

Beyond Fractionalization:

Mapping Ethnicity onto Nationalist Insurgencies*

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Comments welcome!

Abstract:

This paper theorizes the link between ethnicity and conflict. Conventional research relies on the ethnic-linguistic fractionalization index (*ELF*) to explore a possible causal connection between these two phenomena. However, such approaches implicitly postulate unrealistic, individualist interaction topologies. Moreover, *ELF*-based studies fail to articulate explicit causal mechanisms of collective action. In order to overcome these difficulties, we introduce the new index N^* of ethno-nationalist mobilization that relies on simple mechanisms that map ethnic configurations onto political violence. This formalization is confirmed statistically in regression analysis based on data from Eurasia and North Africa.

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Toward the end of the Cold War, a wave of scholarship emerged that associated internal conflict with ethnic factors. Ethnic conflict in the former Yugoslavia and in Rwanda lent such theories considerable credibility. More recently, however, a number of political economists have challenged this causal link (Fearon and Latin 2003; Collier and Hoeffler 2004). Their application of econometric methods suggests that cultural factors wash away once materialist factors, such as per-capita income, access to raw materials, and the like, are controlled for.

While this new political economy literature has rejuvenated the field of civil war studies, it has introduced a new set of problems. Most importantly, cross-national statistics offer little guidance when it comes to the validation of causal mechanisms at the micro level. Statistical evaluation of culture-related theories suffers from precisely this difficulty. Almost without exception, quantitative assessments of ethnic and nationalist influences on internal conflict rely on various versions of the ethno-linguistic fractionalization index (*ELF*).

Ironically enough for a literature that purports to uncover the micro-foundations of politics, political economists have yet to come up with a convincing set of causal mechanisms for political violence. Before following these scholars' advice to reject culture as a determinant of civil war onset, it is therefore reasonable to question whether they are actually testing any meaningful theory of ethnicity or nationalism.

In this paper, we argue that ethno-nationalist mobilization does generate violence in civil wars. In order to back up this claim, we offer an alternative index of nationalist insurgencies called N^* , which does a better job of capturing mainstream theories of nationalist violence. It deviates from standard fractionalization and polarization measures by introducing state-centric, rather than symmetric, ethnic configurations and group-level, rather than individual-level micro mechanisms of mobilization.

Using Fearon and Laitin's (2003) well-known insurgency model as an empirical reference point, we compare our new measure N^* to conventional indicators. Because of coding limitations, we focus on a subset of their global dataset, namely Eurasia and North Africa. The results are very encouraging: for the sub-sample in question, N^* is highly significant, thus casting doubt on the political economists' rejection of nationalism as an explanation of civil wars.

In the following, we introduce conventional indices of fractionalization and polarization and attempt to derive causal mechanisms compatible with them. Building on theories of nationalism, we then return to first principles in order to lay the foundations of our alternative measure. The N^* index is defined and empirically deployed for the Eurasian cases, before being put to use in Fearon and Laitin's model. The essay ends with a discussion of the theoretical significance of our findings.

Conventional fractionalization and polarization indices of ethnicity

In contrast to qualitative studies, econometrics allows us to draw systematic and precise inferences about a large number of cases, provided the underlying causal "story" remains stable throughout the population. However, this does not mean that there is no room for debate, even when highly significant findings are present. Apart from thorny issues of model specification and estimation, perhaps the most tricky issue is how to map micro-level mechanisms onto macro-level behavior. It is well known that human intuition is notoriously incapable of overcoming this obstacle (Schelling 1978). According to Sambanis (2004a, p. 259) this problem applies acutely to civil-war studies:

The already significant gap between micro-level behavior and their macro-level explanation is magnified when...micro-macro relationships are studied solely through cross-national statistical analyses. Such studies often overlook

information about causal pathways that link individual or group behavior with the outbreak of civil war.

For example, virtually all scholars agree that GDP per capita is a strong determinant of the onset of internal conflict, but they cannot agree on what generates this regularity. Whereas Collier and Hoeffler (2004) contend that it reflects the lower opportunity costs for individuals to engage in violence as GDP decreases, Fearon and Laitin (2003) propose a center-periphery model, where income is interpreted as a proxy of state strength. It seems unlikely that such disputes will ever be resolved without disaggregation of the empirically relevant causal logic. In this section, we show that there is a similar mismatch between micro-level mechanisms and macro-level measures with respect to cultural hypotheses of civil war.¹

Virtually all studies of the cultural determinants of civil war rely on some type of fractionalization index. We refer to this family of indices under the acronym of *ELF*, which stands for the index of Ethno-Linguistic Fractionalization. This index was first operationalized by Soviet scholars (*Atlas Narodov Mira* 1964) and was later popularized by Taylor and Hudson (1972). However, there are many versions based on various coding criteria of how to define the relevant ethnic, linguistic or religious groups in the first place (Alesina et al. 2003; Fearon 2003; Roeder 2001).²

In all its guises, the *ELF* is based on the Herfindahl concentration formula:

$$ELF = 1 - \sum_{i=1}^n s_i^2$$

where s_i is the share of group i out of a total of n groups.

The logic behind this expression is well-known and extremely simple, because it measures the probability that two randomly selected individuals from the entire population will be from different groups.

Despite its widespread use, the *ELF* has attracted considerable criticism. Drawing on constructivist assumptions, Latin and Posner (2001) point out that attributing a fixed score to each state obscures the degree to which ethnic identities vary over time. They also note that a single index is under-equipped to capture the multidimensional quality of ethnic identities and the endogenous effects that allow the dependent variable to influence the index over time.

In a sophisticated applied analysis of the *ELF* that focuses entirely on Sub-Saharan Africa Posner (2004) further elaborates on their fourth and final critique. As an alternative, Posner introduces a version of *ELF* that he calls *PREG* or “politically relevant ethnic groups.” Posner’s idea is to relate the identification of ethnic groups to the political logic under scrutiny, which in his case is economic growth. Based on extensive area expertise and thorough coding, Posner and his collaborators are able to provide *PREG* measures at ten-year intervals. Although the introduction of *PREG* represents considerable empirical and conceptual progress, Posner admits that it still afflicted by the same theoretical weaknesses as the other *ELF* indices, since it also relies on the Herfindahl logic.

In order to see what these difficulties entail for civil war studies, it is helpful to reconstruct an explicit causal theory of conflict that is compatible with the *ELF* logic.

¹ The quantitative literature is less uniformly dismissive of ethnic factors than one would infer from reading Fearon and Laitin (2003) and Collier and Hoeffler (2004). By disaggregating the dependent variable into cases of identity and non-identity wars, Sambanis (2001) finds a positive impact of ethnic diversity on civil war onset. For a survey of other, mixed results, see Sambanis (2002).

² Another indicators of ethnicity focus on ethnic dominance and culture distance (see Fearon 2003).

This calls for the identification of a micro-level mechanism of conflict M and of an ethnic configuration E that maps M onto the systemic probability of conflict:

$$\Pr(\text{CivilViolence}) = E(M)$$

Given this formalism, we can now derive a very simple individualist theory of conflict that is compatible with the ELF :

M : If two members i and j interact, the probability of conflict is p_1 if they belong to different groups and $p_0 < p_1$ if they belong to the same group.

E : All pairs of individuals (i,j) in the population have an equal chance of interacting with each other.

Hence the probability of conflict is:

$$\Pr(\text{CivilViolence}) = p_1 ELF + p_0(1 - ELF) = (p_1 - p_0)ELF + p_0$$

where $(p_1 - p_0) > 0$ by assumption M . Thus, the conflict propensity is an increasing linear function in ELF , which means that regression analysis should be well suited to capture the effect of ethnic fractionalization on conflict.

Some reflection suggests why the ELF model of civil wars is inappropriate. It appears to work much better for ethnic riots or brawls that occur between randomly interacting individuals (Tilly 2003). The problem is that full-fledged ethno-nationalist warfare, whether within or among states, presupposes that violence is organized rather than random and that it is not generated in a completely bottom-up fashion. Our causal reconstruction says nothing about how a dyadic conflict at the individual level can escalate to a full-fledged civil war. Moreover, this account is silent on the role of the state, which is paramount in all civil wars by definition (Sambanis 2004b).

Nevertheless, the ELF is not the only index of ethnic composition used in the political economy literature. Reynal-Querol (2002) proposes an alternative to the ELF , namely the RQ index, which belongs to an entire family of discrete polarization measures.

$$RQ = 1 - \frac{1}{4} \sum_{i=1}^n (1/2 - s_i)^2 s_i$$

This measure differs from the ELF in that it scores higher for polarized populations. For example, it ranks the group distribution $\{0.5, 0.49, 0.01\}$ higher than $\{0.33, 0.33, 0.34\}$.

In contrast to the authors that rely on the ELF , Garcia-Montalvo and Reynal-Querol (2002) make an effort to explore the theoretical foundations of their index. Based on a model of rent-seeking, they posit that the total resources spent on lobbying purposes can be interpreted as an index of potential conflict.

Despite their efforts, this link seems tenuous at best, especially because it has only been shown to hold for equally sized groups. In particular, the reference to civil-war violence remains obscure. All that the RQ index does is to replace the mechanism M in the ELF model of conflict with a different functional form, while retaining the same ethnic configuration E . Consequently, the logic is still fundamentally individualist, which means that the RQ offers little more explanatory leverage than the ELF .

A theory of ethno-nationalist civil wars

In view of the difficulties encountered in the previous section, it would seem more promising to start our search for more adequate measures of politicized culture by revisiting first principles. We therefore begin by reconsidering how culture-based theories explain political violence. Most importantly, what are usually referred to as ethnic civil wars are actually manifestations of nationalist conflicts that are carried out along ethnic lines. Theories of nationalism tell us that ethno-nationalist wars, rather than being merely “horizontal” fights among ethnic groups after the collapse of state authority, are actually fought with the help of, and indeed over, state power. Thus, it is high time to bring the state back in!

The conceptual chaos that surrounds terms such as the state, nationalism, and ethnicity may explain why the state has slipped out of the picture in the aforementioned tests of culture-based theories of civil wars. It is therefore necessary to clarify the key concepts beginning with Max Weber’s (1946) classical definitions (see Cederman 2002). First, the *state* can be seen as a territorial organization that exercises legitimate control over its own bounded territory. Second, Weber defines the *nation* as “a community of sentiment which would adequately manifest itself in a state of its own” and which therefore “tends to produce a state of its own.” Third, an *ethnic group*, according to Weber, is a cultural community based on a common belief in a real or putative descent.

Given these actor types, we follow Gellner (1983) in defining nationalism as “primarily a political doctrine, which holds that the political and the national unit should be congruent” (p. 1). Wherever this principle is violated, i.e. where “home rule” for each population has not been established, tensions arise in response to the perceived incongruence. Such tensions sooner or later lead to nationalist mobilization, which may in turn provoke a realignment of previous political coalitions. Furthermore, the result of such a process may entail assimilation, migration, and major reconfigurations of the geopolitical map, including secession, unification and irredentism. These processes do not always unleash considerable amounts of political violence, but they have considerable potential to do so.

This is the starting point of the best contemporary sociological explanations of ethno-nationalist violence. Building explicitly on Gellner’s conceptual legacy, Wimmer (2002) and Mann (2005) interpret such conflict patterns as integral parts of political mobilization under modernity. Starting around the time of the French Revolution, this macro-historical process swept through the modernizing world, spilling over into the non-Western world in the 20th century. Still, this self-consciously historical perspective is not merely descriptive—it also contains a theory that links violence to competition over state power.

In the Western world, this process usually led to the creation of relatively centralized nation-states. However, as nationalism continued to spread throughout the globe, whether driven by local conditions of modernization or by institutional imitation of more advanced models, nation-building tended to be much less successful. In cases where no nationalizing group managed to dominate the state,

a fight erupts over which ‘people’ the state should belong to, and social closure proceeds along many ethnic lines instead of one national line. Sometimes this contest for the control of the state escalates into ethno-nationalist civil wars, destroying much of whatever social and political cohesion there was (Wimmer 2002, p. 91).

This theory of conflict rests on two crucial assumptions that are violated by the political economists’ implicit models, namely that (A1) the state plays a central role for the evolution of conflict, and that (A2) conflict proceeds among groups rather than among individuals.

A1. Conflict over ownership of the state

The state is at the very center of nationalist conflict. In a competition for state goods, ownership of the state is the ultimate prize (Wimmer 2002). In addition to securing the first claim to material resources, the winner acquires crucial symbolic powers and international recognition. From the founding of the United Nations in 1945, there has been virtually only one road to success, namely membership in the exclusive club of its member states. Domestically, the state's importance grew as its role became increasingly intrusive in citizens' lives. This trend applied not only to the Western world, although states have remained relatively weak beyond the West. As state control has increased, opportunities have arisen to favor a specific ethnic group or groups through an ethnicized bureaucracy and/or cultural favoritism in terms of public schooling, language laws, and religious regulations. In this way, ethnicity started to serve as a formidable instrument of social and political exclusion.

A2. Conflict at the level of entire groups

In a pioneering contribution to the theory of nationalism, Gellner (1964) explained how individuals in the modern world identify themselves, and classify others, according to cultural categories. This stands in stark contrast to the pre-nationalist world, in which direct interpersonal connections were paramount. Categorization makes it possible to conceive of large groups as "imagined communities," a prerequisite for nationalism (Anderson 1991). Although one should not exaggerate the extent to which processes of identification override individual, materialist incentives, the nationalist revolution of military affairs amply demonstrates that nationally motivated soldiers are often willing to lay down their lives for the nation. In general, nationalist groups derive their cohesion from collective-level identities that resonate deeply with the emotional needs of modern citizens. Conversely, nationalism can quickly implode multi-ethnic states, such as the former Yugoslavia, once nationalist mobilization starts to pull the component groups apart. All this supports the hypothesis that "competition for state resources is seen as a matter concerning not just individuals or associations of shared interests but rather whole ethnic groups" (Wimmer 2002, p. 103).

Still, establishing that internal conflict is over state ownership, and that the main competitors in such conflict are ethno-national groups, does not mean that we have explained why violence actually ensues in particular cases (Fearon and Laitin 1996). For insights into the conditions under which conflicts are most likely to turn violent, Mann's (2005) recent book on murderous ethnic cleansing is especially helpful. Mann finds the key to violent escalation in political power relations, and in doing so, dispels the myth that constructivist theories have to be naive about power. In his view, violent conflict becomes likely wherever ethnic groups lay claim to the same, or parts of the same, territory, and at the same time stand a good chance of doing so successfully.

Mann evaluates these hypotheses in a series of case studies. In this paper, however, we have a very different goal, namely to develop a theoretically grounded alternative to the *ELF* capable of supporting efforts to investigate the causes of civil wars. Thus, we now turn to the challenge of model-building.

The N^* index: A model of ethno-nationalist civil wars

The general reasoning of the preceding section leaves many gaps open that will need to be filled before applying econometric reasoning. Our two main tasks are to identify the ethnic configuration E and the mechanism M that make up the core of the model. We will consider each task in turn.

*A star-like ethnic configuration E^**

Based on our assumption of the state's central role in ethno-nationalist conflict, we decided to reject the symmetric interaction topologies implied by the *ELF* and the *RQ* indices. Instead, drawing on assumptions *A1* and *A2*, we postulate a star-like configuration with an *EGIP* (ethnic group in power) at the center, surrounded by peripheral groups. This implies that the governmental group or coalition interacts with each non-governmental group, *and that the latter groups do not interact with each other*. We label the ethnic configuration E^* because it looks like a star.³

It is now straightforward to compute the conflict propensity of the entire system:

$$\Pr(\text{CivilConflict}) = 1 - \prod_{i=0}^{n-1} (1 - p(i))$$

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

Note that this center-periphery logic resembles Fearon and Laitin's (2003) insurgency model, although their focus is of course entirely non-ethnic. In contrast, Rokkan (1999) proposes a richer representation of how peripheries interact with central states, by accounting not only for economic and geographic factors but also for a cultural dimension (see also Cederman 2004).

*A micro-level mechanism M^**

To complete our simple model, we need to specify what happens in each dyad, which in technical terms boils down to specifying the function $p(i)$. Let us assume that the n ethnic groups can be ordered as a vector $\{s_0, s_1, s_2 \dots s_{n-1}\}$ where s_0 denotes the size of the *EGIP* and the remaining entries the non-governmental groups in whatever order. Following Mann's emphasis on power-based opportunities, we assume that conflict is likely to occur wherever a peripheral group stands a good chance of challenging the government's control. This is likely to be the case whenever it is strong enough in relation to the *EGIP*'s resources. It seems reasonable to select a logarithmic functional form, as is often done in conflict success functions (Hirschleifer 2001). Thus we define the probability of dyadic conflict with non-governmental group i 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 resources, r is a threshold value and k a slope parameter.⁴

The r parameter stipulates at what power balance the odds are even for a challenge. Parameter k controls how steeply the curve slopes. For $k = 0$ it is entirely flat and as k

³ Cederman (2004) proposes a similar interaction topology within the context of a computational model of nationalist insurgency. Agent-based modeling has the advantage of forcing the modeler to specify exactly with whom the key actors interact.

⁴ This is obviously just one possible functional form. A similar formalization is used in Cederman (2004). We also performed tests with a simple linear model $p(i) = k r(i)$.

goes to infinity, it becomes a perfect step function. The curve in Figure 1 illustrates the functional form for parameters $r = 0.5$ and $k = 5$.

[Figure 1 about here]

Note that although the mechanism seems to be entirely one-directional, this impression is misleading. In fact, no assumption has been made as to which side starts the fight. It may well be that the government preempts what it perceives as a peripheral threat. Moreover, even if the peripheral actor takes action first, the most extreme instances of ethnic violence have almost all been perpetrated by states in response to insurgencies: “Ethnic cleansings are in their most murderous phases usually directed by states, and this requires some state coherence and capacity” (Mann 2005, p. 7).⁵

Obviously, there are countless external factors that could enter this equation, including the geographic reach of the state as well as help to ethnic kin from third countries. These factors require additional coding and will therefore not be considered in this paper. Let us now define our indicator N^* (N -star) as $E^*(M^*)$, a label that reflects the underlying model of nationalism based on a star-like ethnic configuration:

$$N^*(r, k) = 1 - \prod_{i=0}^{n-1} \frac{\left\{ \frac{s_i / (s_i + s_0)}{r} \right\}^{-k}}{1 + \left\{ \frac{s_i / (s_i + s_0)}{r} \right\}^{-k}}$$

In order to get a better feeling for how N^* behaves in comparison to the conventional measures, Table 1 lists the ELF , RQ and $N^*(0.5, 5)$ for a number of sample groups. It immediately becomes clear that the new index is asymmetric with respect to the order of groups. Following convention, we always list the $EGIP$ first (and to clarify matters, it is highlighted in boldface). Therefore, whereas $\{\mathbf{0.7} \ 0.3\}$ yields 0.072, the reversed order $\{\mathbf{0.3} \ 0.7\}$ has a high probability of conflict: 0.843. This is so because if the $EGIP$ controls 0.7 of the resources, it is very hard to unseat, whereas a minority government of 0.3 would constantly be threatened. In contrast, the ELF and RQ do not change with the order of group listings. As more groups are successively added, the ELF increases due to growing fragmentation. By the same logic, the polarization index RQ decreases. In our model, however, the N^* goes up as long as the government’s share of total resources declines.

[Table 1 about here]

Empirical calibration of the N^* index

So far, we have studied the theoretical properties of N^* . It is now time to turn to the empirical performance of the new measure. The main obstacle to coding N^* is the need to specify the ethnic group(s) in power ($EGIP$) for each state. For operational purposes, we consider a group, or a coalition of groups, to be in power if their leaders serve (at least intermittently) in senior governmental positions, especially within the cabinet. Beyond the ethnic extraction of a country’s leading politicians, specific institutional arrangements, such as different types of power sharing and consociationalism, may also be an 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 $EGIP$. Thus, in cases

⁵ So far, this model is not necessarily inconsistent with Mann’s (2005) theory of ethnic cleansing. The cases that are not covered by this theory, however, are genocidal attacks on small minorities, such as the Jews in Nazi Germany or the Tutsi in Rwanda.

where the group in question is systematically excluded from power it is judged to be a non-governmental ethnic group (*NGEG*).

It goes without saying that this definition is hard to operationalize. One source of potential confusion relates to the exact nature of governmental involvement. Different countries have different institutional rules and political cultures, so inclusion and exclusion should ultimately be judged from within these contexts. Another difficulty relates to the possibility that different parts of the ethnic group might be included while other, possibly more radical branches, are excluded. There is also a potentially treacherous temporal dimension: one group may be *EGIP* during one period of a country's history but an *NGEG* during another.

Ultimately, it can be debated to what extent it is possible to identify clearly distinct ethnic groups at all. In the long run, at least, ethnic and national identifications are endogenous to other political processes, including state formation and conflict behavior (Laitin and Posner 2001). From a macro-historical standpoint, it makes sense to interpret violence as a side-effect of a co-evolutionary process that involves the formation of political and cultural units (Cederman 2002). By applying the measure to the post-1945 period, however, we are assuming that group identities are, at least in the majority of the cases studied, clearly marked and relatively stable for the period in question.

In a first cut, we focus on Eurasia and North Africa. Within this subset of the population, we believe that most of the cases can be coded in a relatively straightforward manner. Because of its notoriously hard-to-grasp, fluid identities, Sub-Saharan Africa was judged to be too difficult to code with respect to *EGIP* without extensive additional research. In future work, we plan to extend the coding to this and to other parts of the world, but extensive area expertise will be needed to get this data-gathering exercise right (cf. Posner 2004).

Our coding effort yielded observations for 88 countries, as indicated in the Appendix. We built on Fearon's (2003) list of ethnic groups and selected the *EGIPs*. Wherever more than one group was found to belong to the *EGIP*, the strength of the entire coalition was summed and kept as the first entry in the group vector. We compared our *EGIP* partitioning with an independent coder. To determine if a group was an *EGIP*, we relied on a dataset of leaders' ethnic affiliations collected by Idean Salehyan and his colleagues, as well as on indirect inferences from Gurr's Minorities At Risk (*MAR*) database and the *CIA Factbook*.

This empirical procedure generated an N^* index that is radically different from the *ELF* and the *RQ* indices (correlation-coefficients 0.42 and 0.36 respectively). This is confirmed by Figures 2 and 3, which plots $N^*(0.5, 5)$ as a function of the *ELF* and the *RQ* respectively. If N^* were identical to these other indices, the scatter plots would have formed a diagonal line from the lower left to the upper right. Instead, in both cases, the N^* remains very low for most observations with low to medium *ELF* or *RQ* values. For high ones, however, the variance is very high. Thus, there is no simple one-to-one correspondence between the N^* and the conventional indices.

[Figure 2 about here]

[Figure 3 about here]

Regression analysis

Having established that N^* does in fact represent a new measure, we now proceed to the task of evaluating its impact on conflict behavior. We do so within the context of Fearon and Laitin's (2003) econometric model that focuses on the onset of civil wars during the post-WWII period. This model is based on a pooled time series that contains country-year observations coded as a one if a civil war started within that observation and as a zero for all other cases. We use exactly the same list of independent variables as Fearon and Laitin include in their Model 1 except for the religious fractionalization index, which is dropped due to its marginal theoretical relevance to this paper.⁶ Thus the list includes lagged onset of civil wars, per capita income, population, terrain, territorial contiguity, oil exports, recent independence, political instability, democracy, and the *ELF* (see Table 2). For details about the operationalization of these variables, the reader is referred to the original article.

[Table 2 about here]

Relying on logit analysis, Table 3 contains the results of our first set of estimations.⁷ Due to the limited data availability for the N^* index, we focus on the Eurasian and North African cases only, which yields 3,327 out of the original 6,327 observations. Model 1 replicates Fearon and Laitin's (2003) base model for this subsample. There are no major surprises: the independent variables that were strongly significant in the full sample perform well here as well. The variables per capita income, population, oil exports, and recent independence remain significant. In contrast, the mountain variable and political instability lose their significance. The main "winner" is the democracy variable, which is now significant, although with a positive sign, indicating that democracy contributes to the risk of civil-war onset. All other variables remain insignificant, including (most importantly for our purposes) the *ELF*.⁸

[Table 3 about here]

Model 2 replaces the *ELF* with the *RQ* polarization index. We computed the values based on the same ethnic groups that we used to compose the N^* index in the preceding section. The second column of Table 3 tells us that this replacement changes little in terms of significance. The *RQ* index performs even worse than the *ELF*, and cannot be considered as a determinant of civil wars in this specification. Otherwise, there are virtually no changes as regards the main results that Model 1 produced.

Having failed to generate any statistically significant findings for the cultural variables up to this point, the question remains how well the N^* will do if substituted for the conventional fractionalization and polarization indices. We are now ready to study the main results of this paper.

Model 3 reports on the striking results of this exercise. Whereas all the other variables perform similarly in the previous findings, we now find a high level of significance for the $N^*(0.5, 5)$ measure. With a coefficient of 1.741 and a standard error of 0.66 we can reject the null hypothesis that the N^* has no impact with significance at the $p = 0.008$ level. Note that this finding was obtained while retaining all of the highly materialist variables that previously cancelled out the effect of the other cultural indices in econometric tests.

⁶ We did not find any model specification in which it differed noticeably from the *ELF*. Without exception, it remained as insignificant as that index.

⁷ All econometric models were implemented in Lisp-Stat (Tierney 1990). This package allows for dynamic coding of the log-likelihood function.

⁸ These results are derived from Fearon and Laitin's (2003) *ELF* coding. It is also possible to compute the *ELF* based on Fearon's group data, which we used to calculate the N^* index. Such a reconstruction generates roughly the same results, i.e. the *ELF* remains totally insignificant.

Although the location of the threshold r seems quite reasonably located at 0.5, it would be desirable to estimate its exact location. Fortunately, it is possible to do so by dynamically computing a new $N^*(r,k)$ each time the log-likelihood function is estimated.⁹ Model 4 in Table 3 presents the findings of this exercise. We found that the initial conjecture was quite accurate, because the estimated value is $r = 0.513$. Both the N^* term and the r -value reach significance, although the former estimate becomes somewhat less precise.

It is quite remarkable that such a crude model of nationalism yields significant results. All estimations reported in Table 3 are based on a dependent variable that treats all civil wars as one large category. Still, there are strong reasons to believe that such an inclusive assumption ignores considerable unit heterogeneity, especially since it lumps together ethno-nationalist civil wars with coups and other non-ethnic conflicts (Sambanis 2002a). Fortunately, Fearon and Laitin's (2003) data set includes a variable that separates specifically ethnic wars from other types of internal conflict. If our model of ethno-nationalist conflict is correct, it should perform especially well for this dependent variable. After all, such a coding corresponds directly to the causal story postulated in this paper.

To find out if this intuition can be empirically confirmed we reran Models 3 and 4 for the alternative dependent variable. In fact, Table 4 reveals that the conflict concept is limited to ethnic wars, as opposed to civil wars in general, so the N^* model performs extremely well. As suggested by Model 5, a fixed index $N^*(0.5, 5)$ yields a very high level of significance at $p = 0.001$ that even surpasses the materialist variables per capita income and oil exports in this respect. As before, the variables associated with population and recent independence continue to have a strong impact.

[Table 4 about here]

Model 6 presents a dynamic estimation of r , similar to that shown by Model 4. In this case, the parameter comes out somewhat lower ($r = 0.427$) than in the previous model. As expected, the estimation is even more significant for ethnic wars than for all civil wars. The model seems to fit the data remarkably well for these cases. Indeed, the statistical results of N^* confirm that the postulated causal mechanisms do a good job of summarizing the center-periphery relationship between *EGIPs* and *NGEGs*.

Of course, it would be premature to draw any definitive conclusions for the entire world. In this paper, we have limited ourselves to slightly more than half of the global sample studied by Fearon and Laitin.¹⁰ An extension of the N^* index to the non-Eurasian cases will require careful empirical coding of *EGIP*. Moreover, it will be necessary to study the robustness of the operationalization proposed in the Appendix even for the cases at hand. Such a coding project should also pay more attention to diachronic differences in the status of *EGIPs* than we have been able to do in this paper.

A particularly exciting extension of the current paper would attempt to derive group-specific measures beyond size. Based on the agent-based model introduced in Cederman (2004), we expect the location of the ethnic group to be very important. Future research could rely on Geographic Information Systems (GIS) to measure the logistical obstacles encountered by the capital in its dealings with each group. Such an elaboration is bound to produce a much more accurate estimation of the logistical

⁹ Unfortunately, we don't have enough information to estimate k as well, which is therefore kept at $k = 5$. Additional regression runs, not shown in the tables, confirm that the main findings are not sensitive to the exact value of k . We experimented with $k = 2$ and 10. The former setting weakened the significance of N^* in Model 3, for example, but the result remains significant.

¹⁰ Preliminary tests extending the coding of *EGIP* to all non-Sub-Saharan cases suggest that Models 5 and 6 hold up very robustly, but that Models 3 and 4 are more sensitive. This is hardly surprising given the large number of non-ethnic conflicts in Latin America.

relationship $p(i)$. At this point, we are assuming that “one size fits all,” regardless of the local logistical conditions in terms of geography and transportation infrastructure. In reality, even small groups can be very powerful in asymmetric conflicts, thanks to their ability to take advantage of difficult terrain far away from the capital. A relation-specific model of infrastructure may turn out to be much more robust than the very crude terrain variable used in this paper.

Conclusion

We started this paper by considering the implicit causal logic underlying the political economists’ econometric models. This was prompted by our observation that the causal mechanisms implied by the ethnic fractionalization index have very little to do with any reasonable theory of ethno-nationalist civil wars. Such “tests” of ethnicity get the theory wrong in at least two crucial ways. First, they tend to assume that violence is primarily a reflection of individual, as opposed to group-level, dynamics. Second, conventional econometric models also implicitly assume that conflict patterns are entirely symmetric. The same observation applies to evaluations based on polarization indices. By failing to formulate an adequate theory in the first place, Fearon and Laitin (2003) and Collier and Hoeffler (2004) stack the deck against cultural variables from the outset, and it is therefore not surprising that they conclude that ethnicity does not play a role in determining the outbreak of civil conflict.

It is remarkable that these scholars have systematically overlooked Gellner’s (1983) central point about the tension between states and nations. From his vantage point, it makes little sense to craft a theory of nationalism based on ethnicity without any explicit reference to the state. In fact, Gellner’s most important contribution was to show that national identities are largely distinct from underlying ethnic “raw” material. What matters are politically mobilized ethnic cleavages. However, neither the *ELF* nor the *RQ* index makes any attempt to account for the state as a distinctive actor. According to these models, ethnic conflict can be expected to unfold as if it occurred among ethnic gangs in an American city.¹¹

This fundamental theoretical misspecification would have been much less disturbing had it not been dressed up as a main finding about civil wars. Indeed, Fearon and Laitin seem explicitly intent on targeting Gellner’s theory. Far from shying away from making far-reaching policy conclusions, the authors warn policy makers and scholars against inferring

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).

We hope that the current study will provide a counterweight to such sweeping judgments. In our view, the political economists’ temptation to “cleanse” their models of ethnicity reflects ideological and meta-theoretical prejudices that lack solid empirical foundations. While we do not purport to have found the ultimate model of ethno-nationalist conflict, we do think that our results are promising enough to inspire future research on how cultural and logistical mechanisms interact to produce political violence. As demonstrated by Wimmer (2002) and Mann (2005), such a research program can build on a rich scholarly tradition.

¹¹ This may be the reason why Fearon and Laitin’s (1996) attempt to explain interethnic cooperation starts with an anecdote drawn from Latin’s childhood in Flatbush.

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Table 1. Examples of group configurations

	<i>ELF</i>	<i>RQ</i>	<i>N</i> *(0.5,5)
{ 0.5 0.5}	0.5	1.0	0.5
{ 0.7 0.3}	0.42	0.84	0.072
{ 0.3 0.7}	0.42	0.84	0.843
{ 0.3 0.3 0.2 0.2}	0.74	0.76	0.716
{ 0.2 0.3 0.3 0.2}	0.74	0.76	0.959
{ 0.2 0.2 0.2 0.2 0.2}	0.78	0.64	0.937

*EGIP*s are marked in bold.

Table 2. Independent variables used in Fearon and Laitin (2003)

Independent variable	Remarks
Prior war	lagged variable of civil war onset
Per capita income	lagged variable measured as thousands of 1985 US \$
log(population)	lagged variable
log(% mountains)	share of territory covered by mountains
noncontiguous state	dummy = 1 state is split by water obstacle
oil exporter	dummy = 1 for oil-exporting countries
new state	dummy = 1 for first 2 years of independence
instability	dummy = 1 for change of POLITY regime in prior 3 years
democracy	lagged POLITY IV measure (-10 to 10)
<i>ELF</i>	an implementation of the ethnic fractionalization index

Table 3. Logit analysis of determinants of civil war onset, 1945-1999

	Model 1 Civil War		Model 2 Civil War		Model 3 Civil War		Model 4 Civil War	
	Coeff.	Signif.	Coeff.	Signif.	Coeff..	Signif.	Coeff.	Signif.
Prior war	-1.024 (0.42)	0.015 *	-0.963 (0.409)	0.019 *	-1.024 (0.409)	0.012 *	-1.023 (0.409)	0.012 *
Per capita income	-0.305 (0.082)	0.000 ***	-0.315 (0.081)	0.000 ***	-0.336 (0.084)	0.000 ***	-0.336 (0.084)	0.000 ***
log(population)	0.322 (0.105)	0.002 **	0.335 (0.103)	0.001 **	0.396 (0.105)	0.000 ***	0.394 (0.106)	0.000 ***
log(% mountains)	0.259 (0.146)	0.076	0.275 (0.144)	0.056	0.261 (0.142)	0.066	0.265 (0.151)	0.079
Noncontiguous state	0.043 (0.35)	0.903	0.165 (0.346)	0.633	0.168 (0.344)	0.625	0.169 (0.344)	0.624
Oil exporter	1.331 (0.357)	0.000 ***	1.35 (0.357)	0.000 ***	1.321 (0.354)	0.000 ***	1.319 (0.355)	0.000 ***
New state	2.018 (0.438)	0.000 ***	1.998 (0.44)	0.000 ***	2.072 (0.441)	0.000 ***	2.075 (0.442)	0.000 ***
Instability	0.372 (0.351)	0.289	0.355 (0.352)	0.312	0.438 (0.353)	0.215	0.437 (0.353)	0.216
Democracy	0.056 (0.024)	0.017 *	0.057 (0.023)	0.014 *	0.063 (0.024)	0.009 **	0.063 (0.024)	0.009 **
<i>ELF</i>	0.611 (0.599)	0.308						
<i>RQ</i>			0.522 (0.656)	0.427				
<i>N*</i>					1.741 (0.66)	0.008 **	1.779 (0.818)	0.03 *
<i>r</i>							0.513 (0.158)	0.001 **
Constant	-7.549 (1.131)	0.000 ***	-7.782 (1.174)	0.000 ***	-8.159 (1.16)	0.000 ***	-8.152 (1.163)	0.000 ***
<i>N</i>	3,327		3,327		3,327		3,327	

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

Table 4. Logit analysis of determinants of “ethnic” civil war onset, 1945-1999

	Model 5 “Ethnic” War		Model 6 “Ethnic” War	
	Coeff.	Signif.	Coeff.	Signif.
Prior war	-1.253 (0.484)	0.01 **	-1.292 (0.491)	0.009 **
Per capita income	-0.253 (0.088)	0.004 **	-0.25 (0.088)	0.005 **
log(population)	0.67 (0.125)	0.000 ***	0.694 (0.131)	0.000 ***
log(% mountains)	0.192 (0.186)	0.3	0.139 (0.197)	0.481
Noncontiguous state	0.637 (0.401)	0.112	0.654 (0.405)	0.106
Oil exporter	1.136 (0.431)	0.008 **	1.111 (0.429)	0.01 **
New state	2.312 (0.553)	0.000 ***	2.295 (0.554)	0.000 ***
Instability	0.412 (0.456)	0.367	0.432 (0.456)	0.344
Democracy	0.036 (0.029)	0.217	0.036 (0.03)	0.218
<i>N</i> *	2.681 (0.793)	0.001 **	2.55 (0.788)	0.001 **
<i>r</i>			0.427 (0.087)	0.000 ***
Constant	-11.631 (1.467)	0.001 ***	-11.801 (1.499)	0.000 ***
<i>N</i>	3,327		3,327	

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

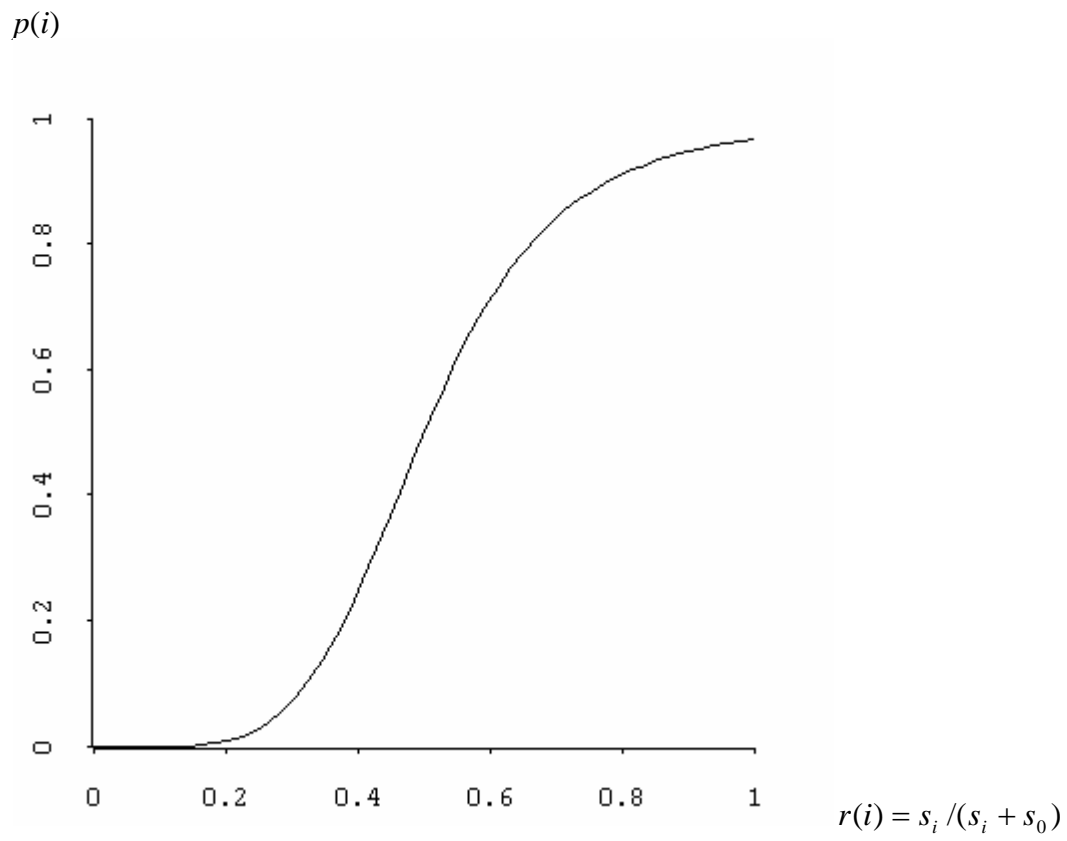


Fig. 1. The dyadic probability of conflict between group i and the government

$N^*(0.5, 5)$

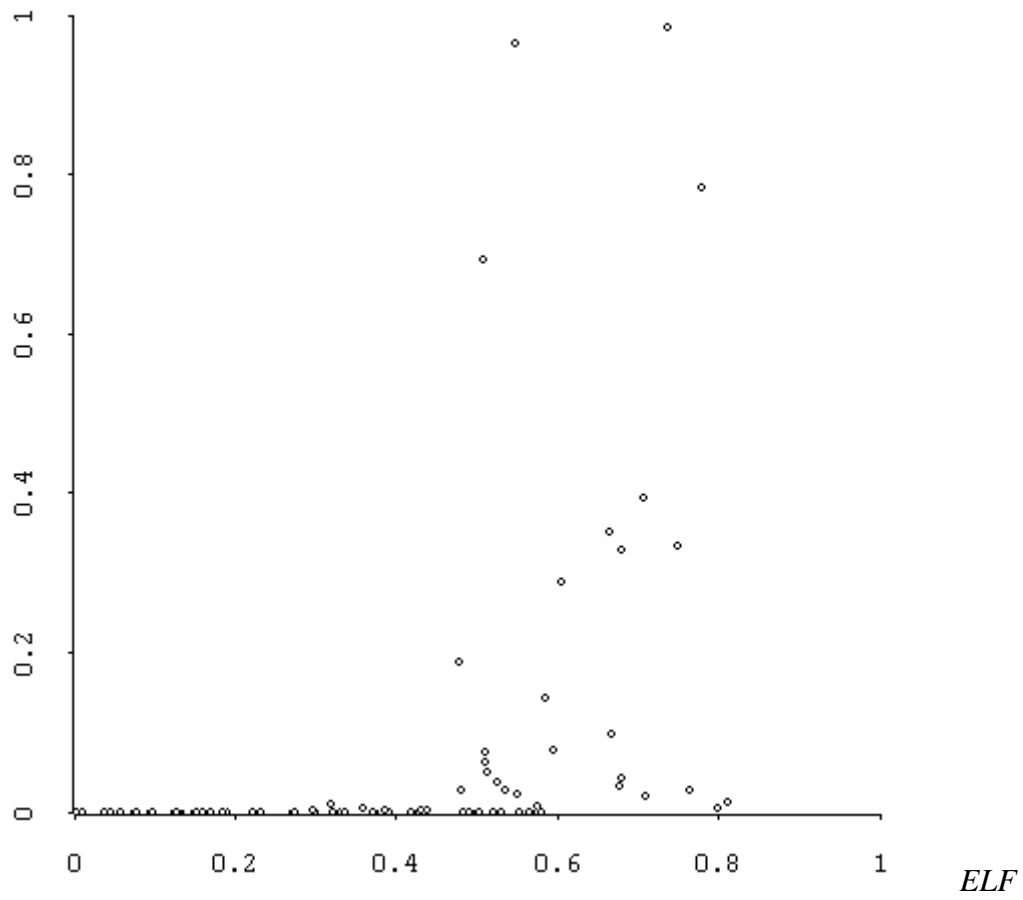


Fig 2. A scatter plot of N^* against the ELF index for all countries in the sample

$N^*(0.5, 5)$

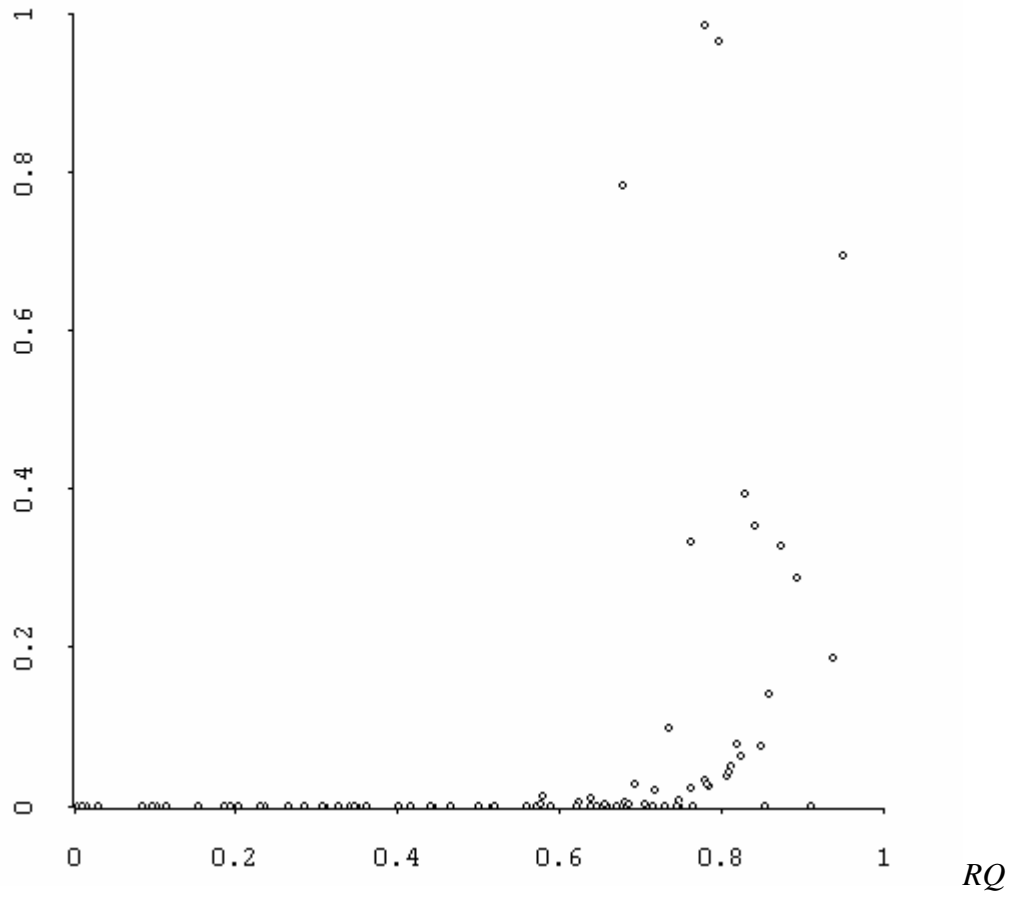


Fig 3. A scatter plot of N^* against the RQ index for all countries in the sample

Appendix: Ethnic Groups in Power

This table lists our coding of the EGIP in the Eurasian and North African sample used in this paper. Bold group names refer to the EGIP. Starred entries are coded as uncertain and are included in the regressions in Tables 3 and 4. However, their exclusion does not affect the results significantly.

ASIA

Afghanistan	Pashtun (38%) , Tajiks (25.3%), Hazaras (19%), Uzbeks (6.3%), Turkmen (2.5%), Qizalbash (1%)
Bangladesh	Bengali (87.5%) , Hindus (10.5%)
Bhutan	Bhote (50%) , Ethnic Nepalese (35%), Sharchops (15%)
Burma	Burman (68%) , Shan (8.5%), Karens (7%), Arakanese (4%), Chinese (3%), Zomis (Chins) (2.1%), Indian (2%), Mons (2%), Kachins (1.5%)
Cambodia	Khmer (90%) , Vietnamese (5.5%), Chams (2.5%), Chinese (1%)
China	Han Chinese (92%)
Fiji	Fijians (49%) , Indian (44%) , European - Mixed (1.51%) , Pacific Islander (1.35%), Rotuman (1.26%)
India	Hindi Speakers (39.9%) , Bengali (8.22%), Telusu (7.8%), Marathi (7.38%), Tamil (6.26%), Gujarati (4.81%), Kannada (3.87%), Malayalam (3.59%), Oriya (3.32%), Punjabi (2.76%), Sikhs (2%), Assamese (1.55%)
Indonesia	Javanese (45%) , Sunda (14.8%), Malays (5.9%), Madura (5.3%), Minangkabau (4.4%), Batak (1.9%), Balinese (1.84%), Bugis (1.5%), Betawi (1.31%), Aceh (1.3%), Banjar (1.1%), Chinese (1%), Susak (1%)
Korea, People's Republic	Korean (99.9%)
Korea, Republic	Korean (99.8%)
Laos	Lao Loum (68%) , Lao Theung (22%), Lao Sung (9%)
Malaysia	Malay (57.7%) , Chinese (25.4%), East Indians (7.2%), Dayaks (3%), Kadazans (2.9%)
Mongolia	Mongols (85%) , Kazakh (5.3%), Tungusic (4.6%), Chinese (2%), Russian (2%)
Nepal	Indo-Nepalese (53.2%) , Bihari (18.4%), Tharu (4.8%), Tamang (4.7%), Newar (3.4%), Magar (2.2%), Abadhi (1.7%)
Pakistan	Punjabi (66%) , Sindhi (13%)* , Pashtuns (Pushtuns) (9%), Mohajirs (8%), Baluchis (3%)
Papua New Guinea	(Not Applicable)
Philippines	Lowland Christ. Malay (91.5%) , Muslim Malay (4%), Chinese (1.5%), Igorots (1.4%)
Singapore	Chinese (76.4%) , Malay (14.9%), Indian (7.9%)
Sri Lanka	Sinhalese (74%) , Sri Lankan Tamils (12%), Moor (7.7%), Indian Tamils (6%)
Taiwan	Taiwanese (84%) , Mainland Chinese (14%), Aborigine (2%)
Thailand	Thai (74%) , Chinese (14%), Malay-Muslims (3.5%), Northern Hill Tribes (1%), Khmer (1%)
Vietnam	Vietnamese (87.5%) , Tay (2%), Chinese (1.8%), Thai (1.7%), Khmer (1.6%), Muong (1.5%), Nung (1.2%)

EASTERN EUROPE

Albania	Albanian (95%) , Greeks (3%)
Armenia	Armenians (93%) , Azeri (3%), Russians (2%)
Azerbaijan	Azeri (90%) , Dagestani (3.2%), Russian (3%), Armenians (2.3%)

Belarus	Byelorussian (78%), Russian (13.2%), Poles (4.1%), Ukrainian (2.9%)
Bosnia	Muslims (43.7%), Bosniaks (Serbs) (31.4%), Croats (17.3%)
Bulgaria	Bulgarian (83%), Turkish (9.4%), Roma (5%), Pomaks (1.67%)
Croatia	Croat (78.1%), Serb (12.2%)
Czechoslovakia	Czech (63%), Slovak (31%)*, Hungarians (4.1%), Roma (2.6%)
Czech Rep.	Czech (81.2%), Moravian (13.2%), Slovak (3.1%), Roma (2.6%)
Estonia	Estonian (64%), Russian (28%), Ukrainian (2.5%), Byelorussian (1.5%), Finn (1%)
Georgia	Georgian (70.1%), Armenian (8.1%), Russians (6.3%), Adzhars (5.8%), Azeri (5.7%), Ossetians (South) (3.2%), Abkhazians (1.75%)
Germany, Dem. Rep.	Germans (99.7%)
Hungary	Hungarian (90%), Gypsy (5%), German (2.6%), Serb (2%)
Kazakhstan	Kazakh (45%), Russian (35.8%), Ukrainian (5.1%), Germans (3.6%), Uzbek (2.2%), Tartar (2%), Uighur (1.4%)
Kyrgyzstan	Kirghiz (52%), Russian (18%), Uzbeks (12.9%), Ukrainian (2.5%), Germans (2.4%)
Latvia	Latvian (56.5%), Russian (30.4%), Byelorussian (4.3%), Ukrainian (2.8%), Polish (2.6%)
Lithuania	Lithuanian (80.6%), Russian (8.7%), Poles (7%), Byelorussian (1.6%)
Macedonia	Macedonian (64.6%), Albanian (21%), Turks (4%), Roma (4%), Serbs (2.2%)
Moldova	Moldovan (64.5%), Slavs (26.8%), Gagauz (3.5%), Bulgarians (2%), Jews (1.5%)
Poland	Poles (97.6%), German (1.3%)
Romania	Romanian (83%), Hungarian (8.3%), Roma (6.5%), Germans (1.5%)
Russia	Russian (81.5%), Tatar (3.8%), Ukrainian (3%), Lezgins (1.7%), Chuvash (1.2%)
Slovakia	Slovakia (80.6%), Hungarian (10.1%), Roma (9.3%)
Slovenia	Slovenes (87.6%), Croat (2.7%), Serb (2.4%), Bosniak (1.4%)
Tajikistan	Tajik (64.9%), Uzbeks (25%), Russians (5%), Tartar (1.4%), Kyrgyz (1.3%)
Turkmenistan	Turkmen (77%), Uzbeks (9.2%), Russians (8%), Kazakhs (2%)
Ukraine	Ukrainian (73%), Russian (22%)*, Jews (1%)
USSR	Russian (50.8%), Ukrainian (15.4%), Uzbeks (5.84%), Byelorussian (3.5%), Kazakhs (2.85%), Tatars (2.4%), Azerbaijanis (2.38%), Armenians (1.62%), Tadzhiks (1.48%), Georgians (1.39%), Moldavians (1.14%), Lithuanians (1.07%)
Uzbekistan	Uzbek (71%), Russian (8%), Tajiks (5%), Kazakh (4%), Karakalpak (2%), Tartar (1.5%)
Yugoslavia, Socialist Federal Republic of	Serbs (36.2%), Croats (19.7%)*, Muslims (10%), Albanian (9.3%), Slovenes (7.5%), Macedonians (5.8%), Hungarian (3.3%), Montenegrin (2.3%)
Yugoslavia, Federal Republic of	Serbs (62.6%), Montenegrin (5%)*, Albanian (16.5%), Roma (4%), Hungarians (4%), Muslims (1.84%), Croats (1.2%)
NORTH AFRICA	
Algeria	Arab (80%), Berbers (20%)
Bahrain	Bahraini (63%), Asian (19%), Other Arab (10%), Iranian (8%)
Cyprus	Greeks (78%), Turks (18%)
Egypt	Egyptian (91%), Coptic Egyptian (9%)
Iran	Persian (51%), Azerbaijani (24%), Gilaki / Mazandarani (8%), Kurds (7%), Arabs (3%), Lur (2%), Turkmen (2%), Baluchis (2%), Bakhtiari (1%)

Iraq	Sunni-Arab (15.5%) , Shi'is (62.5%), Kurds (19%), Turkoman (1.7%)
Israel	Jewish (63.7%) , Palestinians (21.7%), Arab (14.7%)
Jordan	Transjordan Arabs (40%) , Palestinian (57.5%), Circassian (1%), Armenian (1%)
Kuwait	Kuwaiti (40%) , Asian (30%), Other Arab (20%), Iranian (4%)
Lebanon	Maronite (25%) , Shi'is (32%), Sunni Muslim (20%), Palestinians (10%), Druze (6%), Armenian (4%)
Libya	Arabs (92%) , Berbers (5%)
Morocco	Arabs (62%) , Berbers (37%)
Oman	Ibadhi Muslim (73.5%) , Indian (13.3%), Bengali (4.3%), Pakistani (3.1%), Egyptian (1.6%)
Saudi Arabia	Sunni Arabs (66%) , Indians (6%), Egyptians (6%), Pakistanis (4%), Shi'is (4%), Filipino (3%)
Syria	Sunni-Arab (62%) , Alawi (14%)* , Kurds (9%), Christians (8%), Druze (3%)
Tunisia	Arabs (98%) , Berber (1.2%)
Turkey	Turkish (82%) , Kurds (17%), Arab (1.4%)
U. Arab Emirates	Emiri (12%) , South Asian (45%), Iranian (17%), Egyptians (13%)
Yemen	Arabs*

WESTERN EUROPE

Austria	Austrians (93.4%) , Former Yugoslavs (4%), Turks (1.64%), Croatians (1.2%)
Belgium	Flemish (58%) , Walloon (31%)* , Italians (2%), Moroccans (1%)
Denmark	Danes (93.4%) , Asians (1%)
Finland	Finns (93%) , Swedes (6%)
France	French (85%) , Muslim (5.5%), Bretons (4.9%)
Germany (Fed. Rep.)	Germans (95.1%) , Turks (2.4%), Yugoslavs (1%)
Greece	Greeks (97%) , Roma (1.7%), Muslims (1.2%)
Ireland	Irish (91%) , Anglican (3%)
Italy	Italians (98%)
Japan	Japanese (99.4%)
Netherlands	Dutch (96%) , Muslims (4%)
Norway	Norwegian (95%)
Portugal	Portuguese (98%)
Spain	Castillan Speakers (68%) , Catalan (16.9%)* , Galician (6%), Basques (5.4%), Roma (1.9%)
Sweden	Swedes (90%) , Finnish (2.3%)
Switzerland	German (62.2%) , French (16.4%) , Italians (8.1%) , Romansch (1%) , Yugoslavs (5.2%), Spanish (3.1%), Portuguese (2.1%), Turks (1.2%), Asians (1.2%)
UK	English (81.5%) , Scots (9.5%)* , Asians (2.8%), Irish (2.4%), Afro-Caribbeans (2%), Welsh (1.9%), Catholics In N. Ireland (1.19%)