

Fear of Persecution

FORCED MIGRATION, 1952-1995

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Why would people abandon their homes in favor of an uncertain life elsewhere? The short answer, of course, is violence. More specifically, the authors contend that people monitor the violent behavior of both the government and dissidents and assess the threat such behavior poses to their lives, physical person, and liberty. The greater the threat posed by the behavior of the government and dissidents, the larger the number of forced migrants a country will produce. To test hypotheses drawn from this argument the authors use a global sample of countries over more than forty years. Their findings are held to be consistent with their argument, showing that violent behavior has a substantially larger impact on forced migration than variables such as the type of political institution or the average size of the economy.

Keywords: forced migration; refugees; internally displaced persons

This study explores the determinants of forced migration. Specifically, we focus on the push factors that cause people to flee their homes. Our motivating question is, what characteristics of countries lead some to produce large numbers of forced migrants and others to produce none at all? More precisely, what characteristics of countries help explain the variation, over time and across countries, in forced migration movements?

The question is important, as recent events have demonstrated. At the global level, the United Nations High Commissioner for Refugees (UNHCR) reports that the total

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stock of forced migrants reached slightly more than 20 million in 2003.¹ The large number of victims has led several observers to talk about a global refugee crisis.² The crisis has at least two dimensions. First is the humanitarian dimension. Individuals have the fundamental human right to live free of fear of persecution, yet there are many victims who do not live free of this fear.³ Second, forced migration produces negative consequences in the international system. Most observers argue that forced migration imposes substantial costs on national and regional economies, and still others argue that forced migration reduces security. Therefore, the determinants of forced migration are important to understand.

Some definitions are in order before we turn to a characterization of the state of inquiry on the topic. We consider a forced migrant to be one who, owing to a fear of persecution, has abandoned her or his dwelling in favor of relocating elsewhere, either within or beyond the borders of her or his country of residence. This definition builds on those for refugees and internally displaced persons (IDPs) as codified in international law. The 1951 United Nations Refugee Convention uses the “fear of persecution” clause to identify those who seek refuge abroad as people for whom state parties to the convention are legally bound to provide refuge. The UNHCR defines IDPs as “people [who] are also forced to flee . . . but they either cannot or do not wish to cross an international border” (UNHCR n.d.). Forced migrants of country *X*, then, are the sum of refugees and IDPs from country *X*.

The literature on this topic is interdisciplinary and broad, yet few political scientists have tackled the question.⁴ Studies of (voluntary) migration form a vast literature, and below we use some of the insights garnered from a basic migration model to motivate our own ideas.⁵ Here, however, we briefly sketch the status of work on forced migration. Three pertinent characteristics stand out. First, the literature on forced migration is largely idiographic. Descriptive case studies, advocacy and awareness pieces, and policy evaluations dominate what is a fairly large literature composed primarily of books and monographs, although some journal articles are included. A handful of theoretically driven, broadly comparative studies do exist (e.g., Clark 1989; Zolberg, Suhrke, and Aguayo 1989; Schmeidl 1997; Gibney, Apodaca, and McCann 1996; Weiner 1996; Apodaca 1998; Davenport, Moore, and Poe 2003), and we review those below.

Second, the literature is primarily systemic/structural in its theoretical orientation. This is certainly not true of the (voluntary) migration literature: the canonical model in this literature is ground in microfoundations. Yet the theoretical literature on forced migration tends to take the country or society as the unit of explanation and seeks to

1. See <http://www.unhcr.ch/> (accessed May 2004).

2. For example, see Weiner (1995); Loescher and Loescher (1994); and Zolberg, Suhrke, and Aguayo (1989).

3. Although the United Nations High Commissioner for Refugees (UNHCR) reports 20,556,781 displaced persons in the world, few people—and, in any given year, few countries—experience forced migration. Forced migrants represent only .003 of 1% of the world’s population (20 million forced migrants compared with 6.1 billion people; United Nations Population Fund [UNFPA] 2001]). Our models should be able to account for the relative rarity of this event.

4. Zolberg, Suhrke, and Aguayo (1989); Weiner (1992-1993, 1996); Gibney, Apodaca, and McCann (1996); Apodaca (1998); and Davenport, Moore, and Poe (2003) are studies by political scientists.

5. See Faist (2000, chap. 2) for a useful overview of the literature.

identify macrolevel concepts that account for forced migration flows. Like Davenport, Moore, and Poe (2003) and most studies of (voluntary) migration, we eschew a macrolevel orientation in favor of a microlevel analysis that produces hypotheses about macrolevel observables.⁶

Third, the empirical analyses of data in this literature are not particularly strong. Several of the studies suffer from selection bias because they examine only countries that produced refugees (e.g., Zolberg, Suhrke, and Aguayo 1989; Apodaca 1998). The two comparative studies with strong empirics are Schmeidl (1997) and Davenport, Moore, and Poe (2003). Schmeidl focuses on refugee stocks in the third world from 1971 to 1990, whereas Davenport, Moore, and Poe focus on net forced migrant flows throughout the world from 1964 to 1989. Although the theoretical models are rather different, the empirical analyses share a number of similarities. As such, they provide a baseline against which to judge the findings reported below.

Schmeidl (2000, 152) takes a structural approach to explaining forced migration, arguing that “refugees and IDPs flee from similar root causes rather than responding to completely different occurrences.”⁷ Following work by Clark (1989), Schmeidl (1997) distinguishes three types of factors that influence forced migration: root causes, proximate conditions, and intervening factors. This approach focuses on the structural characteristics of countries and societies and fails to connect the behavior of human beings (i.e., governments and dissident groups) to the behavior of other human beings (i.e., forced migrants). Instead, countries and societies are implicitly conceptualized as Eastonian (1965) systems in which inputs (violence, etc.) produce outputs (i.e., forced migrants).

The alternative approach charted in the literature is taken by Davenport, Moore, and Poe (2003), who begin with the choices of individual human beings. They argue that it is important to conceptualize people as making a choice to leave. They observe that in any given episode of forced migration, although many—and sometimes most—people leave, others stay. To explain why many individuals would leave, they identify the major point of agreement in the literature: people abandon their homes when they fear for their liberty, physical person, or lives.

An additional contribution of Davenport, Moore, and Poe (2003) is to conceptualize countries/society as composed of groups of people competing for political power. They argue that the source of threat is the behavior of these groups, and they argue that statistical models of forced migration should specify the behavior that people will find threatening. This focus on the behavior of actors in political competition contrasts with the theoretical approach represented by the Schmeidl studies (1997, 1998, 2000), and we build on this focus.

That said, our study produces several findings that augment or qualify findings reported in the studies by Schmeidl (1997, 1998, 2000) and Davenport, Moore, and Poe (2003). Several of these distinctions are produced by alternative (and, we submit,

6. Faist (2000, 30-35) provides a useful discussion of microlevel, mesolevel, and macrolevel theorizing in the migration literature.

7. Although we do not distinguish root causes from other causes, our definition of forced migrants as the sum of refugees and internally displaced persons (IDPs) is built on the assumption that common factors influence both. In another study, we relax this assumption and explore the determinants of refugee versus IDP flows (Moore and Shellman 2004a).

superior) conceptual and/or operational choices. Our study also opens new directions for research not anticipated by previous studies.

The study proceeds in four sections. We first lay out the argument about the macrolevel information we expect people to monitor when making a decision whether to stay or go. In the second section, we describe our research design and operational measures; and in the third section, we discuss our findings. We conclude with a discussion of the ramifications of the findings and our plans for further inquiry.

STAY OR GO? THE ARGUMENT

We begin with the question, why would one abandon one's home? To fix our thinking about the question, we propose the following stylized scenario. Imagine that an individual is faced with the choice to stay in her home or abandon it and pursue her life elsewhere. If she chooses to relocate, she will not receive compensation for her property, and she cannot take much with her beyond what she can carry. Clearly, staying is preferable to leaving. Imagine further, however, that she is now presented with a lottery where she is going to be the victim of persecution with some probability, $p \in [0, 1]$. As p rises from 0 to 1, we submit that (for most people) there is some threshold value that will lead them to prefer leaving to staying. By this account, then, one of the important theoretical tasks for building a statistical model of forced migration is to specify the factors that will influence peoples' perceptions about the value of p .

To develop hypotheses from this simple story, we need to make some assumptions about how people make decisions and about the information that they would use to assign a value to p . Assume that people are (1) purposive; (2) value their liberty, physical person, and life; and (3) develop beliefs about the behavior of actors in society with respect to those values. The first two assumptions are standard rationalist kinds of assumptions. The third suggests that people will develop expectations about the value of p by observing the behavior of political actors in their country. In other words, we submit that in each country, there exists an information set that is available to all members of that country about the behavior of actors and that people in society make use of that information set to assign a value to p . It will also be true that many people have private information, but in this study we focus on the public information that is held in common. We do so because we are interested in understanding forced migration aggregated at the country level, and commonly available information will influence aggregate outcomes.

To summarize our argument, we examine how the information environment affects an individual's decision to stay or go.⁸ In short, one will leave one's home when the probability of being a victim of persecution becomes sufficiently high that the

8. This study implicitly assumes that the characteristics of neighboring countries do not affect forced migration flows. This is a strong assumption. We relax it in subsequent studies and explore the extent to which pull factors affect forced migration flows. In one study, we focus on the differences between countries that tend to produce large numbers of refugees relative to IDPs (Moore and Shellman 2004a), and in another, we focus on the differences between alternative asylum countries that tend to attract or deter refugees (Moore and Shellman 2004b). However, for the purposes of this study, we concentrate only on how the benefits of staying and the costs of staying impact an individual's decision to stay or go.

expected utility of leaving exceeds the expected utility of staying. Below, we develop specific hypotheses that follow from this argument.

THE INFORMATION SET FOR P

Our second assumption is that people value their liberty, physical person, and life. The third assumption suggests that people will monitor threats to these values. To produce hypotheses about the covariates of forced migration, we must identify the potential sources of threat. Following Davenport, Moore, and Poe (2003), we identify four sources of such threat: government forces, dissident forces, the interaction of government and dissident forces, and foreign troops. The greater the threat by these groups in a given country, the greater p will be and the more forced migrants we will expect that given country to produce. We briefly discuss each source of threat and conclude the section with a discussion of culture and its effect on forced migration.

Most scholars writing on the topic identify government (sponsored) violence as a determinant of forced migration. Some focus on human rights violations and repression (Hakovirta 1986; Gibney, Apodaca, and McCann 1996; Apodaca 1998), others focus on genocide (Jonassohn 1993; Rummel 1994; Jonassohn and Björnson 1998), some emphasize both (Zolberg, Suhrke, and Aguayo 1989; Schmeidl 1997), and still others emphasize state victimization of ethnic minorities (Newland 1993; Kaufmann 1998). It is not difficult to think of cases that fit this pattern: Argentina, Chile, and Cambodia in the 1970s and South Africa in the 1970s and 1980s.

Almost all of this work assumes that the connection between state behavior and forced migration is obvious, yet explicit causal arguments are rare. We connect the two by linking the behavior of the government to peoples' expectations about the future behavior of the government and, thereby, to peoples' perception of threat to their liberty, physical person, and/or life. Because we are studying forced migration at the country level, we are especially interested in publicly observable government behavior that large groups of people will reasonably find threatening. Human rights violations and genocide/politicide are overt behaviors that fit the bill. Covert actions that are not publicly visible do not.

Whereas most researchers focus on state violence, a few scholars consider dissident violence (e.g., Hakovirta 1986; Davenport, Moore, and Poe 2003). Cases such as Angola, Mozambique, and Peru in the 1980s and Sierra Leone in the 1990s, where the dissidents were responsible for the lion's share of the human rights abuses, illustrate the importance of recognizing that although states are often the major threat to their populations (Rummel 1994), the dissident groups are also a potential threat and may be the major source of threat that produces forced migration.

The connection between dissident behavior and forced migration is the same as above: current publicly visible behavior serves as a foundation that people use to develop beliefs about future dissident behavior. Those beliefs affect the perceived cost of staying, and thus we expect that the larger the number of violent dissident activities in a given country, the larger the number of forced migrants it will produce.

The arguments raised above focus on the government and dissidents in isolation from one another. Yet states and dissidents interact, and we suspect that the violence

resulting from that interaction will have an independent effect on peoples' perceived sense of threat. Afghanistan, Colombia, and Sri Lanka in the 1980s and 1990s serve as exemplary cases. Others have argued that civil war has a positive impact on forced migration (e.g., Hakovirta 1986; Zolberg, Suhrke, and Aguayo 1989; Newland 1993; Schmeidl 1997), although the linkage is generally not developed explicitly.

Our argument parallels the ones made above. High levels of violence during civil war are obviously public. We contend that as the level of violence increases in state-dissident contests, people will revise their beliefs about the threat to their liberty, physical person, and/or life. As such, we expect that countries that experience civil wars will produce more forced migrants than those that do not.

Like civil war, international war creates an environment of generalized violence that can threaten populations. Scholars note that the decline in frequency of international war since World War II has diminished war's importance as a cause of refugee flows (Weiner 1996), but it is still a potential source of threat. The presence of active foreign troops on one's soil is certainly public, and thus we expect that when foreign troops are fighting on one's soil, an individual is more likely to feel threatened. We thus hypothesize that countries fighting an international war on their territory will produce greater numbers of forced migrants than those that are not.

This hypothesis is different from those advanced by Schmeidl (1997) and Davenport, Moore, and Poe (2003), who argued that participation in international war will be positively associated with refugee and net migration flows, respectively. As such, both studies measured national involvement in war, whereas we conceptualize the issue as the threat created by soldiers engaged in war on one's own territory and thus measure it differently. Schmeidl found support for the hypothesis, whereas Davenport, Moore, and Poe did not.

Finally, we argue that people live in cultural communities that are critically important to them. We conceptualize culture broadly to include familial ties, language, religious practices, and traditions—including forms of dress, food, music, and leisure activities. In addition, migrant communities form networks that provide information and the cultural space to make migration an option for others who stayed at home (Massey et al. 1993, Faist 2000). Thus, an initial migration flow tends to generate future migration when others follow the initial migrants.

Although forced migration is distinct from voluntary migration, we do expect that diaspora culture will have an impact on forced migration flows. We contend that as more and more people are forced from their homes, a person's family, culture, and ethnic ties begin to break down and the costs of staying increase.⁹ As a result, we expect to find a positive association between the current stock of forced migrants and contemporaneous forced migration flows.

We focus our argument on the information people use to assess p , but previous research suggests that other factors are likely to influence forced migration levels. To provide a conceptual category, we can think of these factors as the benefits of staying. We suggest that there are two benefits that are important to consider when modeling forced migration: income and freedom. By *income*, we mean economic opportunity in

9. Our thinking here is similar to the network argument put forth by Schmeidl (1997) and Davenport, Moore, and Poe (2003), who are building on Massey (1988).

its broadest sense, including wages, salary, or the ability to till the soil. The voluntary migration literature focuses on income as the major, although not the sole, explanatory variable (Stalker 2000, 21-25). Furthermore, those writing on voluntary migration who discuss forced migration argue that income plays a role in forced migration decisions as well (Stalker 2000, 32). This suggests that countries with higher income opportunities will be less likely to produce refugee flows.

Turning our attention to the second variable, when we speak of *freedom*, we mean political freedom and the rule of law. We 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. Weiner (1996); Schmeidl (1997); and Davenport, Moore, and Poe (2003) make similar arguments. This leads us to expect that the greater the political freedom and rule of law in a country, (1) the lower the probability that a country will produce a forced migration event and (2) the fewer forced migrants a country will produce in those cases where an event does occur.

Although we expect the benefits of staying to have a systematic impact on forced migration, we expect these variables to have a smaller impact on forced migrant levels. Just as the voluntary migration literature focuses on income as the major explanatory variable, we submit that information about different types of violence will be the most influential explanatory variables.

RESEARCH DESIGN

OPERATIONAL INDICATORS

We measure the dependent variable differently than previous scholars. Schmeidl (1997) studies a measure of refugee stocks, and Davenport, Moore, and Poe (2003) study a measure of the net stock of forced migrants. Because we are studying the decision to abandon one's home, we include both IDPs and refugees in the analysis, whereas Schmeidl included only refugees. Furthermore, because we examine in separate analyses the questions of whether to resettle in one's own country or abroad and, if abroad, in which country, we focus our attention in this study solely on the national production of forced migrants. Davenport, Moore, and Poe examine both "push" and "pull" factors in their study, but we analyze those issues as distinct questions and thus limit our attention in the present study to forced migrants produced by each country.

That said, the data on forced migrants are, not surprisingly, rather noisy, and there are a variety of issues one must consider. First, virtually no flow measure of forced migrants is available. Instead, the most valid and reliable measures of forced migration are estimates of the stock of refugees from a given country of origin and estimates of the stock of IDPs in any given country of origin (Schmeidl 1998, 2000). Thus, we had to create the flow measure from the stock measure. To do so, we first calculated the sum of the refugee and IDP indicators to create a forced migrant stock variable. Then we took the first difference (i.e., calculated the change in stock from one year to the next) and truncated the negative values at zero. We did so because we are interested in

the first wave of forced migrant movements, not secondary movements or repatriation. Therefore, our flow measure has a distribution from zero to more than 1 million.

The data themselves come from two sources. The refugee data are the official estimates from the UNHCR's (n.d.) statistics division, and the IDP data are unpublished data from the Global Refugee Migration Project (Schmeidl and Jenkins 1999). These are the best available measures of refugee and IDP stocks.¹⁰

We use two variables to measure government threat: genocide and politicide events and violations of human rights. To measure genocide and politicide, we use Barbara Harff's (2003; Harff and Gurr 1988, 1996) data collected for the State Failure Project.¹¹ She produces a list of these events and an ordered scale measure of the annual number of deaths for each event. We use the latter "magnitude" measure but rescale it.¹² This variable is available for all countries and years in our sample.

We are also interested in the extent to which the state engages in repressive behavior, targeting the population at large or targeting people because of their behavior rather than their ethnicity or beliefs. We use the political terror scale (Gibney and Dalton 1996; see also Gibney, Apodaca, and McCann [1996] and Apodaca [1998]) to measure human rights violations. It is an ordered scale ranging from 1 to 5 where higher values represent greater violations of the physical integrity of the person. The data are based on a content analysis of two sets of annual reports: Amnesty International and the U.S. Department of State (see Gibney and Dalton 1996). Two variables are created, one for each source, and we estimate parameters using each to ensure that the results are consistent across both sources. However, we report only the results for the State Department variable below. This variable is available for the years from 1976 to 1996 and most countries in our sample.¹³

To measure dissident threat, we use Banks's (n.d.) cross-national time series archive data set to generate an event count measure of the number of times dissidents used violence in a given country-year. Banks records the number of guerrilla attacks and the number of riots that occur in each year in a country. We count the sum of both event types.¹⁴

10. The UNHCR data cover the period from 1951 to 1999 and are the product of an extensive, multiyear effort by Bela Hovy's team. The IDP data cover the period from 1970 to 1997 and were put together by Susanne Schmeidl, working with Craig Jenkins. They are also the product of an extensive, multiyear effort (Schmeidl also worked with Hovy). See Schmeidl (1998, 2000) and Crisp (1999, 2000) for detailed discussions about the strengths and weaknesses of these data. *The Economist* (2002) provides a similar critique of migration data. A key theme in Crisp's work is that different actors have different incentives to inflate or deflate the estimates of IDPs and refugees. Because we cannot say in a large data set, such as ours, whether the average bias is positive or negative, we cannot judge whether the size of the effects we report are overestimates, underestimates, or about right.

11. The data are available at the State Failure project's Web site: <http://www.cidcm.umd.edu/inscr/stfail/>.

12. Harff's (2003) scale assigns a value of 0 for less than 300 deaths and then increases 0.5 points in a nonmonotonic fashion. We simplified the scale, recoding it as follows: 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.

13. Below we report the parameter estimates we obtained using the U.S. State Department scores. The findings were similar when we used the Amnesty International variable (see Gibney and Dalton 1996).

14. This operationalization is similar to, but distinct from, the approach outlined in Davenport (1995) and used in Davenport, Moore, and Poe (2003). Davenport argues that overt government repression is partially driven by dissident activity. More specifically, he contends that governments respond to four

To measure government-dissident interaction, we use the Correlates of War extrasystemic and civil war data set (Sarkees 2000). To qualify for this data set, a conflict must have at least 1,000 battle deaths. The extrasystemic data set lists conflicts between colonized peoples and the colonial metropole. We include the extrasystemic conflicts because inspection of the UNHCR data revealed that some of the extrasystemic conflict produced refugee flows that were recorded by the UNHCR. We are not certain what rule UNHCR followed to determine whether to report data, but our best guess is that it did so when refugees sought asylum from a UN member (e.g., Rhodesians into Mozambique and Zambia in the late 1970s).¹⁵

As noted above, Newland (1993) argues that ethnic divisions frequently underlie the violent conflicts that generate refugee flows. Sambanis (2001) argues that ethnic civil wars can be distinguished from nonethnic civil wars, and his analyses provide support for his argument. He produces a list of both types of civil wars, and we use his data to determine whether ethnic civil wars have a different substantive impact on forced migration than nonethnic civil wars, as Newland's work implies. Like Fearon and Laitin (2003), we are dubious about the impact of the ethnic composition of countries on civil conflict, but we use Sambanis's data to examine the possibility nonetheless.

To measure war on territory, we begin with the Correlates of War Interstate War list (Sarkees 2000). These are interstate militarized conflicts in which at least 1,000 battle deaths occurred. We then used several standard references, such as the *Dictionary of Wars* (Kohn 1999), to determine whether at least one battle took place on the territory of each participating country. Those countries in which at least one battle took place were assigned a score of 1 for that year, and all other countries were assigned a score of 0.

We also need a measure of political freedom and the rule of law. Democracy is associated with these concepts, and we chose to measure it using the Polity project's measure of institutional democracy (Jagers and Gurr 1995).¹⁶ We adopted the widely used 21-point scale created by taking the difference between the democracy and autocracy scores, each of which is a composite measure of institutional characteristics of the government. In several transition regimes, the political institutions were in flux and thus could not be reliably coded.¹⁷ Rather than code these cases as missing and thus drop them from the data set, we assigned a value of 0 to them, and we coded a dummy variable to which we assigned the value 1 when the democracy and autocracy measures were missing due to a transition and 0 when the democracy and autocracy measures had nonmissing values.

The most direct measure of income opportunity is probably wages, although wages emphasize wage labor over other forms of livelihood. Nevertheless, cross-national

dimensions of dissident activity: the number of acts of dissent, the presence of violence in those acts, the variety across four types of dissident events, and whether dissident behavior exceeds "normal" levels of activity in that society. However, Davenport, Moore, and Poe find that only the variety measure has an impact on net migration.

15. We had assumed that the UNHCR would record only data for UN members, but this is not the case.

16. For a recent debate on the strengths and weaknesses of the various measures of democracy, see the exchange in the February 2002 issue of *Comparative Political Studies*.

17. See Gurr, Jagers, and Moore (1989, 7-8) for a discussion.

data on wages are not available for the spatial-temporal domain of our study. As such, we employ the proxy measure of gross national product (GNP) per capita.¹⁸ To develop observations on as many countries as possible, we drew on two sources to develop our GNP measure: Banks's (n.d.) cross-national time series data archive and the World Bank's (2000) world development indicators data. When the World Bank data had an observation, we used it. We replaced missing observations with Banks's data when such observations were available.¹⁹

Finally, we include the lagged value of each origin country's forced migrant stock. We do so to measure the extent to which culture, family, and friends have been disrupted. We use stock as opposed to flow because we want to measure the total population that has left as opposed to a subset of the population that has left recently. We expect that the greater the total number of migrants forced from the homes around one's own, the greater the loss of cultural ties. Stock, rather than flow, more appropriately captures the argument. Table 1 reports descriptive statistics for our variables.

THE MODEL AND STATISTICAL ISSUES

To test the hypotheses implied by our model, we specify a statistical model and estimate its parameters using data from an unbalanced panel composed of more than 175 countries for the time period from 1952 to 1995.

Our dependent variable, forced migrant flow, is a count of forced migrants, and visual inspection of histograms confirms that the data are not normally distributed.²⁰ Scholars frequently turn to either the Poisson or the negative binomial distribution when analyzing count data that are not normally distributed. A Poisson regression model is appropriate if one assumes that the probability of any given event is independent of any other event in a given unit of time (King 1989b; Long 1997, chap. 8). In our case, we would need to assume that the probability of any given person in a given year in a given country choosing to abandon her or his home was independent of any other person in that same country during that year choosing to abandon his or her home. Our theory rules out that assumption because we have explicitly argued that people will respond to a single information set (that is, the behavior of government, the dissidents, etc., in the country). Thus, we are arguing that peoples' decisions are linked via a common set of information; they are not independent. An appropriate distribution in such a circumstance is the negative binomial distribution, and the negative binomial regression model includes a parameter, α , that enables one to estimate the extent to which the events influence one another within each observation.²¹ Our theory

18. For an analysis of the relationship between gross national product (GNP) and wages, see the U.S. government's Import Administration's report on the topic at <http://ia.ita.doc.gov/wages/98wages/98wages.htm>.

19. The correlation between the World Bank (2000) data and Banks's (n.d.) data is .918. Furthermore, the variables produced essentially the same parameter estimates in a number of regressions we ran to assess the compatibility of the series.

20. This is the case regardless of whether one examines histograms for each country individually or for all countries. Interested readers can create histograms using the replication data available at <http://www.yale.edu/unsy/jcr/jcrdata.htm>.

21. See King (1989b, 764-69) for a detailed explanation of why the negative binomial model is useful for this sort of argument.

TABLE 1
Descriptive Statistics

	<i>Mean</i>	<i>Standard Deviation</i>	<i>Minimum Value</i>	<i>Maximum Value</i>
Genocide	0.156	0.954	0	10
Dissident violence	0.790	2.32	0	55
Civil war	0.076	0.265	0	1
International war on territory	0.011	0.104	0	1
Government terror (PTS)	2.58	1.12	1	5
Democracy	-0.359	7.55	-10	10
Transition	0.038	0.192	0	1
GNP	1.18×10^{11}	5.37×10^{11}	4.04×10^7	8.54×10^{12}
Forced migration	91,421	466,096	0	8,337,550

NOTE: The descriptive statistics differ slightly across model 1 and model 2 estimation samples. PTS = political terror scale; GNP = gross national product.

implies that α will be positively signed and statistically significant. As such, we assume that the forced migration data were produced by a negative binomial-like process.

Although a large number of countries produced forced migrant flows during our period of study, in any given year only about 10% of the countries produced a nonzero flow of forced migrants. This is consistent with the point raised in footnote 3: forced migration is a rare event. In addition, a sample of all countries for which data are available will be composed of two distinct populations: those that effectively have zero probability of producing forced migrants in a given year and those with a nonzero probability of producing forced migrants in a given year. As such, we need a statistical model that is capable of distinguishing between two populations, given covariates. A class of models known as zero-inflated models is capable of doing split population analyses, and we use the zero-inflated negative binomial model to estimate our parameters.²²

Another statistical issue arises because we have data collected over time and across countries. Pooled cross-sectional time series (PCSTS) data tend to suffer from both heteroskedasticity and autocorrelation, each of which threatens proper inference (Beck and Katz 1995). To address heteroskedastic errors, we reestimated the model using both robust standard errors clustered on country and a fixed effects approach. As

22. The hurdle Poisson model (King 1989a) is another option, but we selected the zero-inflated negative binomial (ZINB) model because of the assumption it makes about the possible count values in the population with a nonzero probability of producing forced migrants. More specifically, the ZINB model assumes that some of the cases at risk to producing a positive count may produce a count of zero, whereas the hurdle Poisson model assumes a truncated count such that all cases at risk to producing a count will produce a nonzero count (Zorn 1998). We expect that the covariates that are established by our hypotheses will both distinguish those countries at risk to producing forced migrants from those that are not and affect the number of forced migrants produced. However, we do not anticipate that all countries at risk to producing forced migrants in a given year (e.g., a country with a civil war and low income per capita) will produce a nonzero forced migrant flow. As such, the ZINB model is a better choice than the hurdle Poisson for our study.

explained below, the fixed effects model did not produce major changes in the parameter estimates, and the robust standard errors changed only a few coefficients' statistical significance levels.²³

A final statistical concern involves missing data. Our complete data matrix contains almost 7,000 observations, but many country-years are missing data on one or more variables. As such, we often lose a substantial number of cases due to list-wise deletion. List-wise deletion, or complete case analysis, produces biased parameter estimates (Raghunathan and Paulin 1998), especially when the cases are not missing at random. We know that Organization for Economic Cooperation and Development (OECD) countries are considerably less likely to have missing data than are poor countries. Furthermore, countries that experience violence are more likely to be missing data. Thus, list-wise deletion introduces sample selection bias. We check the robustness of our complete case results by using three simple techniques to estimate the missing values. We then replicate the models using each data set and compare the results across them. We report those analyses in an appendix available on the Internet.²⁴

FINDINGS AND IMPLICATIONS

We report the results for our two zero-inflated negative binomial (ZINB) models in Table 2.²⁵ Model 1 includes all the relevant variables outlined above with the exception of the political terror scale (PTS). Using the PTS limits our spatial-temporal domain. We thus exclude the PTS and estimate model 1 over the 1952 to 1995 time period. Model 2 includes the PTS and analyzes data from 1976 to 1995. We estimate a few other models as well and report them in the online appendix. However, the inferences one draws from the additional models are not dramatically different from those drawn from the models we report here. That is, the results across the various model specifications (and the number of cases analyzed) are stable.

We report both the coefficient estimates and the incidence rate ratios (IRRs).²⁶ IRRs represent the change in forced migrants given a one-unit increase in the explanatory variable, holding all other variables constant.²⁷ In the case of a dummy variable, such

23. Estimation difficulties prevented us from reporting the robust standard errors: Stata produced estimates and robust standard errors but did not produce a χ^2 statistic. There are only a few minor differences between the robust standard errors. In model 1, the level of statistical significance for genocide moved from .05 to .10, for war on territory and democracy it moved from .05 to .01, and for GNP it moved from .01 to .05 in the count equation. In model 2, the level of statistical significance for genocide moved from .05 to .01 and became insignificant for war on territory and democracy. All results are replicable using the do-files and replication data set in the online appendix.

24. At the date of publication the *JCR* Web site is <http://www.yale.edu/unsy/jcr/jcrdata.htm>. The author's Web sites also contain links to the appendix.

25. The ZINB model has two equations: one that uses the absence of an event as the dependent variable and another that uses a count of the number of events as the dependent variable. It is important to note that the signs of the variables will be opposite one another across the two equations, since the dependent variables are coded in opposite directions.

26. However, we present the incidence rate ratios (IRRs) for the negative binomial equations only as an IRR makes little sense in the context of a logit equation.

27. The IRR is the exponentiated coefficient (i.e., given the estimate, b , the $IRR = e^b$).

TABLE 2
Zero-Inflated Negative Binomial Regression for Forced Migrant Flows

	Model 1 (1952-1995)						Model 2 (1976-1995)					
	NBRM: Forced Migration (R + IDPs)			Inflate Equation: Forced Migration (I, 0)			NBRM: Forced Migration (R + IDPs)			Inflate Equation: Forced Migration (I, 0)		
	Coefficient	IRR	Z	Coefficient	Z	Z	Coefficient	IRR	Z	Coefficient	Z	Z
Genocide	0.07	1.07	1.76**	-0.18	-5.16**	0.09	1.09	2.09**	-0.14	2.09**	-2.91**	
Dissident violence	0.11	1.11	4.98**	-0.03	-1.63*	0.14	1.16	4.46**	-0.04	4.46**	-1.39*	
Civil war	1.63	5.14	8.96**	-1.94	-14.36**	1.23	3.41	5.88**	-1.11	5.88**	-6.26**	
International war on territory	1.04	2.82	3.01**	-1.23	-4.22**	-0.15	0.86	-0.37	-0.68	-0.37	-1.66**	
Government terror (PTS)	—	—	—	—	—	0.47	1.59	6.18**	-0.65	6.18**	-9.26**	
Democracy	-0.03	0.97	-2.44**	0.03	4.38**	-0.01	0.99	-0.80	-0.01	-0.80	-0.78	
Transition	-0.03	0.97	-0.12	-0.00	-0.00	0.40	1.49	1.19	0.49	1.19	1.75**	
GNP	-6.65 × 10 ⁻¹³	1.00	-1.85**	4.30 × 10 ⁻¹³	1.60*	-6.77 × 10 ⁻¹³	1.00	-2.30**	6.30 × 10 ⁻¹³	-2.30**	1.56*	
Forced migrants _{t-1}	5.32 × 10 ⁻⁷	1.00	3.23**	-2.96 × 10 ⁻⁷	-2.82**	5.55 × 10 ⁻⁷	1.00	3.16**	1.09 × 10 ⁻⁷	3.16**	0.93	
Constant	10.22	—	85.65**	2.70	2.70**	8.61	—	35.63**	3.62	35.63**	17.65**	
α	—	—	—	3.03	16.37**	—	—	—	2.53	—	14.88**	
χ^2	—	173.74**	—	-7,402.28	—	—	207**	—	—	—	—	
Log-likelihood	—	—	—	—	—	—	—	—	—	—	-5,417.79	
N	—	—	—	—	—	—	—	—	—	—	—	
Total	5,196	—	—	—	—	2,279	—	—	—	—	—	
Zero	4,686	—	—	—	—	1,889	—	—	—	—	—	
Nonzero	510	—	—	—	—	390	—	—	—	—	—	

NOTE: NBRM = negative binomial regression model; R + IDPs = refugees + internally displaced persons; IRR = incidence rate ratio; PTS = political terror scale; GNP = gross national product.

*Significant at .10. **Significant at .05 (one-tailed tests).

as civil war, the IRRs are the relative rates of forced migrants for countries in a state of civil war relative to countries not experiencing civil war.

We further divide the results in Table 2 into those obtained from the negative binomial (i.e., count) equation and those obtained from the logit (i.e., binary or inflated) equation. We expect the signs on the variables across columns to be the opposite of one another.

The first thing to observe in Table 2 is the parameter estimates for α , the overdispersion parameter. Recall that we argued above that one person's decision to abandon her or his home was not independent of other peoples' decisions to abandon their homes. Given that argument, α should be positive and statistically significant. In both models it is, and this is one indication that the negative binomial model is appropriate. We now turn our attention to the more directly interpretable variables.

We begin with a discussion of how state and dissident behavior influence forced migrant flows. In each model, the severity of state-sponsored genocide/politicide, the number of violent dissident events, and the presence of civil war have a statistically significant positive impact on forced migrant flows. In addition, the variable indicating human rights violations (PTS) in model 2 also has a positive statistically significant effect on forced migrant flows. These findings are consistent with our argument that people will monitor state violence, dissident violence, and the interaction of the two when assessing the level of threat to their life, liberty, and person.

Next, we wish to determine the substantive impact of each of the violence variables on forced migrant flows. To address this issue, we report the incidence rate ratios (in the tables) and also determine the expected number of forced migrants given changes in the values of the independent variables.²⁸ The IRRs indicate the percentage change in the expected count of forced migrants given a one-unit change in the independent variable. An IRR of 1.0 is equal to no change; values less than 1.0 indicate a reduction in the expected count, holding all other variables constant.

We can also examine changes in the expected number of forced migrants given a change from the minimum value to the maximum value and by introducing one- and two-standard-deviation increases in each of the independent variables. We use bar charts to plot the expected change in the number of forced migrants in Figure 1. The left-hand side of Figure 1 displays the change in the expected count given a shift from the minimum value of the independent variable to the maximum value of the independent variable for both models 1 and 2. The right-hand side of Figure 1 displays the expected change in the number of forced migrants moving from the mean of the independent variable to one standard deviation above the mean and moving from one standard deviation above the mean to two standard deviations above the mean for both models 1 and 2. When calculating the expected counts, we hold each of the non-dichotomous independent variables at their means and set all dummy variables equal to zero. We do not plot the standard deviation changes for the dummy variables. Note that the scales are different for Figure 1. In addition, we truncated the vertical axis in model 1 and model 2 for the minimum to maximum changes and in model 2 for the standard deviation increases. That the violent behavior of state and dissident actors has

28. We used the `prchange` command in the `SPOST` suite (see Long and Freese 2001).

substantively large effects on the expected number of forced migrants is the major finding from Figure 1.

We begin with a discussion of dissident violence. In both models, the value of the dissident violence event count was so large given a change from its minimum to maximum value (left-hand side of Figure 1) that if we plotted it completely, we could not observe the values of some of the other variables. The effect of a change from the minimum to the maximum value is clearly largest for dissident violence.

When we consider a one-unit increase in, or a one-standard-deviation increase above the mean of, dissident violence, the impact on forced migrant flows is still substantial, but it no longer dwarfs the other variables. In fact, a one-standard-deviation increase in both democracy and size of the economy produce about the same amount of change as dissident violence. The IRRs of 1.11 (1.16 in model 2) suggest that a one-unit increase in dissident violence produces an 11% (16%) increase in the number of forced migrants, which is substantial but not remarkable, relative to the other IRRs. Taken together, these results suggests that the impact of dissident violence is in the right-hand tail of the distribution. It turns out that although 30% of the country-years in our sample have a nonzero count of violent dissident events, 29% of the cases record between 1 and 10 events. The remaining 1% of the cases produced between 11 and 55 events. When we change the number of events from 0 to 11 (the 75th percentile), the impact on the expected number of forced migrants remains modest. But as we move above 11, the impact increases dramatically. This suggests that large numbers of armed attacks and riots might serve as a useful predictor of large-scale forced migration events.

The result for dissident violence stands out from those reported to date in the literature. More specifically, Schmeidl (1997) and others use crude measures of violence and thus do not assess the impact of dissident violence on forced migration. Davenport, Moore, and Poe (2003) include measures of dissident violence in their study but report that civil war and genocide/politicide have substantively large impacts on net migration whereas dissident violence has a relatively small impact. We also find that civil war and genocide/politicide have large substantive impacts (discussed below), but we find that large dissident violence scores have an even larger effect.

As noted, that result differs when one considers less dramatic shifts in the independent variables. For example, when one introduces one- and two-standard-deviation increases in the independent variables, the expected change in forced migrants is about the same in model 1 across dissident violence, democracy, and GNP but is highest for the PTS in model 2. Combining this finding with the min-max changes tells us that low and moderate levels of dissident violence do not produce as large increases in forced migrants as do low and moderate levels of human rights violations. However, given our data, high levels of dissident violence produce larger increases in forced migrants than do high levels of human rights abuses.²⁹

29. The relative size of dissident violence in comparison to both civil war and genocide/politicide may, in part, be a function of measurement: dissident violence has the largest range among the behavior variables. If we had an event count measure of state coercion, rather than the ordinal measures of death magnitude from genocide/politicide and human rights violations, then perhaps state (sponsored) violence variables would have larger effects than they do. Similarly, if we had an annual event count of the number killed during civil war, that variable might have a larger effect.

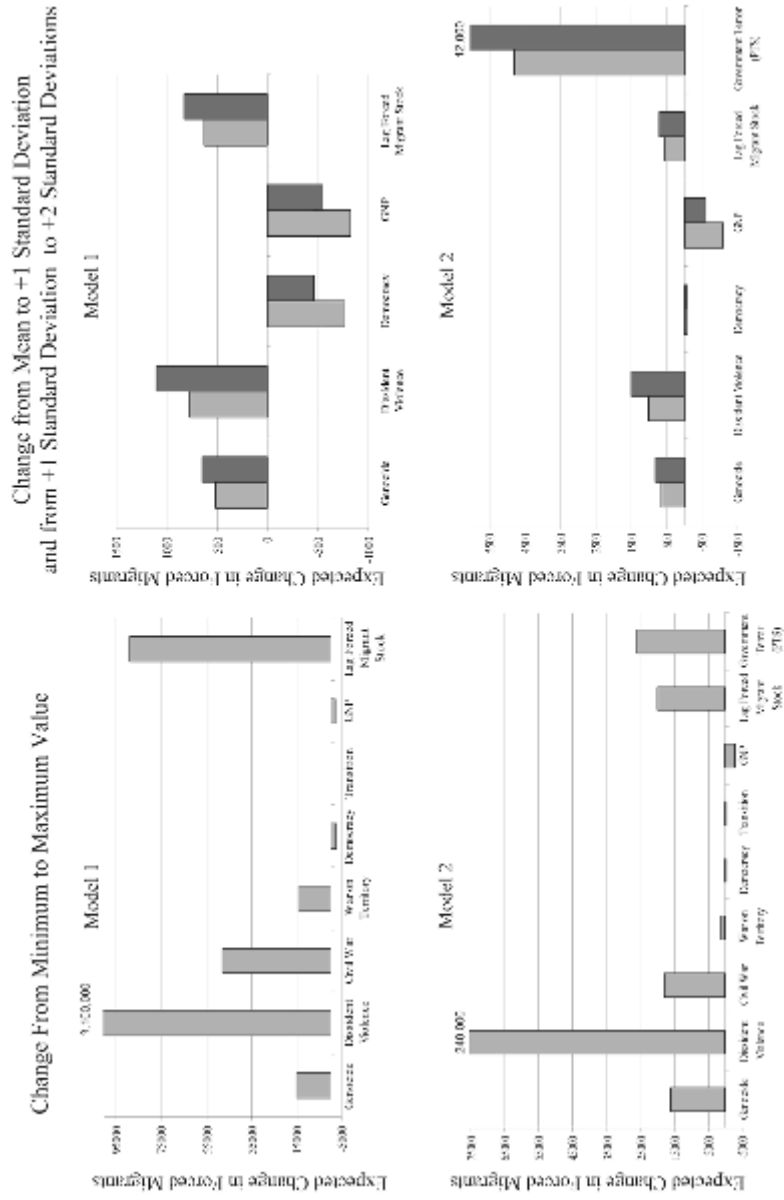


Figure 1: Expected Change in Number of Forced Migrants Given Specific Changes in the Independent Variables
 NOTE: In models on the right, lighter shading shows change from mean to +1 SD above the mean; darker shading indicates change from +1 SD above the mean to +2 SDs above the mean.

Model 2 shows that government terror (PTS) has a large impact on forced migrant flows. The PTS produces an increase of 26,000 forced migrants moving from its minimum (1) to its maximum value (5), and a one-unit increase leads to an expected 59% increase in forced migrants. If we increase the PTS variable one standard deviation above its mean, we expect an additional 4,800 forced migrants, holding all other variables constant. Finally, as we move from one standard deviation above the mean to two standard deviations above the mean, we expect an increase of 12,000 forced migrants. This finding is consistent with Gibney, Apodaca, and McCann (1996) and Apodaca (1998), who report that human rights violations impact forced migration.

Next, we shift attention toward the genocide variable. For model 1 and model 2, as we increase a country's level of genocidal activity from 0 to 10, we expect average countries not experiencing civil war, international war, or a polity transition to produce 15,000 and 16,000 additional forced migrants, respectively. The coefficients on genocide across models 1 and 2 are very similar and make similar predictions with respect to the change in forced migrants, given changes in the genocide variable. The similarity is evident across models in both the left-hand column and the right-hand column of Figure 1, and the impact of a one-standard-deviation increase is similar to that of government terror. Finally, the IRRs in Table 2 indicate that a one-unit increase on the genocide scale leads to a 7% increase in forced migrants in model 1 and a 9% increase in model 2.

Given the findings reported in Sambanis (2001), we probed the argument that ethnic civil wars will create more forced migrants than nonethnic civil wars. When we replaced the Correlates of War civil war measure with Sambanis's measures, the estimates were not meaningfully different. In model 1, the coefficient estimates for ethnic civil war and nonethnic civil war are 1.36 and 1.15, respectively, and a hypothesis test (*F* test) confirms that the difference between the coefficients is equal to 0.³⁰ When we calculated the change in forced migrants for countries experiencing an ethnic civil war and for those experiencing a nonethnic civil war, the expected counts were 24,000 and 23,000, respectively.³¹ In other words, the ethnic civil war variable and the nonethnic civil war variable have nearly the same impact (or coefficient estimate). This was true in both the logit (inflate) equations as well and shows that the civil wars that Sambanis codes as having ethnic origins not only produce the same numbers of forced migrants as civil wars that do not have ethnic origins but also have equal probability of not observing a forced migrant event.

Foreign troops fighting on a country's territory is another source of violence that we hypothesized would influence people's decisions to abandon their homes. The results in Table 2 and Figure 1 show that the variable is only statistically significant in model 1. The results for model 1 predict an increase of 14,000 forced migrants when a country hosts an international war. However, the impact is small (i.e., 1,300) and not statistically significant in Model 2.

30. Model 2 also produces estimates that do not have a statistically or substantively significant difference across ethnic and nonethnic civil wars.

31. When simply including civil war in the model, the expected number of forced migrants produced is 48,000, holding all other dummies at zero and all other variables at their means.

Before turning to a discussion of the other variables, it is useful to discuss the impact of the violence measures on the probability that a country produced zero forced migrants. We report those coefficients in the Inflation columns in Table 2. By and large, the results are similar to those found in the negative binomial regressions. First, when the coefficients have opposite signs across the NBRM (negative binomial regression model) and Inflation columns, the variable has a consistent effect, and a survey of Table 2 shows that this is the predominant pattern. Transition in models 1 and 2 along with democracy and war on territory in model 2 are the only exceptions. Second, the variables that had statistically significant parameter estimates in the negative binomial regressions generally have statistically significant parameter estimates in the binary equation (we discuss the exception for lagged forced migration stock below). Having discussed the variables that we suggest will impact one's estimate of p , we turn our attention to the other variables that we examined.

First, consider the past year's stock of forced migrants. We argue that the more people who have left in the past, the more costly it is to stay. The results in Table 2 are consistent with the hypothesis: in each of the negative binomial regressions, the FM stock variable is statistically significant and positively signed. In addition, as noted above, the impact is substantial: Figure 1 shows that in three of the four models, lagged forced migration stock has the second largest impact in model 1 and third largest impact in model 2. For example, in model 1, when we change the lagged stock from its minimum to its maximum value, we observe an increase of 89,000 forced migrants. When we change it from its mean value to one standard deviation above the mean, we observe an increase of 629 forced migrants. When we shift it from one standard deviation above the mean to two standard deviations above the mean, we observe an increase of 830 forced migrants. In model 2, the effect is considerably diminished when moving from the minimum to the maximum value: 20,000. This is interesting because model 2 includes both measures of state coercion. However, it is also based on the smallest sample with respect to both countries and years included. In any event, the balance of evidence suggests that the stock of forced migrants from previous years has an impact on forced migrant flows.

The other variables we examined are the level of democracy and our income proxy, GNP per capita. We expect both variables to have a negative association with the number of forced migrants and a positive association with the probability of observing zero forced migrants. Table 2 indicates that democracy has the expected signed parameter estimates in both equations in model 1. Yet both coefficients are insignificant in model 2. Figure 1 shows that the size of the effect on the number of forced migrants is relatively small: a change from -10 to 10 on the democracy scale is expected to decrease forced migrants by 2,600 in model 1 and by 210 in model 2, holding all other variables constant. The impact is minuscule compared to the expected change in forced migrants, given a change from the minimum to the maximum value in the other independent variables, and the IRRs indicate that a one-unit increase in democracy produces a 3% (1% in model 2) decrease in the number of forced migrants. However, the left-hand column of Figure 1 indicates that in model 1, democracy has a similar impact to dissident violence and lagged forced migrant stock on forced migration when we consider one standard deviation increases. However, when we control for human

rights violations (model 2), the impact of democracy again drops substantially. Other studies have reported that democracy does not have a statistically significant impact on forced migration (e.g., Davenport, Moore, and Poe 2003). Although we find such an impact, the substantive effect is trivial when one controls for human rights violations.

Because the polity project does not code democracy values during transition years, we included the dummy variable *transition* in the equations. Since political turmoil is associated with transition regimes, this variable might be thought of as an indirect measure of a cost to staying. In any case, we expect transition to be positively associated with the number of forced migrants and negatively associated with the probability of observing zero forced migrants. Table 2 reveals that transition only produced a statistically significant coefficient in the inflate equation for model 2. The balance of the findings suggests that transition has no effect on forced migration.

We also included our income proxy, GNP, in both models. We expected it to have a negative impact on the number of forced migrants and a positive effect on the probability of observing zero migrants. Consistent with those expectations and contrary to other published findings, it produced a statistically significant, properly signed parameter estimate in both of the negative binomial equations and both of the logit equations. Figure 1 reveals that, although GNP is associated with the number of forced migrants, it does not have a large impact: a shift from the poorest country in the world to the wealthiest reduces the expected number of forced migrants by 2,200 (2,800 in model 2). Those numbers are very small relative to the other variables. However, the right-hand column indicates that a one-standard-deviation increase in GNP has a similar effect on the expected number of forced migrants as a one-standard-deviation increase in dissident violence, democracy, and lagged forced migrant stock. In addition, the substantive impact on the probability of observing zero forced migrants is consistent with expectations. Thus, higher GNP reduces both the probability that a country will produce refugees and, given that it produces some, the number it will produce.

As mentioned, we estimated a variety of different models to test the sensitivity of the results. The details are discussed in the online appendix, but we briefly note the general themes here. First, we include population in the model and find that it has no statistically significant effect.³² Furthermore, it does not effect the other coefficient estimates much. Second, we estimate the models using a fixed effects approach.³³ In doing so, we find that the parameter estimates are rather stable and the significance levels do not change much. Finally, we interpolate, extrapolate, and impute missing data and estimate the models using the resulting data matrices. Although there are minor differences across the data sets, by and large, the findings reported in Table 2 are robust and do not appear to be influenced by sample selection bias.

32. To measure population, we use the population measure in the World Bank's (2000) world development indicator data.

33. We coded dummy variables for each country (except one) and included those dummies in the ZINB models. Several of the models would not converge, and in those cases we examined the estimates produced by 50 iterations. Please see the online appendix for a full discussion.

CONCLUSION

This study set out to explore the covariates of forced migration. We argue that individual responses to aggregate level information influence forced migration flows at the country level. Parts of the argument build on hypotheses put forward by others, and although many of our findings are consistent with those reported in the handful of large-*N* statistical analyses on the topic, others stand out. To summarize, the violent behavior of governments and dissidents (and their interaction) are the primary determinants of forced migration flows, and they suggest that high levels of dissident violence might be the strongest indicator. Institutional democracy and income do influence the size of forced migration flows, but their impact is relatively small: the push factor of violence drives the process.

Setting up the question as we have—on the decision to leave—leads to a natural extension to two additional questions. First, will a majority of those who have left relocate within their borders (as IDPs) or across their borders (as refugees)? Second, among those who cross their borders, to what country will the largest (and second largest, etc.) group of them flee? These are questions that have not yet been asked in the literature. We explore them in two additional studies that, with the present study, form the core of our forced migration project (Moore and Shellman 2004a, 2004b).

On a different tack, these results—like those of all large-*N* statistical analyses—are average effects: they tell us precious little about the specific impact of covariates in any given forced migration event. This observation suggests another potentially fruitful direction for future research: analysis of time series case studies. A few such studies exist (e.g., Stanley 1987; Morrison 1993), but it will prove useful to explore the extent to which our findings are corroborated or challenged by time series case studies.

Another important direction for future research concerns out-of-sample forecasting and the assessment of statistical models like this one to serve an early warning capacity. Political scientists tend to focus on specific parameter estimates because they are of theoretical interest as hypothesis tests. However, the overall fit of the model is important in that it might prove useful for developing contingency plans. We plan future work to explore the utility of this type of cross-national and cross-temporal model and country-specific time series models of forced migration for contingency planning. Out-of-sample forecasting assessment will be a key element of that research.

Finally, some existing research identifies interdependence between government coercion and dissent (e.g., Francisco 1995; Moore 1995; Gurr and Moore 1997) and suggests that the behavior of the government and dissidents may well be influenced by forced migrant flows. Developing an argument to support such a suggestion is beyond the scope of this study, but pursuing it strikes us as an eminently reasonable direction for future study.

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