

Do Men Rebel because the State Is Weak?: A Critique of the Fearon-Laitin Model

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In their article in American Political Science Review, Fearon and Laitin have made a seminal contribution to our further understanding of dynamics of occurrence of civil wars, putting forward new empirical findings based on large-N analysis. A careful re-analysis of their dataset suggests, however, that some part of their findings are not very convincing. In the present article, I take up four issues, namely (1) assumption, (2) operationalization and measurement, (3) interpretation of the results, and finally the overall implication, in lieu of conclusion.

1. Introduction

In their article in *American Political Science Review*, Fearon and Laitin [3] have made a seminal contribution to our further understanding of dynamics of occurrence of civil wars, putting forward new empirical findings based on the large-N analysis. According to them, it is not ethnic/religious diversity *per se* or grievances among population but “right conditions for insurgency” that matter for the onset of civil wars. It is actually rather surprising to see how simple their operationalisation of key variables is: their “new states” and “political instability” variables can be easily computed by anyone without much effort. Yet nobody has constructed these variables that show such a strong effect on the onset of civil wars.

Their article exemplifies the importance of theoretical reasoning for innovative research.

All the contributions it makes notwithstanding, however, some part of their research does not seem equally convincing to my eyes. In the present article, I would like to take up four issues, namely (1) assumption, (2) operationalization and measurement, (3) interpretation of the results, and finally the overall implication in lieu of conclusion.

2. Assumption

Firstly, I would like to discuss the implicit assumption in their research design, which is on the homogeneity of what they call “civil war”: there is no distinction of different types of civil war and all civil wars are treated as equal. I argue that this assumption could be problematic. Sambanis also criticises this

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assumption, arguing that “the new economic theories of civil war do not consider if different war types have different causes, and their research designs, which aggregate all civil wars in a single category, implicitly suggest that there are no such differences” (Sambanis [9], p. 265). If there actually is such difference of types of civil war, the conclusions drawn at the aggregate level might not be appropriate for a certain type of civil war.

Fearon and Laitin have surely considered the difference of *ethnic/non-ethnic* wars, just to show that it does not challenge their key findings. This difference is not *the* only important one, however. Another important difference is the one between revolutionary wars (civil wars over the control of central government) and secessionist wars (where one of the warring parties tries to secede from the country). Among their independent variables, some are suspected to have different effects on the different types of civil war. Here, I discuss two such independent variables: the ethnic diversity and the variables related to “grievances”.

Firstly, as for the ethnic diversity, it might affect only secessionist war onset but not revolutionary war onset. Historically, revolutionary wars have occurred even in ethnically highly homogeneous countries, such as in Ireland in the 1920 s, but secessionist wars have been rare in the countries where the people think of themselves as ethnically homogeneous. It seems that this tendency has been strengthened after the end of the Cold War. For example, Bookman [1] argues that “most of the cases of secessionist activity that characterize the world in the 1990 s are cases in

which ethno-nationalism has taken on elements of ethnic separatism” (p.153). Therefore, it may be the case that the ethnic diversity has a significant effect on the onset of secessionist war in particular, while not on the onset of revolutionary war.

Secondly, grievances that motivate potential rebels could be different according to the types of civil war. Because Fearon and Laitin assume that *all* civil wars have the same causes, they simply regress civil war onset on variables related to “grievances”. Their findings that the effect of “grievances” on the onset of civil wars is not statistically significant are not so surprising, because their assumption is rather unrealistic: all civil wars are caused by specific type of grievances, be it economic inequality or the ethnic (language) discrimination. For example, since revolutionary wars are not necessarily (even though they could be) related to ethnic issues, ethnic discrimination might not affect the onset of revolutionary wars while it still affects the onset of ethnic wars or secessionist wars. As for the economic inequality variable, it could affect the onset of revolutionary wars but not ethnic or secessionist wars because the gini coefficients, used as a proxy of economic grievances by Fearon and Laitin as well as other authors, can only capture the *vertical* economic inequality that exists in the society but not *horizontal* inequality between different ethnic groups. One can interpret this index only as a proxy for a seriousness of the class conflict or class cleavage within the country, and in the twentieth century, the class conflict has not caused many secessionist wars but mostly revolu-

TABLE 1. Distribution of onset cases according to the two distinctions: ethnic/non-ethnic, and centre-aimed/ exist-aimed wars.*

		Ethnic or non-Ethnic:			Total
		not-ethnic	ambig/mixed	ethnic	
Rebels aim at :	Center	33	12	16	61
	Mixed/ambig	3	4	7	14
	Exit or Autonomy	0	3	32	35
Total		36	19	55	110

Note: Excluding colonial wars. Based on the replication dataset of Fearon and Laitin [3]. The value of aim is missing in one case: Indonesia in 1953.
 Pearson $\chi^2(4) = 42.1934$ $p < 0.001$

tionary wars.

Fortunately, Fearon and Laitin have included the variable “aim”¹ in their dataset, which makes it possible to split onset cases along the difference between revolutionary and secessionist wars in an exactly same manner as Fearon and Laitin do for the distinction between ethnic and non-ethnic wars (as for the number of cases in each category, see Table 1). Thus I have conducted a re-analysis on each type of civil war onset separately with exactly the same estimation method and independent variables (see Table 2, model 1 and 2).

The results of the re-analysis do not support the hypothesis set above about the effect of ethnic diversity on secessionist wars: the Ethno-Linguistic Fragmentation (ELF) variable remained insignificant for both cases (see table 2, model 1 and 2). But note that, as shown in table 1, there is *no secessionist war* if fighters are mobilized along *non-ethnic* lines. If we conduct a chi-square test for the hypothesis that the rows and columns in this two-way tabulation of the Table 1 are independent and not related to each other, we obtain a high value of the Pearson’s chi-square which is statistically significant at 0.001

level (and thus we reject the hypothesis that there is no association between these two dimensions). The distribution in this table indicates that the pre-existing ethnic diversity which will provide a basis for the ethnic mobilization of fighters is one of the *necessary conditions* for the onset of secessionist civil war, unless we assume that potential secessionist leaders can artificially create a new ethnic identity in order to wage a secessionist war. Actually, if we look at the value of ELF among secessionist war onset cases, the value of ELF is higher than 0.1 for all cases except for Bangladesh (1976), which means that a minority group would share more than 5% of the population if there are two ethnic groups in that country. Of course, this does not mean that the ethnic diversity is a sufficient condition for the secessionist war onset, since we have many cases where the ethnic diversity does not lead to the onset of secessionist wars. This does not mean, however, that the ethnicity does not matter. The ethnicity *does* seem to matter for the onset of secessionist war onset, given the distribution of the two-way tabulation shown in the Table 1.

Is this argument on the relations between the ethnicity and secessionist war

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TABLE 2. Logit analysis of Determinants of revolutionary war, secessionist war and ethnic war onset, 1945-1999.

Dependent Variable :	Models							
	(1)		(2)		(3)		(4)	
	Revolutionary war		Secessionist war		Revolutionary war		Revolutionary war	
Independent Variables	Coef.	Std. Err.	Coef.	Std. Err.	Coef.	Std. Err.	Coef.	Std. Err.
Prior War	-1.241**	0.537	-1.181**	0.509	-1.177	0.773	-0.987	0.736
Per capita income	-0.342***	0.105	-0.243***	0.096	-0.410**	0.163	-0.357**	0.146
log (population)	-0.018	0.105	0.723***	0.133	-0.042	0.163		
log (% mountainous)	0.272**	0.106	-0.016	0.179	0.297*	0.161	0.327**	0.148
Noncontiguous state	-0.083	0.473	0.962**	0.430	0.385	0.572		
Oil exporter	0.490	0.430	1.370***	0.436	0.363	0.653		
New state	1.485***	0.439	1.678***	0.718	1.638***	0.607	1.580***	0.566
Instability	0.649**	0.317	0.899**	0.413	0.434	0.453		
Democracy	0.005	0.023	0.024	0.03	0.041	0.031		
Ethnic fractionalization (ELF)	-0.282	0.488	0.561	0.709	0.201	0.771		
Religious fractionalization	0.588	0.655	-0.086	1.031	-0.159	1.024		
Economic Inequality (Gini)					0.038*	0.022	0.037**	0.019
Language Recognition (at Local Level)								
Ethnic Heterogeneity (EHET)								
Constant	-4.651***	0.98	-12.512***	1.459	-6.268***	2.042	-6.564***	1.018
N	6327		6327		4362		4388	

Note: The dependent variable is coded "1" for country years in which a (particular type of) civil war began and "0" in all others. Estimations performed using Intercooled Stata ver. 8.0. * p < 0.1; ** p < 0.05; *** p < 0.01.

onset tautological? It would be if one assumes that the secessionist war is by definition the ethnic war as well. If the secessionist wars, *by definition*, can *only* occur based on the ethnic mobilization, then the relations found above are nothing more than the reflection of the definition of secessionist wars. However, I would argue that it is not tautological, since secessionist wars *theoretically* could occur based on non-ethnic mobilization, a primary historical example of which is the Civil War in the United States. I would argue that the secessionist wars *could* occur based on the regional or other differences not related to the ethnicity, and thus are *not by definition* ethnic. Therefore, I believe that the rela-

tions found above between the ethnicity and secessionist war onset are themselves interesting, if not surprising.

As for the grievances, the economic inequality (the average gini coefficient for a country in the table 1 of Deininger and Squire [2]) has indeed an effect on the onset of revolutionary wars (see Table 2, model 3). If we add this variable to the baseline model of Fearon and Laitin, its effect is statistically marginally significant (p-value is 0.085, thus significant at 0.1 level but not significant at 0.05 level) and the sign is positive as expected (i.e. the increase in the economic inequality increases the probability of revolutionary war onset, other conditions being equal).

TABLE 2. Logit analysis of Determinants of revolutionary war, secessionist war and ethnic war onset, 1945-1999 (Continued).

Dependent Variable :	Models							
	(5)		(6)		(7)		(8)	
	Ethnic war		Secessionist war		Ethnic war		Ethnic war	
Independent Variables	Coef.	Std. Err.	Coef.	Std. Err.	Coef.	Std. Err.	Coef.	Std. Err.
Prior War	-1.035***	0.369	-1.163**	0.510	-1.030***	0.377	-0.829**	0.364
Per capita income	-0.359***	0.088	-0.254***	0.095	-0.335***	0.088	-0.300***	0.082
log (population)	0.289***	0.088	0.587***	0.136	0.376***	0.084	0.425***	0.076
log (% mountainous)	0.215**	0.109	0.022	0.190	0.158	0.104		
Noncontiguous state	0.489	0.327	0.906**	0.450	0.484	0.328		
Oil exporter	0.978***	0.333	1.068**	0.469	1.022***	0.325	0.963***	0.307
New state	1.798***	0.396	1.765**	0.732	1.812***	0.396	1.798***	0.381
Instability	0.463	0.298	0.842*	0.435	0.505*	0.298	0.531*	0.294
Democracy	0.019	0.021	0.020	0.032	0.013	0.021		
Ethnic fractionalization (ELF)	0.412	0.489	-0.050	0.758				
Religious fractionalization	1.180*	0.653	-0.558	1.064	1.446**	0.666	1.221*	0.651
Economic Inequality (Gini)								
Language Recognition (at Local Level)	0.803***	0.293	1.619***	0.536				
Ethnic Heterogeneity (EHET)					0.008*	0.004	0.008**	0.004
Constant	-8.287***	0.912	-11.802***	1.407	-8.906***	0.928	-8.929***	0.856
N	6325		6325		6153		6183	

Furthermore, if we exclude the irrelevant variables that do not show the statistically significant effects at 0.1 level, the effect of economic inequality becomes statistically significant at 0.05 level (see Table 2, model 4). On the other hand, the economic inequality does not have a significant effect on the onset of secessionist wars or of the ethnic wars (the results are not presented in the table). Note that the results for the gini variable are relatively fragile—for example, its effect becomes statistically insignificant even at 0.1 level when we include all variables of the baseline-model of Fearon and Laitin and we use rare-event logit as an estimator instead of normal logit (but it does remain significant at 0.05 level even with rare-event logit once we exclude irrelevant

variables)². After all, therefore, I admit that there is not so strong evidence that the gini coefficient is a very reliable predictor of the revolutionary war onset. These results do show, however, that the effect of the economic inequality captured by the gini coefficient *is* different according to the types of civil war. What these results imply is that the fundamental assumption of Fearon and Laitin that all types of civil war have the same causes is highly questionable.

As for the language and religious discrimination, some variables again show a statistically significant effect on some type of civil war. For example, as for the variables related to the language discrimination, the presence of the formal or de facto recognition of other language(s) at the

local/regional level³ has a statistically significant effect on the onset of both ethnic wars and secessionist wars (see table 2, model 5 and 6)⁴. In both cases, its effect is highly significant (p-value is less than 0.01) and the sign is positive (i.e. the presence of formal or de facto recognition of some language(s) increases the probability of the onset of war, other conditions being equal)⁵. These results persist even when we drop irrelevant variables (that are not significant at 0.1 level) and when we use different estimators such as rare-event logit. This variable, on the other hand, does not have a statistically significant effect on the onset of revolutionary wars. Therefore, these results again show that the effects of language discrimination (or, more precisely, language recognition) are indeed different for different types of civil war.

In addition to the results discussed above related to the ethnic diversity and grievances, the results of the re-analysis also reveal some interesting things: while variables such as per capita income, new state and instability remain significant for both types of wars, other variables such as population, mountainous terrain and “oil exporter” variable remain significant only for either of the two (see model 1 and 2 in the table 2).⁶ Note that the coefficients of population and mountainous terrain even take an opposite sign for the two cases. This suggests that some of the results in Fearon and Laitin [3] depend on the particularly strong effects of the variables on a particular type of civil war, thereby implying that making inference on all cases of civil wars based on their results could be misleading. Furthermore,

some results of the re-analysis even challenge their main hypothesis. For example, if the larger population represents the weakness of the state as they operationalise it (Fearon and Laitin [3], p. 81), why does it affect *only* secessionist wars? This rather suggests that the effect of larger population might reflect a different mechanism, such as economy of size for secession (i.e. the smaller the existing state, the less probable the rise of secessionist claims because creating an even smaller new state would not be a viable option).

3. Operationalization and Measurement

3.1. The Ethnic Diversity

Fearon and Laitin use the ELF as a proxy for the ethnic diversity. Even though this is a common practice among scholars, it could be problematic because the formula used for calculating the ELF does not summarize the country’s ethnic diversity well. Recall that the formula used is as follows:

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

where Si is the share of the i th group ($i = 1 \dots n$). The number calculated by this formula could be inappropriate as a summary of the ethnic diversity in one country. For example, as Posner [8] has argued, if there are two countries, one with two ethnic groups of equal size (1/2, 1/2) and the other with one ethnic group that shares two-thirds and two more groups of one-

sixth each ($2/3$, $1/6$, $1/6$), this formula will give exactly the same number (0.5) for both countries. While there is a large difference between these two countries in terms of the ethnic diversity (numbers of the ethnic groups and the share of these groups), these countries are not distinguishable if we use the ELF.

One way to check the validity of the analysis using the ELF index is to use the different index of ethnic diversity instead of the ELF and compare the results. If we arrive at the same conclusion even when we use the different indices, we could be more confident on the results. As an alternative measurement of the ethnic diversity, I use the index of ethnic heterogeneity (EHET) constructed by Vanhanen [11]. Interestingly, for example, Sambanis [9] finds that the ethnic diversity indeed matters for the onset of ethnic wars, contrary to the findings of Fearon and Laitin, using Vanhanen's index of ethnic diversity. Therefore, using this index for the re-analysis of Fearon and Laitin also gives us a clue why these authors arrive at completely contradictory conclusions.

The results of the re-analysis using the Vanhanen's index as a replacement for the ELF are shown in the table 2 (model 7 and 8). If we compare the results, it does appear that we will arrive at the different conclusion on the onset of ethnic wars if we use this index rather than the ELF. While the effect of the EHET is not statistically significant for the onset of civil war (at the aggregate level), its Z value is much higher than that of the ELF for the onset of *ethnic wars* (p-value is 0.052, which is very close to the 0.05 threshold) and its Z value increases and gets statisti-

cally significant at 0.05 level once variables that do not show a statistically significant effect (at 0.1 level) are dropped. Of course, these results do not necessarily mean that the results for EHET are right and those for the ELF are wrong. Actually, it is quite difficult to argue which is right and which is wrong when two indices that are *supposed* to measure the same thing (in this case, ethnic diversity) show the different results. They do suggest that, however, we should be cautious in drawing a conclusion based on the results of these indices, since it indicates the measurement error of either (or quite possibly, both) of the indices.

3.2. The Grievances

Based on their findings, the authors argue that there is "little evidence that one can predict where a civil war will break out by looking for where ethnic or other broad political grievances are strongest" (Fearon and Laitin [3], p. 75). They share the fundamental point with a prominent economist Paul Collier, who has been arguing that the civil wars are caused not by the grievances but by the greed or opportunities. They have presented more evidence for the current trend of rejection of grievances as a cause of civil wars.

It seems to me, however, that we should still be careful not to jump to the conclusion that grievances do not matter. It is because their variables may not have covered the types of grievances that could cause civil wars. Even though they do their best to operationalise "grievances", there are still other types of serious grievances not captured by their variables—such as discriminatory treatments of specific

group in the economic life, historical loss of autonomy or independence, *de facto* marginalization of distinct community under current political system (even if equal opportunity of participation is formally given to every citizen), and so on. While many scholars (especially those who conduct qualitative cases studies) have argued that the grievances are one of the causes of violent conflicts, the actual content of the grievances seems to vary to a great extent across the cases. If that is the case, one might argue that the “grievances” are much more difficult to capture quantitatively by a single measure compared to economic and other indicators, and that their results in favour of “conditions of insurgency” might be partly due to the fact that the grievances that could lead to the rebellion are not captured adequately by their variables.

3.3. Oil

As for the “oil” variable, it seems to me that using a binary variable is too crude. There is no theoretical reason to take one-third as a threshold: equating 30% and 0% while differentiating 30% from 34% does not sound plausible. Since there must have been a continuous variable of the share of fuel exports, I argue that they should have used or at least included the continuous variable. By comparing the results for binary and continuous variables, or results for binary variable with different level of threshold, one could have checked how sensitive the results are to the level of threshold. This point is important because the results for their oil variable do seem quite fragile. As Humphreys [4] found, the relationship between their oil

variable and the civil war onset disappears if the outlier cases of Russia and Indonesia are dropped (Russia and Indonesia have total 9 onsets in the dataset)⁷. Furthermore, as discussed above, their oil variable remains significant only for the onset of secessionist wars if we analyze the onset of different types of civil war separately (see table 2, model 1 and 2). It is not surprising because both of the two outliers—Russia and Indonesia—have disproportionately larger number of secessionist wars (out of total 9 onsets, 7 are secessionist wars).

This problem of measurement, however, seems to be solved by Humphreys [5], who constructed the oil-related *continuous* variable based on various sources and has found that this variable has a statistically significant effect on the civil war onset (using the coding of civil war onset of the Fearon and Laitin [3]). At the time of writing, therefore, their oil variable seems to be surpassed in quality by the data constructed by Humphreys, as Professor Fearon himself has noted⁸. A question that remains unanswered is, however, whether the oil variable constructed by Humphreys has a statistically significant effect on all types of civil war or has a significant effect only on some type of civil war as the oil variable of Fearon and Laitin does. The results of the investigation would affect our inference on the relations between the presence of natural resources such as oil and the civil conflict onset.

4. Interpretation of The Results

As for the interpretation of the results, I would like to argue that their interpretation of the results for GDP per capita is not totally convincing. It is because GDP per capita can be a proxy for so many mechanisms and we are not sure what mechanism is working. According to Fearon and Laitin, GDP per capita can be a proxy of three mechanisms: (1) overall financial, administrative, police, and military capabilities of the state, (2) the degree of penetration into rural society by the central administration, (3) the possibility of recruiting young men to the life of guerrilla (Fearon and Laitin [3], p. 80). They argue that “it is difficult to find measures to distinguish among these three mechanisms” but they “believe” that the strong results for per capita income are due “largely” to its acting as a proxy for state military and police strength relative to potential insurgents (Fearon and Laitin [3], p. 80). But since there is no way to distinguish three mechanisms, we simply cannot tell which one—or a combination of several mechanisms—is working behind the relationship between the GDP per capita and the onset of civil war.

Contrary to the authors, I think that there are some possible proxies for three mechanisms associated with GDP per capita. For military strength, for example, one might be able to use the number of troops or the amount of military expenditure of the state as a proxy of it. As for the possibility of recruiting young men to the life of guerrilla, one could use an unemployment

rate as a proxy of the “opportunity cost” of taking up arms. This latter point is made by Sambanis, who argues that “given the lack of clarity about what exactly GDP measures, one wonders why these studies did not use more direct measures of opportunity cost, such as unemployment, especially among young men” (Sambanis [10], p. 20). By introducing these variables, we might be able to distinguish different mechanisms and to see exactly which mechanism is working.

Thanks to a series of large-N studies on the onset of civil war, it has now become clear that the GDP per capita is one of the most reliable predictor of the civil war onset. As Humphreys [5] has pointed out on the relations between natural resources and civil conflict, however, “[p]rominent research has focused on correlations without constructing tests to identify particular mechanisms that may underlie those correlations” and has “arbitrarily favored one mechanism to the exclusion of others” (p. 28). The intellectual challenge we face is to investigate what is the mechanism that works behind the relations between the GDP per capita and the civil war onset.

5. In Lieu of Conclusion: On Overall Implication

Finally, in lieu of conclusion, I would like to discuss the overall implication of the results of their analysis. Despite all the problems discussed above, I still accept that their main argument is plausible to some extent not only empirically but also logically—“right conditions of insurgency,” such as lower GDP per capita, new

states and the regime instability, do matter for the onset of civil wars. Whatever may be the motivations of the potential rebels, the bad conditions of insurgency, which make the rebellion more difficult, would deter the potential rebels from taking up arms. In contrast, right conditions of insurgency are the enabling conditions of civil war and we will see civil wars if (and maybe only if?) these conditions are present.

Does this also mean, however, that men rebel *simply because* the conditions are right? It seems to me that Fearon and Laitin think it does: “if”, they argue, “under the right environmental conditions, just 500 to 2,000 active guerrillas can make for a long-running, destructive internal war, then the average level of grievance in a group may not matter that much” and “what matters is whether active rebels can hide from government forces and whether economic opportunities are so poor that the life of a rebel is attractive to 500 or 2,000 young men” (Fearon and Laitin [3], p. 88). In a word, such a view regards rebels as mere opportunists. There is no need of distinction between motivations and enabling conditions here, because both merge.

They may well be mere opportunists in some cases. As a general proposition, however, this view does not seem very convincing after the careful re-analysis of their research. Firstly, if we conduct an analysis separately for different types of civil war, there *is* some empirical evidence that the grievances matter for the onset of civil wars, even though the evidence is not very strong. In addition, many of the variables associated with the “right conditions of

insurgency”, such as the size of the country (population), mountainous terrain and “oil exporter” variable, actually have significant effects only on specific type of civil war. If all rebels were in fact opportunists and respond to the presence of the “right conditions of insurgency” in an identical manner, there would not be such differences between types of civil war.

Furthermore, if men rebel simply because the right conditions of insurgency are there, I find it rather puzzling why vast majority of people have *not* rebelled against the state despite “right conditions”, especially in large countries such as Russia and Indonesia. I doubt that the rebellion of Chechnya, East Timor or Aceh was a mere product of random effect of “right conditions of insurgency”. If the effect of “right conditions of insurgency” is not random within the country, then it might be the case that the “right conditions of insurgency” *enable* the rebellion by the people who are motivated by other causes, but they do not affect so much the people who are not motivated by some other causes. If that is the case, it would mean that one should distinguish the fundamental causes of conflict that motivate potential rebels to take up arms and “conditions of insurgency” that enable the rebellion, both of which are necessary for the onset of civil war.

To my eyes, “grievances” remain a primary candidate of such “fundamental causes”. Even though many prominent scholars who conduct large-N studies on civil wars have already reached to the conclusion that “grievances do not matter,” I still suspect that this might be rather due to their research design, such as the

lack of distinction of types of civil war and inadequate operationalization of “grievances”. There seems to remain a room for further study before drawing a final conclusion based on the results of the existing literature.

Notes

- 1 This variable is coded 1 if rebels aim at centre, 3 if they aim at exit or autonomy and 2 if their aim is ambiguous, among the onset cases.
- 2 As for the rare-event logit, see King and Zeng [6]; King and Zeng [7]. Statistical software of RElogit is available from <http://gking.harvard.edu/>.
- 3 This variable is called “offrlang” in the Fearon-Laitin dataset, but is not included in the replication dataset available on the internet. See the footnote 4 below.
- 4 The variables related to language and religious discrimination constructed by Professor Fearon and Professor Laitin are not included in their replication dataset published on the internet, and are provided personally by them for this re-analysis. I would like to express my sincerest gratitude for their kindness and generosity. For the replication of this re-analysis in the present article, please contact Professor Fearon and Professor Laitin directly for the language and religious discrimination variables. Their contact information is available at their website: <http://www.stanford.edu/~jfearon/> and <http://www.stanford.edu/~dlaitin/>.
- 5 The interpretation of these results is not easy and straightforward, because this variable might be picking out countries with ethnic minorities that have mobilized enough to get some official recognition at the regional/local level, as professor Fearon has pointed out (in the personal communication with the author). If it is the case, this variable is not a proxy for the language discrimination but for the ethnic mobilization for some other reasons. In order to check this, one must investigate whether ethnic groups that have rebelled in these countries had got the official recognition of their language or not. The results of the investigation would be an interesting contribution for the academic discussion on whether the power-sharing between ethnic groups leads to the stability and peace in that country or to the escalation of the demands of minorities and further conflict.
- 6 These results hardly vary when the ambiguous cases are included in the dependent variable.
- 7 The effect of the oil variable actually remains marginally significant at 0.1 level when these two countries are dropped. When irrelevant independent variables (insignificant at 0.1 level) are dropped, however, the effect of the oil variable becomes statistically insignificant even at 0.1 level (p-value is 0.119).
- 8 Personal communication with the author, December 4, 2004.

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