One-Thirteenth of a Data Point Does Not a Generalization Make: A Response to Dulić*

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While Dulić’s article is a helpful assessment of the sources Rummel used, it fails as a test of his general democide estimation methods or as an evaluation of his democide estimate for Tito’s Yugoslavia. Aside from mistakenly generalizing from one regime to Rummel’s results for 218, Dulić ignores 39 years of Rummel’s estimates for 1949–87 to concentrate on his own estimation of what Rummel’s estimate would be for 1944–48 (12.8% of the period Rummel covered). Dulić loads his estimate with war killed and the greater democide of other factions for 1941–45, miscompares the total to the 1948 census, and thereby wrongly concludes that Rummel’s estimate for the full 1944–87 period is too high. But Dulić provides no comparative estimate of his own for the full period to show this.

Dulić’s (2004) assessment of my sources, and the addition of others, to the question of Yugoslavia’s democide are of critical help to comparative research on this horror, and I thank him for it.

However, it fails as an analysis of my comparative work on democide for a number of reasons.

(1) I estimated the democide of 218 regimes, 1900–87. Tito’s Yugoslavia was only one of them. Testing my estimate and method on 1 out of 218 ‘data points’ can say nothing about my estimates or methods in general.

(2) He does not do a critical analysis of the whole 1944–87 period for my estimate of Tito’s democide, but only 1944–48, 12.5% of the period I covered. Even then, he bases his conclusions on what he estimates my estimate would be if I had made it for those five years. He really has reduced his analysis to, say, a thirteenth of a ‘data point’, and that is what he thinks it might be.

(3) He claims my Tito democide estimate is ‘much too high to be taken lightly’ (p. 99). He provides no estimate of his own for comparison, a serious omission. Given the length of the article, and its detailed treatment of my sources and statistics, it’s like a lot of foreplay, but no action.

(4) His claim above is based on a comparison to the 1948 census of his estimate of my estimate of Tito’s democide, 1944–48; plus the 1.6 million murdered during the war 1941–45 by the Chetniks, Partisans, Croatians, and Nazi, Hungarian, and Italian occupiers; and plus the war dead. It is a mystery how he can conclude anything about my democide total for Tito, 1944–87, when he ignores 39 years of my estimates, and when his estimate of my estimate for 1941–48 is much less than half the total he compares to the census. Yet, his claim that the overall estimate for 1944–87 is too high is the basis for his overall conclusion.

(5) He misunderstands how I selected and used my sources. My basic approach was to find first the most probable highs and lows...
for an estimate, and I did this by including reasonable authoritative sources from the different sides to a conflict or democide. Thus, for a communist regime, I used communist sources, anti-communist sources, and others for or opposing a regime. The idea is that the estimates by those for a regime would create a low, and those against a high, and the true estimate would lie somewhere between. Then I tried to find a reasonably unbiased source to get the mid-estimate. When one was unavailable, I studied the context and estimates and made my own judgement. Usually, such a mid-estimate was closer to the low than the high.

It is surprising that Dulić ignored my lows and highs, but had he included them in his analysis, he would have found that the estimates he accepted fell between 6

(6) Dulić accepts the 1948 census as doubtlessly correct, and therefore says definitely, of one of my estimates of all killed by all factions or occupiers, ‘that over 3 million Yugoslavs cannot have perished’ (p. 96; italics added). This was a Tito regime census at the time that the regime was carrying out massive purges. I don’t trust it, therefore, but nonetheless, I used these censuses as comparatives, but not measuring sticks.

(7) Dulić writes,

had he taken the time to analyse the demographic changes in Yugoslavia, and compared his data to existing censuses, he would have realized that the numbers do not correspond with what ‘a hypothetical, reasonable analyst would arrive at in viewing all the available information’. (p. 99)

I don’t understand this. I included demographic data from the UN Demographic Yearbook, whose source is Yugoslavia, and presumably its censuses and intercensus estimates, for each of the years (1950, 1962–67), and 12 estimates from various sources for 1941. From these, I calculated the projected population for 1950 and 1960, and the population deficit for 1941–50, and 1941–65. Dulić should have paid more attention to what I did.

(8) On my disaggregation principle, he uses a statistical analysis to show its inadequacy, but for estimates of this kind taken from only available sources, which may overall be biased one way or another, and for which I generally did not use the mean or median to get my overall estimates, the conclusion of a statistical analysis is self-evident. The idea is simple. When taking estimates from partisans on the different sides involved, where even academics and experts may be partisan as well, the biased low estimate of one partisan may tend to be cancelled by the high estimate of an opposing one. This is all that I mean, and I do not imply that the result will be unbiased.

Incidentally, my assertion of a ‘tendency’ for opposing sources to cancel each other out is not contradicted by his one sample point, no more than tossing a coin one time and getting a head contradicts the tendency for the coin over many tosses to approach equality between the number of heads and tails.

Reference
