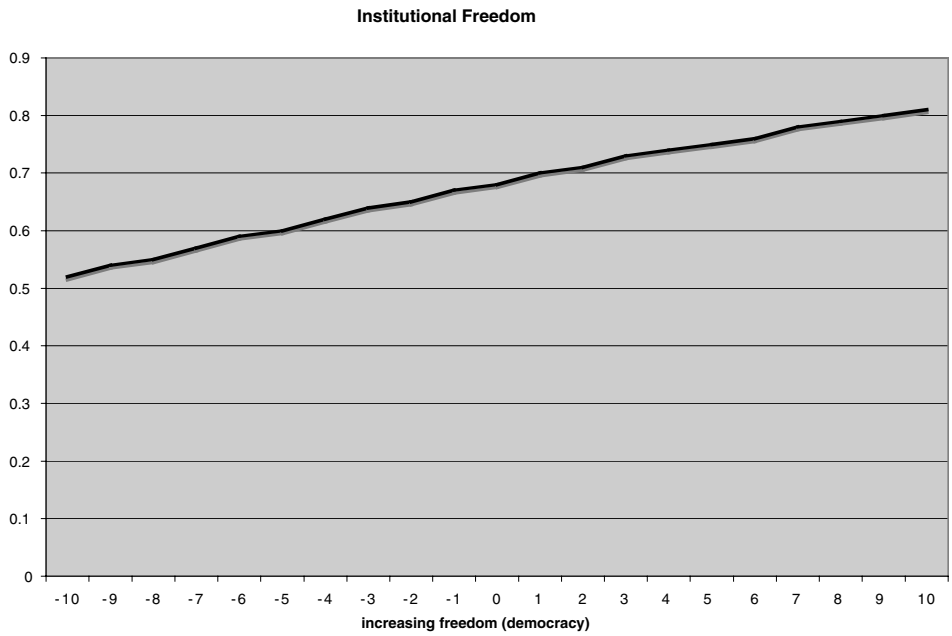

FIGURE 2 Effect of increasing institutional freedom on the probability of forced migration in the next period.

Conclusions

To recapitulate our findings, when using annual data the strongest risk factors of an impending forced migration event are civil war and the presence of a forced migration event in the preceding period. Physical integrity abuse is also significantly related to the probability of forced migration in the following year. Because genocide makes victims of potential forced migrants, it is negatively related to future migrant populations, and is not a useful risk factor. Finally, our study produced a curious finding: institutional democracy produced a statistically significant coefficient with the opposite sign from what we expected, and we speculated that it might be explained by democracies' superior ability to control their borders. We thus submit that our analysis yields three useful risk factors and one anomaly requiring further inquiry.

Does this list of risk factors represent a contribution that might be useful to those who have to create contingency plans? It is interesting to compare our list against the list of early warning indicators published by the UNHCR (1996, Annex B). As noted earlier, an advantage that a statistical model has over a seat-of-the-pants analysis is that the former produces specific estimates about the change in probabilities. The three risk factors our study recommends can account for considerable shifts in the probability of observing an event the following year. How do those three indicators compare with the list of thirty indicators identified by the UNHCR as factors triggering, prompting or otherwise influencing departure?

Our model directly captures four variables (human rights abuse, opposition movements, past forced migration and institutional freedom) that measure six of those 30 risk factors. Two emerge as risk factors. Another eight are likely present in our data, since the concepts underlying the UNHCR indicators are reflected by variables in our models. For example, ethnic, social, and religious tensions, each listed by the UNHCR as an early warning indicator, will influence our measures of dissident violence and civil war. Of the remaining

early warning indicators, three deal with environmental issues (seasonal considerations, terrain passability, and natural disasters), and three deal with third-party influence (military intervention, international relief, and international protection). These, as well as economic instability and government corruption, are identified by the UNHCR but absent from our theory (and therefore, our model), but may be important risk factors for forced migrant flight. Our study suggests that real-time monitoring of the three risk factors of civil war, PTS or CIRI indicators of human rights abuses, and the presence of reufgee/IDP flows in the previous year can give one a specific prediction about the change in probability of observing a forced migrant event in the coming year. In addition, the UNHCR's early warning list could be monitored in a seat-of-the-pants fashion to adjust the probability accordingly. Given the coarse temporal aggregation of our data, this is only useful for broad-gauge contingency planning, perhaps most useful for deciding where to deploy analyst time with respect to monitoring the more comprehensive UNHCR list.

Having discussed risk assessment we turn our attention to opportunities for improving upon this study. Davenport et al. (2003) differentiate "push" factors from "pull" factors related to forced migration. The former are influences on the decision to *leave* home; the latter are destinational considerations. Both push and pull influences may be risk factors for forced migration; however, data limitations required us here to concentrate solely on the reasons an individual would leave. Future work involving pull factors should consider a series of early warning indicators we were unable to involve here, including the influence of foreign groups and governments, and relations with and situations in neighboring countries (particularly asylum policies).

With this work in hand, we turn to what should be done next. The answer, it seems to us, is broadly twofold. First, more work needs to be done to tease out the effects of the risk factors we identified, and potentially to identify additional indicators. Most important here will be the disaggregation of data both on indicators and on subsequent forced migrant flow. As noted earlier, we explore annual data; therefore, the effect of significant variables in this model is exerted up to twelve months before forced migration is observed. Future efforts to identify useful risk factors would do well to break observations into more finely grained levels of aggregation.

Once our model has been revisited and refined, the second step will be an assessment of its usefulness for contingency planning. Because we are generally interested in hypothesis testing, political scientists tend to focus on the behavior of individual variables within a model. Our interest here, for example, is in identifying observables that can meaningfully predict forced migration. However, contingency planning places as much emphasis on the overall fit of the model as on the significance and behavior of the variables that enter into it. Out-of-sample forecasting would give an accurate idea of the model's predictive ability across time and over space, and strikes us as a logical direction for future research.

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