# Measuring Grievance:

## Ethno-Political Exclusion and Civil War Onset*

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*Abstract:*

In a previous paper entitled "Beyond Fractionalization: Mapping Ethnicity onto Nationalist Insurgencies," we introduced a new index of ethno-political mobilization, called $N^*$ that captures the risk of civil war. However, this opportunity-driven measure merely captures the potential for violent conflict. In the current paper, we develop an extended model of civil war based on a combination of $N^*$ and an indicator of ethnic exclusion. We find statistical support for a model that relies on both ethno-political opportunity structures and minority grievances.

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In the recent literature on civil wars, it has become customary to classify explanations according to whether they focus on opportunities or incentives of rebellion. This logic is frequently thought of in terms of Collier and Hoeffler's (2004) oft-cited dichotomy between "greed" and "grievance." These authors also contend that greed, rather than grievance, explains even ethnic civil wars. Similarly, Fearon and Laitin (2003) reject explanations that center on ethnic grievance in favor of an opportunity-driven model of insurgency.

However, the greed-grievance framing of the debate rests on two typically unstated assumptions, which we argue are both erroneous. First, it is assumed that opportunity structures are primarily materialist. Second, the explanatory dichotomy is interpreted as if opportunities and grievances were mutually exclusive, with the former usually dominating the latter. Most contributions to the political-economy literature on internal conflict subscribe to both assumptions, i.e. to materialist opportunism and explanatory priority (see again Collier and Hoeffler 2004; Fearon and Laitin 2003).

In our previous work, we questioned the first of these assumptions by suggesting that specific ethnic configurations are more conflict-prone than others. In particular, Cederman and Girardin (2005) show that the alleged failure of ethnic explanations of civil-war onset is due to the theoretical inadequacy of the most popular operationalization of ethnic opportunity structures, the ethno-linguistic fractionalization (*ELF*) index.

Although our alternative perspective casts doubt on the assumption of materialist opportunism, it says little about the second assumption. In the current paper, we explicitly reject the notion of explanatory priority in favor of a model that combines both opportunity and motivation as necessary conditions for violence to erupt. We do so by constructing a measure of ethnic minorities' grievance levels and combining it with our model of simple ethnic opportunity structures.

Our argument is laid out in four sections. First, we summarize our formalization of $N*$ and expose it to a cross-sectional test. Then follows a section that introduces an aggregate measure of ethnic grievances, before turning to the combined opportunity-grievance model. A concluding section discusses the theoretical repercussions of our analysis.

## Modeling ethnic opportunity structures

Drawing on classical theories of collective violence, scholars of civil wars typically divide their explanations into those that stress opportunities and those that emphasize motivations. For instance, Collier and Hoeffler's (2004) catchy formula "greed and grievance" associates greed with opportunities and grievance with motives. In a critique of psychological theories of conflict, especially Gurr's theory of relative deprivation (Gurr 1970), Collier and Hoeffler assert that "all societies may have groups with exaggerated grievances" (p. 564). In their view, rebel movements' opportunities to stage collective action determine the outbreak of civil wars: "'opportunity' and 'viability' describe the common conditions sufficient for profit-seeking, or not-for-profit, rebel organizations to exist" (p. 565). According to Collier and Hoeffler (2004) individual-level opportunity costs, proxied through primary commodity exports among other indicators, go a long way toward accounting for civil war onset. By and large, their rendering of rebels' opportunity structures is a materialist one, although they include "social cohesion" as a factor measured in terms of the *ELF* index.

Similarly, Fearon and Laitin (2003) propose an opportunity-driven model that stresses logistical factors of insurgency, although their determinants relate to state-level factors such as state strength and geography, rather than to individual incentives. Like Collier and Hoeffler, Fearon and Laitin have a primarily materialist interpretation of structural opportunities in mind. In their view, analysts would be mistaken to argue

> that ethnic diversity is the root cause of civil conflict when they observe insurgents in a poor country who mobilize fighters along ethnic lines. Instead, the civil wars of the period have structural roots, in the combination of a simple, robust military technology and decolonization, which created an international system numerically dominated by fragile states with limited administrative control of their peripheries (Fearon and Laitin 2003, p. 88).

In our previous work, we took issue with this narrowly materialist rendering of opportunity. In Cederman and Girardin (2005), we argue that specific ethnic configurations are more conflict-prone than others. Thus, the notion of opportunity cannot be divorced from cultural factors. In order to rectify these shortcomings, we proposed an alternative measure of ethno-nationalist conflict-proneness that we call *N** and which appears to be strongly related to conflict outcomes, in particular ethnic civil wars.

To prepare the ground for our model of grievance, we summarize the logic of the *N** index as defined by Cederman and Girardin (2005). It is important to note that this index deviates from standard fractionalization measures by introducing state-centric rather than symmetric ethnic configurations and by postulating group-level, rather than individual-level micro-mechanisms of mobilization.

Let us assume that a state features an ethnic configuration comprising *n* groups $\{ s_0, s_1, s_2 \ldots s_{n-1} \}$ where $s_0$ denotes the ethnic group(s) in power (*EGIP*). For operational purposes, we consider a group, or a coalition of groups, to be in power if their leader(s) serve (at least intermittently) in senior governmental positions, especially within the cabinet. In addition to the ethnic background of a country's leading politicians, specific institutional arrangements, such as different types of power-sharing and consociationalism, may also be indicators of power inclusion. Mere regional autonomy without significant input into cabinet-level governmental decision-making, on the other hand, is clearly *not* sufficient to warrant status as *EGIP*.

Assuming that only dyadic conflict between the *EGIP* and the respective peripheral groups can happen, it is possible to compute the probability of civil war as

$$\Pr(CivilConflict) = 1 - \prod_{i=1}^{n-1} \left(1 - p(i)\right)$$

where $p(i)$ is the probability of dyadic conflict erupting between the *EGIP* and the marginalized group *i*.

Furthermore, we postulate that conflict happens if the power balance tips in favor of the peripheral group in question. Using relative demographic group sizes as a proxy for power, we postulate that the probability of conflict in the dyad involving the *EGIP* and group *i* can be written as

$$p(i) = \frac{1}{1 + \{r(i)/r\}^{-k}}$$

where $r(i) = s_i / (s_i + s_0)$ is group *i*'s share of the total dyadic population, *r* is a threshold value and *k* a slope parameter. We use $r = 0.5$ and $k = 5$ for convenience.

Using Fearon and Laitin's (2003) well-known insurgency model as an empirical reference point, we compare our new measure *N** to conventional indicators such as the ethno-linguistic fractionalization index. Because of coding limitations, we focus on a subset of their global dataset, namely Eurasia and North Africa. Although strong results for our measure can be derived for civil wars in general, as expected *N** performs especially well when applied to ethnic civil wars (for the definition, see Fearon and Laitin 2003). We will therefore focus on that dependent variable in this paper.

Table 1 presents the key results already obtained by Cederman and Girardin (2005). Displayed for replication purposes, Model 1 confirms that the *ELF* is insignificant for the subset of cases considered here. Model 2 introduces the *N** measure, which is strongly significant ($p = 0.001$).

[Table 1 about here]

These results cast doubt on the political economists' reluctance to include ethnic politics in their opportunity-driven models of civil wars.

So far, our analysis has adopted Fearon and Laitin's pooled time-series design. In order to test the robustness of *N**, and to develop a better intuition for where it performs well and where it does not, we collapse the panel design to a purely cross-sectional one. In this formulation, the dichotomous dependent variable is set to one if the country experienced at least one ethnic war during the entire sample period and zero otherwise. All independent variables are held at their arithmetic means. We now have merely 85 country cases.[1] Because our model is static, it makes sense to drop the variables indicating lagged ongoing wars and new states.

Table 2 presents the results of our cross-sectional models. We begin by comparing the performance of *ELF* and $N^*$. Models 3 and 4 show that, as in the panel design of Table 1, only $N^*$ reach significance ($p = 0.011$). Thus it can be concluded that in the cross-sectional setup, $N^*$ remains the best measure of ethnic conflict-propensity. As in Fearon and Laitin (2003), however, the GDP variable is the one that is the most significant in Model 4. Otherwise, the effects of the population, oil and democracy variables are also statistically confirmed.

[Table 2 about here]

We proceed by dichotomizing the $N^*$ index by coding all cases for which $N^* > 0.0005$ as one and all other cases as zero.[2] In the static regression framework, this makes sense because even small values of the annually computed $N^*$ would lead to considerable probabilities when repeated over the entire sample period. We can therefore expect a jump in the accumulated effect of $N^*$ between zero and small positive values. The findings of Model 5 indicate that this is a more accurate way to model opportunity structures in the cross-sectional version of the model. In this case, the binary version of $N^*$, here labeled $bN^*$, becomes much more significant ($p = 0.006$), surpassing all other variables in terms of significance. In fact, GDP and democracy become insignificant. The only other variable that remains significant is the population indicator.

Dichotomization has the additional advantage of allowing us to generate a compact table that illustrates the model's performance in individual cases. By cross-tabulating the binary $N^*$ variable against the dependent variable, Table 4 reveals a strong bivariate association between the two variables ($\chi^2 = 10.88$, $p = 0.001$).[3] The upper row of the table contains the cases for which $bN^*$ is zero, and the lower row those for which it is greater than zero. The columns hold the values of the dependent variable, with the left column depicting the peaceful observations and right column the violent ones. Given this layout, the correctly explained observations can be found in the upper left and lower right-hand quadrants. Whereas the former cell, which displays the "correct negatives," contains a plurality of the cases (N = 41), the corresponding number of "correct positives" is 17. Unsurprisingly, the list of correct negatives features many stable and wealthy Western countries. We also see that based on $bN^*$ it becomes possible to place a number of conflict-ridden countries in the correct lower right-hand quadrant of the table.

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[1] It should be recalled that due to severe coding complications, $N^*$ has so far only been coded for Eurasia and North Africa (Cederman and Girardin 2005). However, efforts to extend data collection to the rest of the world are already underway.

[2] The initial value of the cutoff point was defined by the lower numeric precision of preliminary computation in Xlisp-Stat. However,we conducted robustness tests to explore the sensitivity of this threshold. Other values, such as 0.01, 0.001 and 0.0001 were also tried but yielded roughly the same results, although in some cases with somewhat reduced significance.

[3] The strength of the association varies with the exact level of the cutoff. For 0.01 it is merely $\chi^2 = 4.31$ at $p = 0.043$ and for 0.0001it amounts to $\chi^2 = 8.11$ at $p = 0.004$.

[Table 3 about here]

The problematic cases can be found "off the diagonal." This is where *N** fails to make the right prediction. It turns out that our dichotomous version of *N** misses relatively few real cases: only six of the 85 country cases turned out to be false negatives.[4] Because of its permissive logic, however, *N** tends to over-predict conflict outcomes. Indeed, the lower left-hand quadrant of Table 4 contains as many as 21 false positives.

**Measuring grievance**

Is it possible to render the *N** approach more discriminate? Simply put, we would like to create a model that refines the existing index by throwing out as many of the false positives as possible, while minimizing the number of new false positives. This seems straight-forward enough, but we are left with the theoretical question of what will do the trick. How can the wheat be separated from the chaff?

Simply perusing the list of false positives does not suggest any obvious answers. Nevertheless, it is striking that as many as a third of these cases (6 out of 21) belong to the former Soviet sphere of influence. Georgia and Moldova (not counting Afghanistan) were the only correctly predicted sites of conflict in this sphere. It is true that most of these states' lifetimes are limited to the post-Cold War period, which reduces opportunities for conflict, but many experts expected the "near abroad" to produce much more violence than it actually did (e.g. Goble 1993). This may reflect the efforts by the European Union, which has successfully pressured many of these states to elect increasingly benign policies toward their Russian minorities (King and Melvin 1999/2000).

Furthermore, the list of true positives contains a relatively large number of cases that have been characterized as "sons of the soil" (Weiner 1978). Using Fearon's (2004) coding, we identify six out of the 18 cases in this category. In fact, within our Eurasian and North African sample, two of the cases so coded were missed in our tabulation (i.e. the Philippines and Bangladesh).[5] According to Weiner's (1978) classic treatise, the sons-of-the soil conflict pattern captures situations in which members belonging to the *EGIP* settle in the periphery of the country in question because of state inducements or other economic opportunities. This migratory pressure triggers nationalist mobilization among the ethnically distinct local populations, i.e. the "sons of the soil." This dynamic can be exacerbated by the government policies that favor the settler population. Such policies are often perceived as a threat to the identity of the newly mobilized peripheral group, with center-periphery conflict as a likely consequence.[6]

[4] This is not surprising given the large size of China and Russia and the small size of the contending groups in question. Fearon's group list does not even include an entry for Chechnya due to its lower bound of group size.
[5] Depending on the precision used for dichotomization of *N** Bangladesh can also be counted as a one.
[6] Fearon (2004) suggests that such bitter struggles are likely to last for a very long time.

So far, however, our observations are not based on any systematic theory that can cover a more general set of cases. To make theoretical progress, it is necessary to consider the common theme in these subsets of cases, namely grievances resulting from discrimination (Gurr 2000, p. 71). Ethnic groups can suffer social, cultural and economic disadvantages because of uncoordinated prejudices, but here we will be concerned with state-coordinated political discrimination.

According to Wimmer's (2002, p. 91) account, ethno-nationalist mobilization is driven by the politicization of ethnic differences. Once the state ceases to be an even-handed, culturally neutral institution, it by definition fails to be representative of the entire population. If the access to public benefits and the extraction of resources follow ethnic lines, identity formation and mobilization are more likely within the disadvantaged groups. Disaffected elites belonging to the ethnic minorities will be more successful in recruiting grass roots support within their own groups. Thus, the key factor driving this struggle over state resources is political discrimination orchestrated by powerful state actors.

It thus makes sense to focus on political discrimination. Although the notion of *EGIP* already captures some aspects of political exclusion by singling out a group (or a coalition of groups) as in charge of the central government, this is a very crude distinction that fails to do justice to the full spectrum of discriminatory policies that can trigger grievances among the peripheral groups. Moreover, the micro-level mechanism underpinning $N^*$ primarily considers the peripheral groups' opportunities to rebel, although it does not exclude preemptive action by the *EGIP*. However, the discrimination mechanism flows directly from the governments own actions, thus complementing the periphery-driven logic of $N^*$.

In other words, in order to generate a more nuanced story of where conflict can be expected to erupt, we need to measure grievance.

Fortunately, the Minorities at Risk (*MAR*) dataset provides the missing pieces of the empirical jigsaw by offering a measure of political discrimination that targets each minority group coded therein (Davenport 2005). The indicator ranges from zero to four. We chose to dichotomize each annual group-based reading by coding values above two as a one.[7] Because our research design is cross-sectional and state-centric, it makes sense to create an aggregate measure of political discrimination for each state in the sample. This can be readily done by accumulating the number of "discrimination group-years," a quantity that is then normalized by the number of groups in the country.[8] Thus, our aggregate measure of political discrimination in country $i$, $PD_i$, can be written:

[7] According to the code book (Davenport 2005, pp. 98-99), the third step of the variable POLDIS corresponds to "substantial underrepresentation due to prevailing social practices by dominant groups" and the fourth step describes situations in which "public policies substantially restrict the group's political participation by comparison with other groups."

[8] Care has to be taken not to include the *EGIP*s in this list, because the MAR group list of "minorities" in some instances includes groups that we code as *EGIP*s. Obviously, we are only interested in discrimination against peripheral groups. It should also be noted that the MAR group list differs slightly from the group list used to compute $N^*$, which is based on Fearon's (2003) definitions.

$$PD_i = \frac{\sum_{g=1}^{n_i}\sum_{y} d_{gy}}{n_i}$$

where $d_{gy}$ is the dichotomized discrimination score for each group $g$ and year $y$, and $n_i$ is the number of groups in state $i$. We choose not to normalize by the number of years of discrimination, because it can be expected that longer periods of discrimination lead to higher levels of grievance.

The histogram in Figure 1 presents the distribution of the cases, which is very skewed toward low levels of discrimination. In fact, the median is as low as twelve discrimination years, with almost half of the sample exhibiting no such policies at all. However, the picture is very different for the remaining half of the sample.[9]

[Figure 1 about here]

**Modeling opportunity and grievance as explanations of ethno-nationalist wars**

We are now ready to put the pieces together. In the previous section we argued that including grievance helps us to craft more discriminate explanations of internal conflict. This argument differs fundamentally from the way that these two factors have usually been treated in the political economy literature. In their eagerness to show that opportunity rather than motivation drives violence, these authors typically compare their relative effects as additive terms (e.g. Fearon and Laitin 2003) or in alternative regression models (e.g. Collier and Hoeffler 2004).

As indicated above, however, we consider the assumption of explanatory priority to be fundamentally flawed (regardless of whether opportunities or grievances are privileged). Drawing on Elster's (1984) "filter explanations," it makes more sense to view both factors as necessary conditions that must both be fulfilled in order for the violent outcome to materialize. In brief, actors need to possess both the opportunity and the willingness to act (Starr 1978). More precisely, civil wars occur when peripheral contenders to the state are powerful enough to challenge the center, and sufficiently motivated to do so (Gurr 2000). However, our research design does not allow us to judge whether one factor precedes the other, as argued by Regan and Norton (2005), who claim that grievance is the more basic condition.

How can this logic be formally modeled? Rather than introducing both explanatory factors as additive terms into our regression, we propose a simple multiplicative

[9] It should be noted that some of the coding in *MAR* seems at odds with our theoretical purpose. For example, several western democracies score very high on the discrimination measure. Moreover, Gurr and his colleagues count "foreign workers" as a "minority at risk", which is subject to the highest level of political discrimination. Incidentally, one of the authors belongs to this category. However, such cases do not matter for our refined $N^*$ index below, because the basic $N^*$ measure amounts to zero in most developed democracies.

functional form that relies on an interactive term that combines our $N^*$ index with the grievance indicator developed in the previous section.[10] The result could be called political-discrimination $N^*$, or $PDN^*$ for short, which is defined in the following manner:

$$PDN^* = N^* \times PD$$

Let us now consider the findings of our refined model. Table 4 presents the results from three models. Model 6 confirms that political discrimination, on its own, does not appear to have any effect on civil war outcomes. The coefficient of *PD* is positive, but not even nearly significant. However, the situation is very different once we introduce $DN^*$. The findings of Model 7 reveal that the combined indicator performs very well compared to $N^*$ alone as reported in Model 4. Not only is the effect positive, as expected, but the level of significance improves considerably (from $p = 0.011$ to 0.005), thus suggesting that the new model improves on the simpler version. In fact, together with the *GDP* and population variables, the $DN^*$ measure is among the most significant explanatory variables of Model 4.

[Table 4 about here]

In keeping with the logic of Table 2, we also consider a dichotomized version of our combined index. This measure, which we call $bPDN^*$, is defined as

$$bPDN^* = \begin{cases} 1 & \text{if } N^* > 0.0005 \text{ and } PD > 16.6 \\ 0 & \text{otherwise} \end{cases}$$

where 16.6 corresponds to the mean of *PD*. As was the case with $bN^*$, this definition produces strong results.[11] According to Model 8, the combined measure is highly significant ($p = 0.001$), which represents a considerably improvement over the corresponding level for $bN^*$ ($p = 0.006$) as reported in Model 5.

It is interesting to study the cross-tabulated version of these findings. Following the logic of Table 3, Table 5 tabulates the dichotomized explanatory variable *bPDN* against the dependent variable for ethnic onset. It is clear that the refined measure leads to more accurate predictions. Because the refined measure is more conservative than the basic $N^*$ version, a number of countries move from the lower to the upper row (see the cases in boldface). This adjustment drastically shortens the list of false positives from 21 down to 6. The improvement has a flipside, however, since the number of false negatives increases somewhat from 6 to 10. All in all, these changes are reflected in an improved $\chi^2 = 21.21$ over the previous model's 10.88.

As expected, the refined measure manages to eliminate a number of false post-Soviet conflict predictions, including the Czech Republic, Estonia, Kyrgyzstan, Latvia,

[10] See Gurr (1970) for a model based on a similar multiplicative functional form.

[11] Obviously, these results depend on the exact level of these two thresholds. Sensitivity analysis shows that the combined indicator $bPDN^*$ improves on the performance of $bN^*$ for all of the other cutoff values of the latter dummy variable that were tried above, although the significance decreased in some cases.

Slovakia, Tajikistan and Turkmenistan. This is a reasonable finding because, as we have already noted, these countries have typically exhibited declining rates of discrimination, especially in those cases that can be considered as likely future members of the European Union. This is the main reason why the number of false positives could be reduced.

[Table 5 about here]

By the same token, there is a higher number of false negatives due to the more selective nature of our indicator. Again, two of these are Georgia and Moldova, where the political discrimination score lies below the mean of the political discrimination index *PD*. The same situation applies to Cyprus and Morocco.

Before drawing any further conclusions about our findings, we need to consider two methodological objections. It could be argued that the presence of an interaction term in Models 6 and 7 causes them to be miss-specified. In an important article, Braumoeller (2004) demonstrates that the conventional interpretation of interaction terms rests on flawed foundations especially when lower-order terms are not included in the regression model in question. We agree that this would be a problem if our theory had assumed that lower-order terms have an independent effect. However, this is not case because *DN** is an integrated index its *ELF* that constitutes a refinement of *N**, and should therefore replace the simpler measure.[12]

Furthermore, because of the static research design, it is necessary to consider the possibility that causation may be reversed between conflict and political discrimination. So far, we have assumed that the central governments' discriminatory policies trigger grievances that feed into peripheral mobilization processes. However, it is also possible that discrimination is a consequence of conflict. Based on the highly aggregated measure of *PD*, which covers the entire sample period, it is impossible to scrutinize the temporal precedence of these two factors.

To find out what causes what, we created a rough annual measure of discrimination for each state $i$ and year $y$, averaging the discrimination indicator $d_{gy}$ over all group-specific observations for the year in question. This measure was plotted as a time series, with each ethnic-conflict year highlighted (not included in this paper). In most cases, each conflict period was preceded by considerable discrimination that did not seem to change notably after the onset of conflict. Russia is an exception, because here discrimination appears to reflect the ongoing Chechen conflict. The Philippines also exhibits a time series in which reverse causation cannot be excluded. In some additional cases, it was impossible to resolve the issue of causal precedence, either because of ongoing conflict from the very beginning of the sample period or from the first year of independence. In addition, a two-sample means test of political discrimination for the country-years with and without conflict did not allow us to reject the hypothesis that the means are the same ($p = 0.149$).

[12] Regressions with both lower-order terms, *N** and *PD*, as well as the interaction term *PDN**, show that all factors are insignificant, suggesting that there is little support for such a specification. The low significance of *PDN** in this model can be explained by its high correlation with *N** ($r = 0.770$). It therefore makes sense to drop all lower-order terms.

Still, it is impossible to deny need for spatial and temporal disaggregation. For example, it would be instructive to align dyadic discrimination stories with particular group-specific conflict patterns.[13] As a first cut, however, we believe that the current results represent a significant advance over other measures that rely on either grievance or opportunity alone.

## Conclusion

In this essay, we have followed up our previous work that targets the assumption of material opportunism as an explanation of civil-war onset. Building on Cederman and Girardin (2005), we have extended our previous opportunity-based *N** index by incorporating a measurement of grievance operationalized as a response to governmental discrimination. This extension contradicts the common assumption of causal precedence in favor of opportunities or grievances. We claim that viable explanations of ethno-nationalist civil wars have to combine both types of explanations in an integrated model that treats them as necessary conditions rather than as additive, causal factors.

Our refined index *PDN** performs considerably better than the "raw" *N** version, which already easily outperformed popular measures such as the ethno-linguistic fractionalization index. In fact, if formulated as multiplicative measure, ethnic configurations and ethno-nationalist mobilization mechanisms driven together by political discrimination together appear to have a strong impact on the outbreak of political violence. Based on this refined measure, we are able to improve the econometric performance of our models quite dramatically. Moreover, with a simple, highly aggregated cross-sectional research design, we are able to discern important trends that could serve as inspiration for more context-sensitive modeling of the micro-level mechanisms in question. This task, however, will have to be left for future research.

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[13] Such a dyadic research design would require extensive recoding, because we are currently relying on Fearon's group list for the calculation of *N** and on the MAR listings of ethnic minority groups to measure political discrimination.

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Table 1. Logit analysis of determinants of “ethnic” civil war onset, 1945-1999

| | Model 1 | | | Model 2 | | |
|---|---|---|---|---|---|---|
| | Coeff. | Std Err. | Signif. | Coeff. | Std Err. | Signif. |
| Prior war | -1.103 | 0.508 | 0.030* | -1.091 | 0.487 | 0.025* |
| GDP | -0.216 | 0.088 | 0.014* | -0.255 | 0.091 | 0.005** |
| Population | 0.554 | 0.13 | 0.000*** | 0.658 | 0.132 | 0.000*** |
| Mountains | 0.223 | 0.189 | 0.236 | 0.244 | 0.187 | 0.192 |
| Noncontiguous | 0.001 | 0.429 | 0.998 | 0.27 | 0.42 | 0.521 |
| Oil exporter | 1.475 | 0.441 | 0.001*** | 1.43 | 0.444 | 0.001** |
| New state | 2.24 | 0.549 | 0.000*** | 2.368 | 0.558 | 0.000*** |
| Instability | 0.35 | 0.455 | 0.442 | 0.488 | 0.462 | 0.291 |
| Democracy | 0.056 | 0.03 | 0.061 | 0.066 | 0.031 | 0.033* |
| ELF | 0.889 | 0.749 | 0.235 | | | |
| N* | | | | 2.609 | 0.793 | 0.001** |
| Constant | -10.685 | 1.476 | 0.000*** | -11.681 | 1.548 | 0.000*** |
| N | 3327 | | | 3327 | | |

*) p<0.05; **) p<0.01; ***) p<0.001

Table 2. Cross-sectional logit analysis of opportunities for “ethnic” civil war onset, 1945-1999

| | Model 3 | | | Model 4 | | | Model 5 | | |
|---|---|---|---|---|---|---|---|---|---|
| | Coeff. | Std Err. | Signif. | Coeff. | Std Err. | Signif. | Coeff. | Std Err. | Signif. |
| GDP | -0.376 | 0.196 | 0.055 | -0.567 | 0.226 | 0.012* | -0.33 | 0.219 | 0.132 |
| Population | 0.602 | 0.26 | 0.021* | 0.916 | 0.313 | 0.003** | 0.855 | 0.304 | 0.005** |
| Mountains | 0.215 | 0.258 | 0.404 | 0.167 | 0.285 | 0.558 | -0.066 | 0.303 | 0.828 |
| Noncontiguous | -0.224 | 0.925 | 0.809 | 0.049 | 0.96 | 0.959 | 1.113 | 1.155 | 0.335 |
| Oil exporter | 1.918 | 1.051 | 0.068 | 2.086 | 1.221 | 0.088 | 1.04 | 1.083 | 0.337 |
| Instability | 1.083 | 1.798 | 0.547 | 0.884 | 1.975 | 0.654 | 1.333 | 2.018 | 0.509 |
| Democracy | 0.131 | 0.073 | 0.073 | 0.197 | 0.085 | 0.020* | 0.062 | 0.08 | 0.438 |
| ELF | 2.102 | 1.411 | 0.136 | | | | | | |
| N* | | | | 7.779 | 3.056 | 0.011* | | | |
| bN* | | | | | | | 2.801 | 1.019 | 0.006** |
| Constant | -7.076 | 2.79 | 0.011* | -9.141 | 3.161 | 0.004** | -9.898 | 3.359 | 0.003** |
| N | 85 | | | 85 | | | 85 | | |

*) p<0.05; **) p<0.01; ***) p<0.001

Table 3. Table of correct and false predictions based on *N**

| | |
|---|---|
| N* close to zero & no ethnic war (correct negatives)<br>Albania, Armenia, Austria, Belarus, Belgium, Bulgaria, Cambodia, Czechoslovakia, Denmark, Egypt, Finland, France, Germany, Germany (GDR), Greece, Hungary, Ireland, Italy, Japan, Korea (South), Libya, Lithuania, Mongolia, Netherlands, North Korea, Norway, Poland, Portugal, Romania, Saudi Arabia, Spain, Sweden, Switzerland, Tunisia, Ukraine, Uzbekistan, Vietnam, Vietnam (South), Yemen, Yemen (Arab Rep.), Yemen (People's Rep.). N = 41 | N* close to zero & ethnic war (false negatives)<br>Azerbaijan, Bangladesh, China, Philippines, Russia, UK. N = 6 |
| N* above zero & no ethnic war (false positives)<br>Bahrain, Bhutan, Czech Republic, Estonia, Israel, Kazakhstan, Kuwait, Kyrgyzstan, Laos, Latvia, Malaysia, Nepal, Oman, Singapore, Slovakia, Syria, Taiwan, Tajikistan, Thailand, Turkmenistan, United Arab Emirates. N = 21 | N* above zero & ethnic war (correct positives)<br>Afghanistan, Algeria, Burma, Cyprus, Georgia, India, Indonesia, Iran, Iraq, Jordan, Lebanon, Moldova, Morocco, Pakistan, Sri Lanka, Turkey, Yugoslavia. N = 17 |

Table 4. Cross-sectional logit analysis of opportunities and grievances driving "ethnic" civil wars, 1945-1999

| | Model 6 | | | Model 7 | | | Model 8 | | |
|---|---|---|---|---|---|---|---|---|---|
| | Coeff. | Std Err. | Signif. | Coeff. | Std Err. | Signif. | Coeff. | Std Err. | Signif. |
| GDP | -0.458 | 0.189 | 0.016* | -0.571 | 0.225 | 0.011* | -0.495 | 0.237 | 0.037* |
| Population | 0.532 | 0.265 | 0.045* | 0.974 | 0.322 | 0.003** | 0.717 | 0.289 | 0.013* |
| Mountains | 0.287 | 0.261 | 0.272 | 0.16 | 0.292 | 0.583 | 0.067 | 0.299 | 0.824 |
| Noncontiguous | -0.181 | 0.893 | 0.839 | -0.122 | 0.966 | 0.9 | 0.169 | 1.049 | 0.872 |
| Oil exporter | 2.139 | 1.016 | 0.035* | 2.426 | 1.181 | 0.040* | 2.014 | 1.187 | 0.09 |
| Instability | 0.806 | 1.796 | 0.654 | 0.998 | 1.997 | 0.617 | 1.212 | 2.051 | 0.554 |
| Democracy | 0.143 | 0.073 | 0.050* | 0.214 | 0.087 | 0.014* | 0.159 | 0.086 | 0.064 |
| PD | 0.021 | 0.018 | 0.24 | | | | | | |
| PDN* | | | | 0.276 | 0.098 | 0.005** | | | |
| bPDN* | | | | | | | 3.088 | 0.908 | 0.001*** |
| Constant | -5.932 | 2.755 | 0.031* | -9.66 | 3.251 | 0.003** | -7.521 | 3.088 | 0.015* |
| N | 85 | | | 85 | | | 85 | | |

*) p<0.05; **) p<0.01; ***) p<0.001

Table 5. Table of correct and false predictions based on *bPDN**

| | |
|---|---|
| *bPDN** close to zero & no ethnic war<br>(correct negatives)<br>Albania, Armenia, Austria, Belarus, Belgium, Bulgaria, Cambodia, Czechoslovakia, **Czech Rep**., Denmark, Egypt, **Estonia**, Finland, France, Germany, Germany (GDR), Greece, Hungary, Ireland, Italy, Japan, Kazakhstan, Korea (South), Kuwait, **Kyrgyzstan**, **Laos**, **Latvia**, Libya, Lithuania, Mongolia, **Nepal**, Netherlands, North Korea, Norway, **Oman**, Poland, Portugal, Romania, Saudi Arabia, **Slovakia**, Spain, Sweden, Switzerland, **Syria**, **Tajikistan**, **Thailand**, Tunisia, **Turkmenistan**, **United Arab Emirates**, Ukraine, Uzbekistan, Vietnam, Vietnam (South), Yemen, Yemen (Arab Rep.), Yemen (People's Rep.). N = 56 | *bPDN** close to zero and ethnic war<br>(false negatives)<br>Azerbaijan, Bangladesh, China, **Cyprus**, **Georgia**, **Moldova**, **Morocco**, Philippines, Russia, UK.<br>N = 10 |
| *bPDN** above zero & no ethnic war<br>(false positives)<br>Bahrain, Bhutan, Israel, Malaysia, Singapore, Slovakia, Taiwan. N = 6 | *bPDN** above zero & ethnic war<br>(correct positives)<br>Afghanistan, Algeria, Burma, India, Indonesia, Iran, Iraq, Jordan, Lebanon, Pakistan, Sri Lanka, Turkey, Yugoslavia. N = 13 |

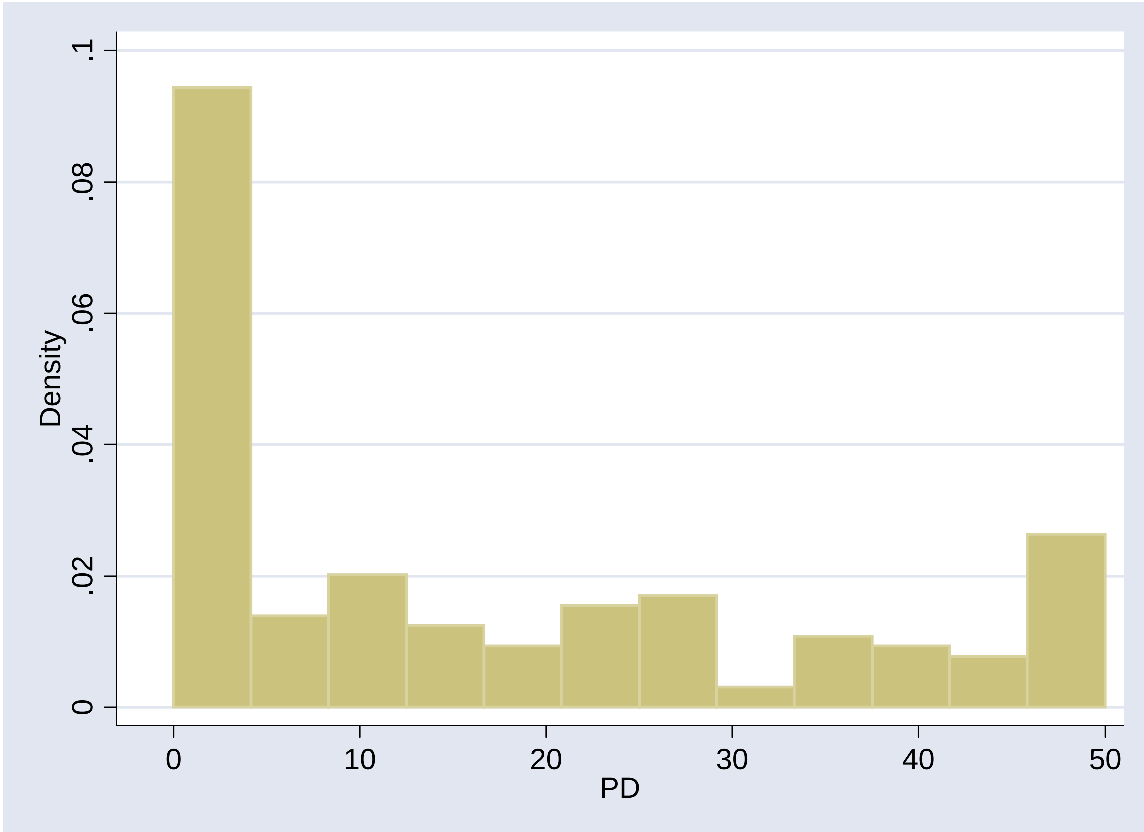


Figure 1. The distribution of values for the political discrimination indicator *PD*.