APPENDIX

The following appendix sections are intended to further inform interested readers in our research efforts and to help facilitate replication. We begin with a discussion of the estimation techniques used in the primary models presented in the main text. We then move to an expanded discussion of various alternative models and estimation techniques utilized to check the robustness of our results. Overall, these different analyses left us considerably confident in the findings of our article.

Estimation Techniques

To estimate our models using Prais-Winsten estimation with panel-corrected standard errors, we employed the Stata 9 command XTPCSE. However, because of limited temporal variance after breaking up our sample into pre- and post-September 11 periods we were forced to utilize an alternative estimation technique. In order to estimate pre- and post-2001 models for immigration judges (results presented in immigration judge models 2a and 2b in Table 2 and discussed in the main text of this article) we were forced to use the Prais-Winsten estimation technique with only robust standard errors for the immigration judge dependent variable utilizing the Stata 9 PRAIS command (with the ROBUST command used to calculate robust standard errors). To check the robustness of the results garnered from this alternative technique we also ran these two models utilizing generalized least squares with the Stata 9 command XTGLS. The only difference in results between these two estimation techniques was a decrease in the statistical significance from 0.01 confidence to 0.05 confidence for some parameter estimates. The substantive impact of the coefficients remained unchanged. We also ran the pre- and post-2001 asylum officer models with the Prais-Winsten estimation technique using robust standard errors. We present these results alongside those of the results estimating the pre- and post-2001 immigration judge models in Table 3 below (these same judge models are also presented in Table 2 in the main text of this article). We now present an extended discussion of additional checks that support the robustness of our results.

Additional Robustness Checks

As discussed in the main text of this article our dependent variable is the percentage of successful asylum applicants as determined by either an asylum

officer or by an immigration judge. In the model we presented in the main text we exclude cases that, first, had less than five out of six years of acceptance rates reported for both the officer and judge. Second we excluded any observation from the selected panels that had less than ten decisions in a given year. However, in addition to this sample we ran several other models with different restrictions on the sample. First, we ran models with only a minimum of five cases as the threshold for inclusion into our final model, while not varying the panel size requirement. This variation resulted in little substantive change in our parameter estimates and standard errors. We also ran a model on a more restricted sample, with panels that included observations of the dependent variables for all six years. This reduced our total panel number from 96 to 84 while reducing our total number of observations from 527 to 485. Again, the results presented in the main text of this article were supported, in spite of the loss of cases. In fact, the more restrictive results are statistically and substantively stronger in this model than in the one presented in the text. We chose to present the results we did because we wished to test our model on as many theoretically interesting countries as possible, while excluding other typical destination countries for asylum seekers in the Western European countries (as discussed by Neumayer 2005a, 2005b) in addition to Australia, Canada, Japan, and New Zealand. Our final model was tested on a sample that includes all the countries listed in Table 4 below.

Due to the restrictive panel requirements we used to build our main model we unfortunately had to exclude several countries of theoretical interest. Thus, to ensure that our results are robust to as many cases as possible we also ran some robustness tests on a sample which decreased the number of observations necessary for a panel to be included in our sample – from five to four, to three, and then to two, while utilizing GLS estimation (with normalized standard errors) and Prais-Winsten estimation (with robust standard errors) techniques. The advantage of these tests is that they include more cases than those presented in the article, but their comparative weakness is that the techniques for dealing with autocorrelation and heteroskedasticity are somewhat more suspect. The variation in panel size prevented us from using the Prais-Winsten estimation technique with panel-corrected standard errors for this estimation; however, the results maintain their robustness. Given the unbalanced nature of these samples we opted for the more restrictive sample discussed at length in our article so that we could be confident in our comparisons of the pre- and post-2001 asylum system. Our results are robust throughout these various specifications and despite the exclusion of some cases from our final model because of incomplete data issues. As a result, we

TABLE 4
INCLUDED COUNTRIES

1.	Afghanistan	33.	Eritrea	65.	Moldova
2.	Albania	34.	Estonia	66.	Mongolia
3.	Algeria	35.	Ethiopia	67.	Morocco
4.	Angola	36.	Fiji	68.	Nepal
5.	Argentina	37.	Gambia, The	69.	Nicaragua
6.	Armenia	38.	Georgia	70.	Niger
7.	Azerbaijan	39.	Ghana	71.	Nigeria
8.	Bangladesh	40.	Guatemala	72.	Pakistan
9.	Belarus	41.	Guinea	73.	Peru
10.	Bhutan	42.	Guinea-Bissau	74.	Philippines
11.	Bolivia	43.	Haiti	75.	Romania
12.	Bosnia & Herzegovina	44.	Honduras	76.	Russia
13.	Brazil	45.	India	77.	Rwanda
14.	Bulgaria	46.	Indonesia	78.	Senegal
15.	Burkina Faso	47.	Iran	79.	Sierra Leone
16.	Burma (Myanmar)	48.	Iraq	80.	Somalia
17.	Burundi	49.	Israel	81.	South Africa
18.	Cambodia	50.	Jordan	82.	Sri Lanka
19.	Cameroon	51.	Kazakhstan	83.	Sudan
20.	Central African Republic	52.	Kenya	84.	Syria
21.	Chad	53.	Kuwait	85.	Tajikistan
22.	Chile	54.	Kyrgyzstan	86.	Tanzania
23.	China (P.R.C.)	55.	Laos	87.	Togo
24.	Colombia	56.	Latvia	88.	Turkey
25.	Congo (Brazzaville)	57.	Lebanon	89.	Turkmenistan
26.	Congo (Kinshasa)	58.	Liberia	90.	Uganda
27.	Cote d'Ivoire	59.	Lithuania	91.	Ukraine
28.	Croatia	60.	Macedonia	92.	Uzbekistan
29.	Cuba	61.	Malaysia	93.	Venezuela
30.	Ecuador	62.	Mali	94.	Vietnam
31.	Egypt	63.	Mauritania	95.	Yemen
32.	El Salvador	64.	Mexico	96.	Zimbabwe

are confident in the robustness of our findings regardless of what criteria we use to include countries in our sample.

Next we discuss issues of multicollinearity. As we began our multivariate analyses we did a series of bivariate correlations, which indicated the results our analyses will probably not be affected greatly by the problems associated with multicollinearity. Table 5 below presents the bivariate correlation coefficients generated from the observations that we utilized for our final model. In addition to tests for bivariate collinearity we also utilized the Klein procedure to test for the presence of multicollinearity in the four main models of our analysis (Klein 1962; Klein and Nakamura 1962), which we presented in Table 1 and discussed in the main text of this article. To perform the Klein procedure we ran a series of ordinary least squares regression models with each of the explanatory variables taking turns as the dependent variable. We recorded each of the resulting

TABLE 5
CORRELATION COEFFICIENTS

		1	2	3	4	5	6	7	8	9	10	11	12
1	Asylum Judge Percentage Approved	1.00											
2	Asylum Officer Percentage Approved	0.46	1.00										
3	Violations of Physical Integrity $(PTS)_{t-1}$	0.20	0.33	1.00									
4	Democracy $(Polity2)_{t-1}$	–0.34	–0.47	–0.26	1.00								
5	US $MilitaryAid_{t-1}$†	–0.05	–0.32	–0.24	0.46	1.00							
6	US Trade $(\ln \$1000US\ 2004)_{t-1}$	–0.29	–0.27	0.10	0.36	0.31	1.00						
7	Sanctions†	0.11	0.31	0.23	–0.27	–0.45	–0.10	1.00					
8	English Language†	0.04	–0.07	0.13	–0.05	–0.06	–0.12	–0.08	1.00				
9	Arabic Language†	0.02	0.14	0.07	–0.32	0.01	0.00	0.05	0.04	1.00			
10	Spanish Language†	–0.40	–0.26	–0.09	0.32	0.18	0.36	–0.04	–0.19	–0.16	1.00		
11	Top 10 Undocumented Immigration Countries†	–0.40	–0.38	–0.06	0.25	0.17	0.31	–0.07	–0.03	–0.12	0.48	1.00	
12	Post 2001†	–0.03	–0.27	–0.02	0.03	0.04	0.02	–0.16	–0.01	–0.01	0.00	–0.02	1.00

Notes: Number of observations = 527.
† = discrete change of dummy variable from 0 to 1.
ln = natural log.
PTS = State Department Political Terror Scale (1 = best human rights practices and 5 = worst).
Polity2 = 21-point institutional democracy scale from Autocracy (–10) to Democracy (10).

R-squared statistics, making sure that none of them reached a value greater than 0.50. The range of these values for all four models lies between 0.0141 and 0.4926, with most values at or around 0.20.[1] The results of our Klein test assures us of a low presence of multicollinearity in the models that we present in the text of our article and that "singularities, indeterminacy and nonfinite values" should not affect any of our individual parameter estimates (Klein and Nakamura, 1962:274).

Due to our interest in the effect of political events after the September 11 attacks on asylum acceptance rates, we also ran several of our models with interactive effects introduced individually to our model. Most of the estimations with interaction effects produced results that indicate changes in the substantive impact of coefficients in the direction we expected after the events of September 11, but many of them were statistically insignificant. We believe that this is due to multicollinearity introduced when the interactive terms were added to the model. We again utilized the Klein procedure to assess the level of multicollinearity of these interaction terms that we added to the model, which resulted in R-squared statistics between 0.80 and nearly 0.99. In the end we chose to downplay these results because of the concerns that these extreme levels of multicollinearity entail. Instead, we rely upon the comparisons of the models estimating the pre- and post-2001 time period presented in the text, which are free from these issues and consequently give us much more confidence in the validity of these results.

Next, in addition to coding language and cultural heritage, we ran several models with variables coded to capture the percentage of a country's Muslim population in order to observe if asylum seekers from these countries receive asylum at lower acceptance rates than other countries, *ceteris paribus*. To generate these specific measures we utilized the data set created by Fearon and Laitin (2003); specifically, we borrowed their variable measuring, by scale, the Muslim population in each country. This measure made little difference in our model. In addition to running our model specified with this index we also created several dichotomous threshold variables from this index to observe if, at some specific tipping point, asylum seekers from countries with a population over a certain percentage of Muslim citizens would receive asylum at rates lower than other countries, *ceteris paribus*. We ran models with this dichotomous variable coded 1 when the Muslim population of a country was greater than or equal to 50 percent, 80 percent, and 90 percent, respectively; again, however, none of these measures made a substantive or statistically significant impact on

[1]Full results available from the authors.

our results. We believe one possible explanation driving this null finding, when compared with the results of Arabic language variable presented in our main models, is that not all predominantly Muslim countries speak the same language (e.g., Bangladesh, Malaysia, and Indonesia). In addition we believe theoretically that asylum seekers from certain countries are less likely to be associated with the September 11 attacks, consciously or not, by asylum officers and judges; after all, the September 11 hijackers where originally from Arabic-speaking countries and not from South and Southeast Asia. This result lends additional credence to our theoretical position that US decision-makers perceive specific cultural cues perhaps unconsciously, as they weigh various factors within the *milieu* of available information, which thus affects the outcome of the asylum process.

We now turn to a possible multicollinearity problem that may exist when we include both the dichotomous measure of US imposed sanctions and the PTS measure of human rights. Because many US sanction programs have the stated mission of improving a country's respect for human rights, countries with a poor record in this regard may also be sanctioned by the US to improve these conditions. In these cases our model may pick up this double counting. However, we checked for this occurrence statistically, in addition to running models with only one of the two variables in question, and found no evidence to support this possibility. We ran similar tests when including the top ten undocumented immigrant-sending country variable and the Spanish language heritage variable; again our results are robust.

Finally we present descriptive statistics for the values taken from each of the observations utilized in our final model presented in Table 6 below. We also present the original wording of the five categories that make up the Political Terror Scale (Gastil, 1980):

Level 1: Countries under a secure rule of law, people are not imprisoned for their view, and torture is rare or exceptional. Political murders are extremely rare.

Level 2: There is a limited amount of imprisonment for nonviolent political activity. However, few persons are affected, torture and beatings are exceptional. Political murder is rare.

Level 3: There is extensive political imprisonment, or a recent history of such imprisonment. Execution or other political murders and brutality may be common. Unlimited detention, with or without a trial, for political views is accepted.

TABLE 6
DESCRIPTIVE STATISTICS

	Mean	Std. Dev.	Min	Max
Immigration Judge Percentage Approved	39.14	19.36	0	100
Asylum Officer Percentage Approved	39.41	17.97	0.39	86.85
Violations of Physical Integrity $(PTS)_{t-1}$	3.14	0.98	1	5
Democracy $(Polity2)_{t-1}$	1.25	5.95	–9	10
US $MilitaryAid_{t-1}$†	0.71	0.46	0	1
US Trade (ln \$1000US 2004)$_{t-1}$	20.06	2.43	14.41	26.32
Sanctions†	0.19	0.39	0	1
English Language†	0.18	0.39	0	1
Arabic Language†	0.13	0.34	0	1
Spanish Language†	0.14	0.34	0	1
Top 10 Undocumented Immigration Countries†	0.08	0.27	0	1
Post 2001†	0.52	0.50	0	1

Notes: Number of observations = 527.
† = discrete change of dummy variable from 0 to 1.
ln = natural log.
PTS = State Department Political Terror Scale (1 = best human rights practices and 5 = worst).
Polity2 = 21-point institutional democracy scale from Autocracy (–10) to Democracy (10).

Level 4: The practices of level 3 are expanded to larger numbers. Murders, disappearances, and torture are a common part of life. In spite of its generality, on this level terror affects those who interest themselves in politics or ideas.

Level 5: The terrors of level 4 have been expanded to the whole population. The leaders of these societies place no limits on the means or thoroughness with which they pursue personal or ideological goals.

APPENDIX REFERENCES

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