On the Use of Social, Economic, and Political Factors to Forecast Instability

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ABSTRACT

This study extends the early-warnings approach presented by O'Brien (2002), which examined macrostructural factors to forecast the intensity level of country-specific instability. This analysis adopts O'Brien's pattern classification algorithm and split-sample validation design to establish baseline forecasting results. It additionally applies maximum-likelihood ordered logistic estimation as an easy-to-use alternative to pattern classification that allows for two primary revisions and extensions to this line of research. First, this study revises the selection of variables to better facilitate cross-country comparisons of the effects of macrostructural factors. Second, it draws upon spline regression methods to provide new empirical evidence for established theories that relate social homogeneity to regional instability. The resulting model maintains reasonable forecasting accuracy while quantifying the relationships between macrostructural factors and regional instability. The practical implication is to focus analysts not only on high-priority geographic regions, but also on those factors that might escalate or mitigate the likelihood of conflict.

KEYWORDS: conflict and instability analysis, ordered logistic estimation, social regime change, spline regression

The need for quantitative early warnings of impending conflicts poses a grand challenge given the idiosyncratic complexities that distinguish regions, groups, cultures, and economies. Statistical methods provide the standard paradigm for identifying general factors leading to conflict, often by modeling a dichotomous (binary) indicator of observed conflict as a logistic response function of measurable macrostructural variables. However, models of observed conflicts fail to adequately address the fact that measurable destabilizing factors can often escalate the instability in a region without tipping the region into violent conflict. Thus, a better approach might examine dependent variables that proxy instability rather than observed occurrences of conflict.

O'Brien (2002) suggests such an approach by adopting a subjective proxy for instability based on the Conflict Simulation Model[2] (KOSIMO) data bank (see Pfetsch and Rohloff 2000), which allows for non-violent conflicts that have the potential to escalate into violent conflicts. From this data, O'Brien derives an instability index in which each country-year is mapped to one of four intensity levels. His approach has practical merit in that it can (1) help focus foreign-policy studies and resource-planning efforts on potential hotspots, and (2) prioritize potential conflicts according to either likelihood-of-occurrence or intensity-in-case-of-occurrence or both.

This paper extends O'Brien's approach in several ways to better identify and quantify the factors that escalate or reduce instability. The resulting model maintains reasonable forecasting accuracy while providing meaningful and quantifiable interpretations of the factors associated with regional instability. The practical implication is to not only focus analysts on high-priority geographic regions, but to also focus them on those factors that exacerbate or mitigate the instability.

This article is arranged as follows. First, I summarize the early-warnings methodology presented by O'Brien. Second, retaining his basic methodology, this study introduces an alternative forecasting algorithm that allows for the prescribed extensions to the overall analysis, and conducts a comprehensive set of performance comparisons of the two algorithms.

Third, this study describes how proxy variables representing a country's *propensity-for-conflict*, as used in previous studies, have the implication of introducing quasi fixed-effects variables into the model, which unfortunately capture the cross-country variation that we would hope to explain with the true macrostructural factors. I therefore exclude this variable to increase the explanatory power of the macrostructural factors, and introduce a statistical correction method known as robust cluster sampling to properly account for the fact that observations are cross-country independent but not necessarily within-country independent.

Fourth, this analysis draws upon the state-strength literature to incorporate regime change as a factor for modeling the impact of social homogeneity on instability. Specifically, I examine how instability relates to the proportion of population belonging to the largest ethnic and religious groups. The analysis presented here extends previous work in four ways. (1) Drawing directly from O'Brien's work, I examine the effects of regime change on polytomous levels of instability rather than dichotomous occurrences of conflict. (2) The model employs logistic splines to compare the *marginal* impact to instability from incremental increases in social homogeneity in different social-regime partitions, rather than to merely compare the relative likelihood of instability due to being in one regime rather than another. (3) The model explicitly estimates responses in three social-regime partitions, rather than two, corresponding to populations that are either socially diverse, intermediate, or homogeneous. (4) The model introduces maximum-likelihood search techniques as a means to objectively identify natural thresholds between regime partitions.

Fifth, after comparing the impacts of social homogeneity on instability as describe above, I examine the empirical relationship between stability and other economic and sociopolitical variables, such as the prevalence of trade openness and civil liberties.

Literature Review

The findings in this article have both practical implications for forecasting, as well as theoretical implications for understanding the relationship between social homogeneity and regional instability. One would typically provide a compendious review of the relevant state-strength and regional-conflict literature at the beginning of such an article, followed by a discussion of theory and empirical findings. However, this article is arranged as a methodological progression from an established empirical model to a new and arguably improved empirical model. For the sake of continuity for the reader, I opt to review the relevant theoretical and empirical literature toward the end of this article in the context of the final empirical model, called the "regime-change model," the results of which are presented in Table 2.

Data

This article extends O'Brien's work by first adopting his dataset. The dependent variable is derived from the KOSIMO data bank, which assumes that violent conflicts evolve from nonviolent crises. Conflicts are categorized according to an intensity index with the following four levels:

1. No conflict,
2. Crisis; mostly nonviolent,
3. Severe crisis; sporadic, irregular use of force, `war-in-sight' crisis,
4. War; systematic, collective use of force by regular troops.

From this categorization, O'Brien constructs a dependent variable of integer values ranging from 1 to 4 corresponding to the highest intensity level of all conflicts experienced by each country in each year.

The original data set data set spans 167 countries with an average of 22 annual observations per country. Each record contains the dependent variable described above, and eleven measured independent variables, including (1) *per capita GDP*, (2) *caloric intake*, (3) *political rights*, (4) *civil liberties*, (5) *trade openness*, (6) *ethnic homogeneity*,

(7) *religious homogeneity*, (8) *democracy*, (9) *average life expectancy*, (10) *infant mortality rat*e, and (11) *youth bulge*. In addition to these *measured* variables, the data contain a *propensity-for-conflict* variable derived for each country from the percentage of sample years spent in conflict. I refer the reader to O'Brien (2002) for further description of the data and sources.

All of the independent variables have missing country-year data. Therefore, some of the country-year observations are incomplete; I call these partial records. Partial records affect the forthcoming comparisons between the pattern-recognition algorithm employed by O'Brien and the regression method introduced in this paper, because the pattern-recognition algorithm can incorporate partial records, but the statistical method ignores partial records. That is, in all of the following empirical analyses, the pattern recognition algorithm will potentially have more information at its disposal than the statistical method. The forthcoming analyses explore the implications of partial records.

Forecasting Methodology

O'Brien uses a pattern classification algorithm called fuzzy analysis of statistical evidence (FASE; see Chen 1995 and 2000) to analyze the relationships between the dependent and independent variables. Once the algorithm trains on a sample of data, it generates four estimated probabilities of occurrence for four conflict levels for each country-year observation. These probabilities sum to one, since all countries must fall into one of the four conflict-level categories. O'Brien aggregates these likelihood measures into one of three expected intensity levels,

1. none or low intensity,
2. moderate intensity, or

3. high intensity,

based on the following six decision rules, which I call *Type I* aggregation:

1. If the combined probability of conflict types 1 and 2 is greater than 67%, then the expected intensity level is "none or low".
2. If the combined probability of conflict types 2 and 3 is greater than 67%, then the expected intensity level is "moderate".
3. If the combined probability of conflict types 3 and 4 is greater than 67%, then the expected intensity level is "high".
4. If more than one of the first three decision rules applies to a particular country-year, then select the highest expected intensity level.
5. If none of the previous decision rules applies, then the expected intensity is indeterminate and the observation is excluded from subsequent calculations of accuracy.
6. A forecast is correct if and only if a country experiences one of the two conflict types associated with the forecasted intensity level.

As with regression methods, FASE allows one to extrapolate; that is, to obtain predicted probabilities and compute expected intensity levels for records that were not used in the training process. This allows O'Brien to conduct a split-sample validation by first estimating a model on 10-, 15-, and 20-year samples of country-year records, and then obtaining expected intensity levels for post-sample records extending 5 years beyond the sample period.[3] We compare the forecasts with the observed intensity levels in the post-sample years by computing the forecast accuracy (equation 1).[4]

$$\text{Accuracy} = \frac{\text{Number of correctly classified country - year observations}}{\text{Number of country - year observations classified}} \tag{1}$$

The model presented in this article retains the basic methodology described above, but introduces a more conventional but perfectly appropriate statistical method known as maximum-likelihood ordered logistic estimation (MLOLE) as an alternative to FASE. This method estimates the relationship between the dependent and independent variables as a logistic response function and, like FASE, generates four estimated probabilities for each country-year representing the likelihood of occurrence of each of the four conflict levels. As with FASE, these likelihood measures are aggregated into one of the three aforementioned expected intensity levels. The expected intensity levels are compared with observed intensity levels via the accuracy calculation.

The results compare the performance of FASE and MLOLE based on accuracy. However, under the six Type I aggregation rules listed above, it is possible for country-year observations to be indeterminate and excluded from the accuracy calculation (rule 5). We find that both FASE and MLOLE exclude observations under rule 5, but they do not exclude the same observations. In most cases the two methods exclude different numbers of observations, resulting in accuracies that are calculated with different denominators. Such inconsistencies preclude direct comparisons on the basis of accuracy. This analysis overcomes this problem by introducing the following six decision rules, which I call *Type II* aggregation:

1. If the combined probability of conflict types 1 and 2 is greater than the combined probabilities of types 2 and 3 and of types 3 and 4, then the expected intensity level is “none or low”.

2. If the combined probability of conflict types 2 and 3 is greater than the combined probabilities of types 1 and 2 and of types 3 and 4, then the expected intensity level is “moderate”.

3. If the combined probability of conflict types 3 and 4 is greater than the combined probabilities of types 1 and 2 and of types 2 and 3, then the expected intensity level is “high”.

4. If more than one of the first three decision rules applies to a particular country-year, then select the highest expected intensity level.

5. If all probabilities equal zero, then the expected intensity level is “none or low”.

6. A forecast is correct if and only if a country experiences one of the two conflict types associated with the forecasted intensity level.

There are no indeterminate observations under Type II aggregation, allowing for consistent comparisons of accuracy across methods.

Ordered Logistic Estimation

This study introduces maximum-likelihood ordered logistic estimation (MLOLE) as an easy-to-use alternative to FASE because it offers two primary advantages. First, MLOLE is well-documented and readily available in commercial off-the-shelf statistical software packages. Second, MLOLE estimates and displays functional parameters (regression coefficients) that allow for direct and meaningful interpretations of the marginal effects of the independent variables on the likelihood of conflict.

All statistical analyses in this article use a widely accepted statistical software package named STATA (see StataCorp 2003). This software supports a fully documented algorithm for MLOLE[5], as described by Aitchison and Silvey (1957), Zavoina and McKelvey (1975), and McCullagh (1980). Borooah (2001) provides a detailed technical discussion, with examples, of the ordered logit algorithm employed in STATA.

Preliminary Comparisons

This analysis includes three split-sample validation exercises to compare the forecasting performances of MLOLE and FASE. The three exercises correspond to three different sample periods. Sample A has a 10-year training period (1975-84) and 5-year forecast period (1985-89). Sample B has a 15-year training period (1975-89) and 5-year forecast period (1990-94). Sample C has a 20-year training period (1975-94) and 5-year forecast period (1995-99). The results for each sample are displayed in Table 1.

Three models are estimated for each sample, including (1) a *full* model of all 12 independent variables, (2) a *reduced* model of 9 independent variables, and (3) a *factor-*

effects model of 8 independent variables. The columns of Table 1 compare forecast performances of different methods. Each cell in Table 1 contains five values.

The first row in each cell lists the number of training records (in brackets) used to analyze the relationships between the dependent and independent variables. Note that the number of training records in the MLOLE column equals the number of training records in the FASE(a) column for each model (row in the table). Recalling that MLOLE can use only complete records for estimating regression coefficients, I generated FASE(a) forecasts by restricting FASE to examine only complete records in order to obtain the most consistence column-wise comparisons between FASE and MLOLE. Note that the number of training records in the FASE(b) column exceeds the number of training records in the FASE(a) column for each model. I generated FASE(b) forecasts by incorporating partial records into the training set in order to determine whether FASE's ability to incorporate partial training records would systematically improve its ability to extrapolate into a sample of forecast records.

The second and third rows in each cell correspond to accuracy of 5-years forecasts. The second row lists the accuracy calculated from type I aggregation (denoted *Accuracy I*), followed by the number of records (in parentheses) that were classified under type I aggregation.[6] The third row lists the accuracy calculated from type II aggregation (denoted *Accuracy II*), followed by the number of records (in parentheses) in the forecast period. Since type II accuracy considers all complete records in the forecast period, it provides the most consistent row-specific comparison of forecast performance across columns.

Since columns MLOLE and FASE (a) use the same training records to provide consistent *column-wise* comparisons, and type II accuracy considers all complete forecast records to provide the most consistent *row-specific* comparisons, it follows that the most consistent *cell-wise* comparisons are provided by comparing the type II accuracy of columns MLOLE and FASE(a). We find that these accuracies are comparable for the full and reduced models in all samples, and that MLOLE achieves somewhat higher accuracies for the factor-effects model.

Full and Reduced Models

The *full* model is taken directly from O'Brien (2002), where FASE was used to examine patterns in the relationship of all 12 independent variables and the likelihood that states will experience a given intensity level of instability. For all three sample periods, MLOLE and FASE achieve comparable forecast accuracy (see Table 1).

The *reduced* model excludes all 3 independent variables obtained from the U.S. Census Bureau, including *youth bulge*, *infant mortality rate*, and *life expectancy*, bringing the total number of independent variables from 12 to 9. I exclude these three variables in the reduced model because they jointly account for a disproportionately large share of partial records, which are deleted by MLOLE during estimation. By excluding these variables, the number of usable training and forecast records are increased. As with the full model, MLOLE and FASE achieve comparable forecast accuracy for the reduced model (see Table 1).

Quasi Fixed-Effects

The full and reduced models both include as an independent variable a proxy for the *propensity-for-conflict*, which is calculated for each country as the percentage of years in the training sample that the country spent in conflict. The reported argument for this variable is that some countries have a higher overall propensity to engage in conflict than others. In the logistic-response framework utilized by MLOLE, this argument describes a fixed-effects model in which instability varies across countries for reasons unknown and not necessarily related to the independent variables.[7] The fixed-effects model dominantly reflects the independent variables in terms of their "within-country" effects on instability, and requires that the "within-country" relationships are the same for all countries.

Fixed-effects models are particularly restrictive for our purposes, because they limit us to within-country comparisons, such as "increases in food consumption within a particular country reduce the odds of conflict," but restrict us from cross-country comparisons such as "countries with higher food consumption have lower odds of conflict." Furthermore, fixed-effects models offer no qualitative or quantitative explanation for the cross-country variation in instability. The full and reduced models listed in Table 1 do not precisely conform to the fixed-effect logistic model, but they include a representation of the observed cross-country response as an independent variable, thereby forming a quasi fixed-effects representation. For the aforementioned reasons, the remainder of this paper develops alternatives to the quasi fixed-effects full and reduced models.

Factor-Effects Model

To obtain a more reasonable and meaningful model of the influence of the macrostructural factors, this study now proceeds to the factor-effects model, which excludes the propensity-for-conflict variable and relies strictly on the independently measured macrostructural variables. This approach still requires a means to statistically correct for the fact the country-years observations are cross-country independent, but not necessarily within-country independent. As I will discuss later, such corrections are possible using a cluster-sampling technique. The result is a legitimate model, whose forecast results can still be directly compared to those generated using FASE, and which provides a baseline for the more sophisticated regime-change model that will follow in the next section.

This factor-effects model estimates the effects of the 8 remaining independent variables. Row-wise comparisons in Table 1 show that we experience a loss in type II accuracy for both MLOLE and FASE when we move from the reduced model to the factor-effects model, but this is a worthwhile trade-off for capturing cross-country comparisons that relate instability to the independent variables. The factor-effects model concludes my comparison between MLOLE and FASE and provides the foundation for the remaining analyses in this article.

To this point, this study has identified MLOLE as an appropriate method for analyzing this type of data, and demonstrated its ability to perform comparably with FASE in split-sample validations of forecast accuracy. However, it has not addressed the statistical soundness of the empirical results. To do so, I refer the reader to the factor-effects model results listed in Table 2. For each independent variable listed in the first column, the factor-effects column lists the associated odds ratio (with P-values listed to the right).

To interpret the results, first recall that the *odds* in standard logistic regressions of a dichotomous variable represents the likelihood of an occurrence relative to a non-occurrence (i.e. Odds(x) = Pr(x)/[1-Pr(x)]). The *odds ratio* represents the change in the odds due to a unit increase in the independent variable. Odds ratios greater than one imply that the odds, and therefore probability, are increasing; odds less than one imply the odds are decreasing. In the current analysis, in which the dependent variable has four levels of intensity, the *odds* denotes the odds that instability will escalate to the next level of intensity. Brant (1990) and Borooah (2001) describe the interpretation of odds for ordered logistic models (a.k.a. conditional logit models). For example, in the factor-effects model of Table 2, the estimated odds ratio for political rights is 1.13, which implies that a unit increase in political rights will increase the odds by 13%. In this case, a unit increase in political rights is defined by a 10 percentage-point move along the civil-liberties index. So, a 10 percentage-point move along the civil-liberties index increases the odds by 13% that instability will escalate to the next level of intensity. The remainder of this paper uses the phrase "odds of escalation" to mean the "odds that instability will escalate to the next level of intensity."

The factor-effects model yields some intuitive results, whereby openness, freedom, and wealth decrease the likelihood of escalating instability. For example, we find that a $1,000 increase in per-capita GDP reduces the odds of escalation by 3%. An expansion of trade openness (value of imports and exports) equal to 10% of GDP decreases the odds of escalation by 16%. Finally, a 10% move up on the civil liberties index reduces the odds of escalation by 39%.

The results for democracy and political rights imply that these factors increase the odds of escalation. I discuss these results more extensively in the context of the regime-change model, but for now I will simply state that multicollinearity is the culprit for these non-intuitive results.

If ethnic and religious homogeneity relate similarly to group dynamics, and group dynamics relate to stability, then one would expect ethnic and religious homogeneity to have similar impacts on the odds of escalation. However, that is not the finding in the factor-effects model. We find that a one percentage point increase in the proportion of population belonging to the largest ethnic group reduces the odds of escalation by 1%, whereas a similar increase in the largest religious group increases the odds of escalation by 3%. This counterintuitive result occurs because the factor-effects model fails to properly address regime change in the context of social homogeneity. This problem is subsequently corrected in the regime-change model.

In summary, having excluded the fixed-effects variable representing propensity-for-conflict, the factor-effects model identifies statistically significant relationships, and demonstrates that MLOLE obtains forecasting results that are comparable or superior to FASE. However, it fails to properly specify and test for some key dynamics, specifically those relating to social regime change.

Cluster Sampling

By removing the propensity-for-conflict variable, the factor-effects model allows us to make cross-country and within-country comparisons with respect to changes in the independent variables. The statistical criteria for testing the significance of such relationships are the standard errors associated with the odds ratios. If the country-year observations are not within-country independent, then the estimated standard errors will underestimate the true standard errors, possibly allowing us to conclude that factors are significant when they are not. We correct for this problem with a technique known as cluster sampling (Huber 1967, Rogers 1996), which provides robust estimates of the standard errors. For the regime-change model described below, Table 2 presents both normal and robust P-values corresponding to the normal and robust standard errors of the odds ratios.

Regime Change Model

The remainder of this article describes modifications to the factor-effects model to incorporate the principle of social regimes. In this article, we do not use the phrase *regime change* in its more common context, a change in governing authority, but rather use the phrase to refer to a shift between distinctly different levels of social diversity. I describe the empirical methodology for modeling regime change, review previous empirical studies in the state-strength literature that identify social regimes in the context of social homogeneity, and expand the factor-effects model to include regime comparisons based on social diversity and calorie consumption. The resulting empirical model provides significant insights into the role of social, economic, and political factors, and achieves reasonable forecasting results.

Variate-Based Empirical Regime Change

The principle of variate-based empirical regime change applies when (1) a continuous independent variable can be partitioned into sub-intervals that meaningfully represent fundamentally different social regimes, and (2) the independent variable has a different impact on the dependent variable in different regimes. For example, one might define various metrics by which to measure a country's level of ethnic diversity. Regime change might apply if there exist fundamentally different conditions and relationships in *ethnically diverse* versus *ethnically homogenous* regimes, and if one can meaningfully partition an ethnic-diversity metric into distinct regimes.

Spline Methodology

This study uses spline methods to model regime changes. To understand the spline method, we first recall that our model estimates the likelihood of escalation as the logistic response to a linear combination of independent variables. Suppose that $k = \alpha + \beta \cdot x + \gamma y + \delta z$ is a linear combination of independent variables x, y, and z. The model selects coefficients α, β, γ, and δ to obtain the maximum-likelihood estimates of the logistic response function

$$\Pr(escalation \mid x, y, z) = \frac{e^k}{1+e^k}$$

The estimated response to an increase in x, when x is incrementally increased by Δx, is described by the corresponding odds ratio

$$OR_{\Delta x} = \frac{Odds(escalation \mid (x+\Delta x), y, z)}{Odds(escalation \mid x, y, z)} = e^{\beta \cdot \Delta x}.$$

We introduce regimes into this model using indicator variables and interaction terms, as is commonly done in linear regression models (Marsh and Cormier 2001). For example, suppose the domain interval for variable x spans two distinct regimes that are partitioned by a threshold (aka. spline knot) x_0 such that $[x_{min}, x_0]$ defines one regime and $[x_0, x_{max}]$ defines the other regime. We would estimate a 2-regime spline model by introducing two variables, an indicator variable

$$d = \begin{Bmatrix} 0 & , x \le x_0 & \text{(regime 1)} \\ 1 & , x > x_0 & \text{(regime 2)} \end{Bmatrix}$$

and an interaction variable

$$d \cdot x = \begin{Bmatrix} 0 & , x \le x_0 & \text{(regime 1)} \\ x & , x > x_0 & \text{(regime 2)} \end{Bmatrix}.$$

We introduce these variables into the linear combination to obtain $k = \alpha + \phi \cdot d + \beta \cdot x + \theta \cdot d \cdot x + \gamma \cdot y + \delta \cdot z$, where

$$k = \begin{Bmatrix} \alpha + \beta x + \gamma y + \delta z & , x \le x_0 \;\; \text{(regime 1)} \\ (\alpha + \phi) + (\beta + \theta)x + \gamma y + \delta z & , x > x_0 \;\; \text{(regime 2)} \end{Bmatrix}.$$

Under this framework, the marginal effect of a Δx increase on the odds of escalation will differ depending on the regime, where the odds ratio differs among regimes as follows:

$$OR_{\Delta x} = \begin{Bmatrix} e^{\beta \cdot \Delta x} & , x \le x_0 \;\; \text{(regime 1)} \\ e^{(\beta+\theta)\cdot \Delta x} & , x > x_0 \;\; \text{(regime 2)} \end{Bmatrix} \tag{2}$$

In practice, statistical packages will provide three *estimated* odds ratios, denoted $\overline{OR}$, corresponding to the three variables x, d, and $d \cdot x$. The expected response under regime 1 is obtained directly from the estimated odds ratio for Δx

$$E[OR_{\Delta x} \mid x \le x_0] = \overline{OR}_{\Delta x} = e^{\beta \cdot \Delta x},$$

but the expected response under regime 2 is obtained from the product of the odds ratios corresponding to variables x and $d \cdot x$

$$E[OR_{\Delta x} \mid x > x_0] = \overline{OR}_{\Delta x} \cdot \overline{OR}_{d \cdot \Delta x} = e^{\beta \cdot \Delta x} \cdot e^{\theta \cdot \Delta x} = e^{(\beta+\theta)\cdot \Delta x}.$$

Such calculations are implicit in the forthcoming discussion of empirical results. Jaccard (2001) describes this methodology in greater detail.

Social Groups: Methods and Findings

Researchers study social homogeneity as one of several factors tied to social and political discrimination and oppression. These dynamics have been studied from many perspectives (see Gurr 1993, Kubicek 1997, Collier 2000 and 2001, Ellingsen 2002, Murshed 2002, Reynal-Querol 2002, Caprioli and Trumbore 2003). O'Brien (2002) finds that ethnic and religious homogeneity increases the likelihood of crisis, but he does not explicitly define discrete regimes. Previous work, however, has identified a clear need to differentiate regimes in the context of social diversity based on such questions as whether a society is comprised of a dominant social group, or significant minority groups.

The literature suggests three regimes with respect to social group size: (1) a *diverse* regime in which no group is proportionally large enough to assert dominance based strictly on its relative size, (2) an *intermediate* regime in which the largest social group is proportionally large enough to discriminate against the rest of the population, and (3) a *dominant* regime in which the dominant group is so large that minority groups become insignificant. Both Collier (2000) and Ellingsen (2002) suggest a three-regime model, but offer different explanations for the existence of the *dominant* regime. Collier examines the issue from an economic perspective, and argues that a dominant group can become so large that it becomes pointless to discriminate against minorities. Empirically, he tests for the existence of an *intermediate* regime and finds that the likelihood of civil war doubles when the largest ethnic group constitutes between 45% and 90% of the population. Ellingsen argues that a dominant group can become so large that minorities become powerless and abandon efforts to resist the dominant group. Empirically, she tests for the existence of the *dominant* regime and finds that the likelihood of domestic conflict is halved when the largest ethnic, religious, or linguistic group constitutes more than 80% of the population.

The literature generally describes factors such as ethnic and religious affiliations as contributors to group identity and cohesion. One might suggest that these are orthogonal contributors, and should therefore have independent yet similar relationships with stability.[8] Such hypotheses imply that social-regime partitions apply independently to each factor that contributes to social homogeneity. For our data, this implies

Proposition 1: The largest ethnic group will switch regimes at the same thresholds as the largest religious group.

This study extends the approaches taken by Collier and Ellingsen is four ways. First, I adopt O'Brien's approach by conducting this analysis on ordered multinomial measures of conflict intensity, rather than dichotomous incidences of civil war or armed conflict.

Second, this study not only compares the relative odds of escalation under alternate regimes, but it also compares the marginal response in the odds of escalation from incremental changes group size under alternate regimes. In the previously-defined notation, Collier and Ellingsen used an indicator term $\phi{\cdot}d$ to compare the latent odds of conflict between regimes

$$E[OR_d] = \begin{Bmatrix} e^{\alpha} & , x \le x_0 & \text{(regime 1)} \\ e^{(\alpha+\phi)} & , x > x_0 & \text{(regime 2)} \end{Bmatrix}.$$

I extend this approach by also using an interaction term $\theta{\cdot}d{\cdot}x$ to compare the *marginal response* to group size across regimes (see equation 2). This extension avoids potential estimation bias and provides more meaningful interpretations of the response to social-homogeneity dynamics.

Third, rather than distinguishing one regime from the other two, this model simultaneously tests for all three regimes: diverse, intermediate, and dominant. Like Collier, I identify two thresholds to partition the social grouping factors into three regimes. However, I introduce two sets of independent parameters to independently compare both the *intermediate* and *dominant* regimes against the baseline *diverse* regime. The thresholds for ethnic group size are assumed (allowed) to be independent from those for religious group size. If we let x_{τ_1} denote the threshold between the diverse and intermediate regimes and let x_{τ_2} denote the threshold between the intermediate and dominant regimes, then the model estimates three marginal responses to unit changes in group size:

$$E[OR_{\Delta x}] = \left\{ \begin{array}{lll} e^{\beta \cdot \Delta x} & , x \le x_{\tau_1} & (\text{regime } 1) \\ e^{(\beta+\theta)\cdot \Delta x} & , x_{\tau_1} < x \le x_{\tau_2} & (\text{regime } 2) \\ e^{(\beta+\theta+\rho)\cdot \Delta x} & , x_{\tau_2} < x & (\text{regime } 3) \end{array} \right\}.$$

Figure 1 provides a visual representation of the hypothesized relationship between the odds of escalation and social diversity in the three regimes. This model allows one to test three propositions:

Proposition 2: Increases in the proportion of the largest social group do not increase instability in socially diverse regimes,

Proposition 3: Increases in the proportion of the largest social group do increase instability in socially intermediate regimes,

Proposition 4: Increases in the proportion of the largest social group increase instability in socially intermediate regimes more than in dominant regimes.

Fourth, I introduce maximum-likelihood search as an objective method for identifying the regime thresholds for social group size. This process is appropriate when thresholds (knot locations) are unknown (Marsh and Cormier 2001). Under this method, one assumes a range of possible values for regime thresholds, varies each threshold incrementally across its respective range, estimates a logistic model for each set of thresholds, and observes the log-likelihood estimate for each model. The model that achieves the highest log-likelihood value is the maximum-likelihood model.[9] This analysis draws from Collier (2000) to define an interval of 45% to 90% across which to search. I specify a search across this interval by allowing each threshold to vary in 1% increments.[10] The results in Table 3 show that the intermediate regime ranges from 60% to 83% for the largest ethnic group, and from 61% to 88% for the largest religious group. Note that the search independently identified nearly the same regime partitions for ethnic and religious group proportion; this result supports proposition 1.

Empirical findings for the regime-change model, shown in Table 2, support propositions 2, 3, and 4 for both ethnic and religious factors; the only exception is an inability to observe a distinction in response between religiously intermediate and dominant regimes. Consider the effect of a one percentage-point increase in the proportion of population belonging to the largest ethnic group:

a. In ethnically *diverse* regimes, where the group's proportion is less than 60%, the odds that instability will escalate to the next level of intensity *declines* by 3%. Under robust estimates, this estimated decline is not statistically significant. However, both findings are consistent with Proposition 2.

b. In ethnically *intermediate* regimes, where the group's proportion is between 60% and 83%, the odds of escalation *increases* by 18%.

c. In ethnically *dominant* regimes, where the group's proportion exceeds 83%, the odds of escalation *declines* by 5%.

Now, consider the effect of a one percentage-point increase in the proportion of population belonging to the largest religious group:

d. In religiously *diverse* regimes, where the group's proportion is less than 61, the odds of escalation *declines* by 30%.

e. In religiously *intermediate* regimes, where the group's proportion is between 61% and 88%, the odds of escalation *increases* by 10%.

f. The data fail to suggest a significant difference in the response between religiously *dominant* and *intermediate* regimes. This finding fails to support Proposition 4.

Caloric Consumption: Method and Findings

The factor-effects model showed that the odds of escalation increases by 4% in response to each 100 calorie increase in daily per-capita caloric intake. Rather than a social explanation, this result is better explained by estimation bias. Conventional wisdom suggests that people will fight over food only once it becomes scarce in an absolute sense. We would not expect marginal declines in the availability of food to escalate instabilities in affluent societies, but we might in societies where food consumption fell below some minimum physical-need threshold. This logic implies

> *Proposition 5:* Below a minimum-calorie threshold, increases in caloric intake will decrease the likelihood that instability will escalate.

> *Proposition 6:* Above a minimum-calorie threshold, changes in caloric intake will have no effect on the likelihood that instability will escalate.

To explore these propositions, I define a two-regime spline model. Based on a sorting of the daily calories variable, which ranged from 1520 to 3771, I selected the 10th percentile value of 1961 calories as the regime threshold. The results shown in Table 2 support proposition 5 and are inconclusive regarding proposition 6. In the low-calorie regime, a 100 calorie increase reduces the odds of escalation by 35%, which supports proposition 5. In the high-calorie regime, the relationship clearly plateaus; a 100 calorie increase will increase the odds of escalation by a mere 5% (obtained by the product of the daily calories parameter, 65%, and the calories interaction term, 163%). Unfortunately, proposition 6 implies that the marginal response in the high-calorie regime should be 0%. A χ^2 test rejected proposition 6 with only 5.4% significance. These findings suggest that the results regarding proposition 6 are inconclusive. Nevertheless, it is clear that food scarcity contributes to instability at the lowest consumption levels, and that consumption has a relatively small influence if any on stability at higher consumption levels.

Economic Factors

Economic fitness is represented by variables for per-capita GDP and trade openness. The following propositions are ubiquitous in the economic and state-strength literature.

Proposition 7: Increases in per-capita GDP will decrease the likelihood that instability will escalate to the next intensity level.

Proposition 8: Increases in trade openness will decrease the likelihood that instability will escalate to the next intensity level.

The results in Table 2 support these propositions. A $1,000 increase in per-capita GDP reduces the odds of escalation by nearly 6%. An expansion of trade openness that is proportional to 10% of national GDP will decrease the odds of escalation by nearly 19%.

Sociopolitical Factors

The model includes three variables representing the relative placement of each country on indexes that indicate the relative levels of democracy, political rights and civil liberties. The following propositions are ubiquitous in the state-strength literature.

Proposition 9: Increases in democracy will decrease the likelihood that instability will escalate to the next intensity level.

Proposition 10: Increases in political rights will decrease the likelihood that instability will escalate to the next intensity level.

Proposition 11: Increases in civil liberties will decrease the likelihood that instability will escalate to the next intensity level.

The results in Table 2 provide conclusive support for proposition 11. Specifically, a 10% progression along the civil liberties index will decrease the odds of escalation by nearly 41%. The results for democracy and political rights are contrary to propositions 9 and 10, but are also less conclusive because the robust P-values suggests that corresponding odds-ratios are not statistically significant (at the 5% significance level). Although one could argue that democracy can increase the likelihood of conflict under certain conditions (e.g. Jensen 1997), we should expect broad cross-country comparisons such as those estimated in this model to conform to the common view that democracy and political rights increase the likelihood that objectives will be pursued through non-violent means.

The clear culprit for these counterintuitive empirical findings for democracy and political rights is *multicollinearity*. The political rights variable is over 91% correlated with the other two variables, which themselves are 87% correlated. The most common problem resulting from multicollinearity are inflated standard errors associated with the correlated variables, which could preclude one from accepting that the variables are statistically significant factors in the model. However, that is not the problem in this case. Here, the primary problem resulting from multicollinearity is that the estimated coefficients (and therefore the odds ratios) associated with correlated variables are biased.

Standard corrections for such estimation bias from multicollinearity are to either (1) drop one or more of the correlated variables, (2) devise a reasonable indexed variable to serve as a proxy for the correlated variables, or (3) resort to more elaborate estimation techniques such as principal components or ridge regression. Since these three variables are included primarily as *control* variables, it is beyond the scope of this article to explore exhaustive corrective measures for differentiating the individual effects of each of these variables on instability. However, completeness demands limited efforts to confirm and correct for multicollinerity, which I do by estimating three subsequent *reduced* regime-change models (not shown), each of which excludes two of the three correlated variables. In each case, the resulting odds ratio for the variable-of-interest is less than one[11], which is consistent with propositions 9, 10, and 11. Of these three models, the model that included civil liberties was the only model in which the sociopolitical variable-of-interest was significant at the 5% level for both standard and robust P-values; it estimated that a 10% progression along the civil liberties index will decrease the odds of escalation by nearly 19%. These results are consistent with those of the full regime-change model shown in Table 2, in which civil liberties is the only one of the three sociopolitical variables found to be significant at the 5% level based on robust P-values.

It seems reasonable to conclude from the sociopolitical variables, first, that civil liberties[12] are more influential on stability than political participation (i.e. democracy and political rights), and, second, that civil liberties increase stability according to proposition 11.

Conclusions

We can derive significant insights and more precise forecasts by modeling polytomous levels of instability rather than dichotomous occurrences of conflict. From a practical perspective, such enhancements can improve our ability to identify those regions whose stability is likely to decline, and better understand the factors that will escalate or diminish the likelihood of conflict.

Perhaps the most significant contributions of this article relate to the ability to explicitly estimate distinct social-homogeneity regimes, and to compare the marginal response in stability to changes in social homogeneity across regime thresholds. The corresponding empirical findings support various hypotheses related to the role of groups in understanding social phenomena. Specifically, we find that instability tends to begin to escalate when the largest ethnic or religious group exceeds a particularly threshold proportion of the population, which this study estimates to be 60%. Presumably, such a grouping allows the dominant group to discriminate against minorities, leading to instability. In the case of a dominant ethnic group, we find that instability tends to begin to decline once the dominant group exceeds a subsequent threshold, which this study estimates to be 83%, presumably because the minorities become so small that either it becomes futile for minorities to resist, or it becomes valueless for the majority to discriminate.

In addition to social homogeneity, one can simultaneously explore the marginal influence of economic and sociopolitical factors on stability. Although a few propositions regarding these factors were empirically inconclusive, we generally find that ideologically appealing factors such as per-capita GDP, trade openness, daily calories, and civil liberties decrease the likelihood that instability will escalate to the next level of intensity.

Future Work

Practical applications for this line of research can extend beyond the scope of the forecasts described in this paper. For example, Borooah (2001) provides examples for estimating the marginal effect of incremental changes in independent variables on each of the probabilities corresponding to the possible outcomes. For our purposes, one could estimate the marginal effect of *hypothetical* changes in the independent variables on the forecasted probability corresponding to each level of instability for each country.

Such an approach could help analysts to more precisely anticipate the possible impacts to stability from a myriad of hypothetical scenarios, such as changes in land-use, cross-border migration patterns, climatologic shifts, environmental scarcity, capital investments, economic sanctions, or various international policies or resolutions. The resulting foresights could aid to (1) focus resource-planning efforts on potential hotspots, and (2) prioritize potential conflicts according to either likelihood-of-occurrence or intensity-in-case-of-occurrence or both.

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Footnotes

[1] The data for these analyses were obtained from www.geocities.com/seanob88, and are also available at www.yale.edu/unsy/jcr/jcrdata.htm.

[2] This data is maintained at the Heidelberg Institute on International Conflict Research (HIIK) at the Department of Political Science at the University of Heidelberg. The KOSIMO Manual is available at (www.hiik.de/en/manual.htm).

[3] O'Brien actually forecasts the expected intensity levels for post-sample records extending not only 5 years, but also 10 and 15 years beyond the sample period. However, merely for manageability of the results, I limit performance comparisons between our methods to 5-year forecasts.

[4] O'Brien actually defines and computes all three standard performance metrics for forecasting, including accuracy, recall, and precision. In this article, I revise the steps by which country-year forecasts are aggregated to obtain more direct accuracy comparisons between our two empirical methods. The metrics of recall and precision are less important for these comparisons.

[5] See the *oligit* command in the Stata Reference Manual.

[6] Note that the accuracy estimates reported by O'Brien (2002; see Table 3) do not appear in Table 1 of this article. The reason is that MLOLE can only extrapolate to *complete* forecast records, whereas FASE can extrapolate to both *complete* and *partial* forecast records. That is, FASE can generate forecasts for country-year records that have missing data, whereas MLOLE cannot. For example, O'Brien used FASE (b) with a Full Model for Sample A to forecast instability for 705 eligible forecast records. Of these, 97 of the forecasts were *indeterminate*, and the accuracy for the remaining 608 records was 90%, as reported in Table 3 of O'Brien 2002. In this article, to compare FASE to MLOLE for Sample A, I discard all partial forecast records and thereby reduce the number of eligible forecast records from 705 to 305. I use FASE (b) with a Full Model for Sample A to forecast instability for 305 eligible forecast records. Of these, 39 of the forecasts were *indeterminate*, and the [type I] accuracy for the remaining 266 records was 87.6%. Similarly, the number of eligible forecast records is reduced in this article from 772 to 457 for Sample B, and from 795 to 562 for Sample C.

[7] A similar description is the random-effects model, in which the propensity-for-conflict varies across countries according to a random distribution.

[8] For example, if the largest religious group were an equal subset of the largest ethnic group, then both social groups would cross regime thresholds at the same proportions. Alternately, if the largest religious group remained static in relative proportion to the population, then a growing ethnic group would cross regimes thresholds while the largest religious group would not. In another extreme, if the largest religious group was split between the largest ethnic group and ethnic minorities, then a growing ethnic group could cross regimes thresholds while the largest religious group would not. Alternately, if the largest religious group encompassed the entire population, then a growing ethnic group could cross regimes thresholds while the largest religious group could not.

[9] Although not done here, one can adjust the standard errors of the estimated odds ratios to account for the fact that the thresholds are being estimated. To do so, one would add the number of thresholds (m_T) to the number of independent variables (m_V) to obtain the true number of estimated parameters, ($m_{Total} = m_V + m_T$). All standard errors would be adjusted (increased) accordingly by replacing the previous m_V with the adjusted m_{Total}. This exercise would increase the associated P-values and provide more robust hypothesis tests, but would not change the estimated odds ratios.

[10] I restrict the width of the intermediate regime to be at least 20%. That is, $(x_{\tau_2} - x_{\tau_1}) \geq$ 20%.

[11] In each of the three models, the estimated odds ratios for the other variables are reasonably unaffected.

[12] Defined by Freedom House (www.freedomhouse.org) as a measure of freedom of a country's people "to develop views, institutions, and personal autonomy apart from the state."

Table 1

Forecast-of-Instability Comparisons of Maximum-Likelihood Ordered Logistic Estimation (MLOLE) versus Fuzzy Analysis of Statistical Evidence (FASE)

	MLOLE complete records only		FASE (a) complete records only		FASE (b) complete & partial training records	
Training: 1975-84 — Sample A — Forecast: 1985-89						
Full Model		*[271]*		*[271]*		*[1399]*
Accuracy I	*92.4*	*(275)*	*82.4*	*(289)*	*87.6*	*(266)*
Accuracy II	*87.9*	*(305)*	*89.2*	*(305)*	*84.3*	*(305)*
Reduced Model		*[820]*		*[820]*		*[1399]*
Accuracy I	*84.5*	*(440)*	*86.2*	*(407)*	*84.8*	*(409)*
Accuracy II	*83.4*	*(463)*	*86.4*	*(463)*	*83.4*	*(463)*
Factor-effects Model		*[820]*		*[820]*		*[1399]*
Accuracy I	*85.3*	*(334)*	*78.7*	*(343)*	*76.5*	*(268)*
Accuracy II	*80.0*	*(463)*	*75.4*	*(463)*	*72.1*	*(463)*
Training: 1975-89 — Sample B — Forecast: 1990-94						
Full Model		*[576]*		*[576]*		*[2104]*
Accuracy I	*82.2*	*(437)*	*79.8*	*(425)*	*82.3*	*(406)*
Accuracy II	*81.4*	*(457)*	*82.5*	*(457)*	*83.2*	*(457)*
Reduced Model		*[1283]*		*[1283]*		*[2104]*
Accuracy I	*80.7*	*(466)*	*76.3*	*(448)*	*77.6*	*(446)*
Accuracy II	*78.5*	*(497)*	*78.5*	*(497)*	*77.1*	*(497)*
Factor-effects Model		*[1283]*		*[1283]*		*[2104]*
Accuracy I	*82.8*	*(400)*	*68.8*	*(346)*	*78.1*	*(265)*
Accuracy II	*76.0*	*(534)*	*67.0*	*(534)*	*66.1*	*(534)*
Training: 1975-94 — Sample C — Forecast: 1995-99						
Full Model		*[1067]*		*[1067]*		*[2876]*
Accuracy I	*81.2*	*(520)*	*82.3*	*(480)*	*82.5*	*(480)*
Accuracy II	*78.5*	*(562)*	*82.9*	*(562)*	*81.7*	*(562)*
Reduced Model		*[1817]*		*[1817]*		*[2876]*
Accuracy I	*81.7*	*(530)*	*83.9*	*(490)*	*83.1*	*(491)*
Accuracy II	*80.0*	*(583)*	*82.0*	*(583)*	*79.1*	*(583)*
Factor-effects Model		*[1817]*		*[1817]*		*[2876]*
Accuracy I	*84.1*	*(440)*	*77.0*	*(344)*	*83.6*	*(275)*
Accuracy II	*76.3*	*(583)*	*68.6*	*(583)*	*64.5*	*(583)*

Numbers in brackets [ ] denote numbers of training records.
Numbers in parentheses ( ) denote numbers of forecast records.

Table 2

Ordered Logistic Estimation of Instability Intensity Levels

	Factor-effects Model		Regime-Change Model			
Observations	*1817*		*1817*			
Log-likelihood	*-1514*		*-1365*			
$\sigma(\chi^2)$	*0.00*		*0.00*			
Independent variable	*Odds ratio*	*P-value*	*Odds ratio*	*P-value*	*Robust P-value*	*Notes*
Per capita GDP	*.97*	*.001*	*.94*	*.000*	*.008*	*Per $1,000*
Trade openness	*.84*	*.000*	*.81*	*.000*	*.001*	*Per 10% of GDP*
Civil liberties	*.61*	*.000*	*.59*	*.000*	*.000*	*Per 10% of index*
Political Rights	*1.13*	*.010*	*1.21*	*.000*	*.065*	*Per 10% of index*
Democracy	*1.20*	*.000*	*1.13*	*.001*	*.212*	*Per 10% of index*
Daily calories	*1.04*	*.005*	*.65*	*.070*	*.332*	*Per 100 calories*
Indicator term			*.00*	*.050*	*.299*	*Exceeds 10th percentile*
Interaction term			*1.63*	*.041*	*.278*	
% in largest ethnic group	*.99*	*.002*	*.97*	*.007*	*.365*	*Per 1% of pop*
Indicator 1 term			*.00*	*.000*	*.001*	*Exceeds 59%*
Interaction 1 term			*1.22*	*.000*	*.001*	
Indicator 2 term			*10^7*	*.000*	*.004*	*Exceeds 83%*
Interaction 2 term			*.81*	*.000*	*.001*	
% in largest religious group	*1.03*	*.000*	*.79*	*.000*	*.001*	*Per 1% of pop*
Indicator 1 term			*.00*	*.000*	*.000*	*Exceeds 60%*
Interaction 1 term			*1.39*	*.000*	*.000*	
Indicator 2 term			*.10*	*.362*	*.684*	*Exceeds 88%*
Interaction 2 term			*1.01*	*.810*	*.916*	
Forecasts						
Type I accuracy	*84.1*		*85.0*			
Type II accuracy	*76.3*		*77.2*			

Table 3

Estimated Partitions for Social-Diversity Regimes

	Regime Partition		
	Diverse	Intermediate	Dominant
Largest ethnic group	0% - 59%	60% - 83%	84% - 100%
Largest religious group	0% - 60%	61% - 88%	89% - 100%

Figure 1

Partitions for Social-Diversity Regimes