

Review comments on revised manuscript “**Global TEC prediction performance assessment of IRI-2016 model based on EOF decomposition**” by Li et al., 2019; submitted to *Annales Geophysicae*

The paper has been revised and the current version is significantly improved. However it still contains some aspects which are not clear and I suggest that the authors look at their suitability as detailed in the following comments:

1. Page 5, line 5; I still think that combining IRI TEC and GIM TEC thereafter decomposing a single data file should be re-looked at. Aren't the authors concerned that by doing this, they are removing the differences/similarities which they intend to study? Magnitude comparison is not a strong justification for combining these datasets. If they exhibit similarities/differences, they will manifest or not show in trends and identified physical features. Therefore this reviewer thinks that IRI TEC and GIM TEC should be decomposed separately.
2. Page 8, section 3.2: This is related to the previous comment. It would have been more straight forward to decompose IRI TEC and GIM TEC separately. There will then be two different figures of Figure 4. This is when the features in Figure 5 can be independently compared. Otherwise, it appears that Figure 5 is generated using values plotted in Figure 4 which were a result of IRI TEC and GIM TEC combination. Please consider re-looking at this. Once this is done, the rest of the figures may slightly change, and perhaps the physical features may remain.
Otherwise, provide a strong justification for combining these datasets not in terms of magnitude. If one was complimenting the other in a different problem, then combining them would perhaps work; but you would need to state the errors associated with these datasets. In the current problem, you are comparing the two datasets and combining them appears to be defeating the intention of the problem.
3. I find the newly added Figure 2 useful. However the physical features are not interpreted. Are these seasonal differences at different latitudes expected? Are there no references in literature to support your observations?

On page 6, in the statement “The IRI-2016 model provides ionospheric parameters of up to 2000 km and will inaccurately predict the TEC up to GNSS satellites located at an altitude of approximately 20,000 km. The IRI-TEC may be smaller than GIM-TEC because of the missing plasmaspheric content”

I suggest changing the words “... and will inaccurately predict ...” to “... is expected to be lower than ...” This is because there have been cases where IRI TEC is greater than GIM TEC; and there is sufficient literature showing this.

On page 15 in the statement relating IRI TEC and GIM TEC in terms of A_{11} ; am not sure that associated coefficients, in this case A_{11} can be sufficient to explain the magnitude differences between IRI and GIM TEC. Please cross-check and correct if necessary.