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Interactive comment on "Comparison of quite time ionospheric total electron content from IRI-2016 model and GPS observations" *by* Mulugeta Melaku and Gizaw Mengistu Tsidu

Anonymous Referee #2

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REVIEW OF THE ARTICLE Comparison of quite time ionospheric total electron content from IRI-2016 model and GPS observations Mulugeta Melaku and Gizaw Mengistu Tsidu

The article considers the real problem, namely the validity of the IRI-2016 ionosphere model, which has become the most popular model up to date. The subject is suitable for the Annales Geophysicae. However, I recommend the major revision and resubmission in order to improve the results. The following shortcomings were found:

1. In the abstract and in the first chapter the authors stated the requirement of the ionosphere for the correct forecast of the radio wave propagation. However, the

altitude range covered by IRI is suitable only for HF-UHF forecast. Moreover, TEC allows to predict only integral attenuation, as far as no profile properties can be derived from TEC. 2. The motivation for the article is not clearly stated. IRI has been verified intensively for years, using ionozondes, satellites and GPS receivers as well. What is the novelty of the work? What is the hypothesis to be checked using GPS observations and IRI calculation? Please state it clearly! 3. When the authors deal with 5x5 gridded TEC values they do not work with evidence. Instead they work with the results of an IGS computer model (some kind of Kalman filter and gridding technique). Thus, the title becomes wrong – you compare one model with another model. If they want to validate IRI model then exactly 422 sites must be used, with further gridding and mapping if necessary. 4. It is not clear, whether the authors used IRITEC subroutine, or they calculated vertical electron profiles and integrated them manually. 5. The monthly basis can suffer from biases. It is obvious to use 27 days periods corresponding to Bartels rotation cycles. 6. Short remark about (4). The correlation coefficient (4) makes sense only for stationary processes and for the processes that have normal distribution. No tests are presented that prove the aforementioned requirements. If they are violated then the results have no sense. 7. Figures 2 and 3 in Mercator projection are awful and unreadable. I see the authors want to prove that they have calculated everything declared. But at that scale it is impossible to make difference between Canada and the US. It is much better to choose a couple of the most interesting frames and print them at large scale. For high latitudes the orthogonal polar projection must be chosen. 8. The style of the presentation in the article can be accepted only if the authors used real F10.7 and Kp indices (or IG index) from the database. But even in this case I recommend to improve the results in the following way: a. Use only 422 sites with GPS data b. Use estimations of TEC, namely if error is larger than 20% of TEC the data must be discarded c. Present the results as a function of Solar Zenith Angle and Magnetic Local Time. That will be compact and informative, and there will be no necessity to plot tens of filled contours. I think that all simulated data have been stored thus it won't take a lot of time to reduce and remap

the results.

Please also note the supplement to this comment:
https://www.ann-geophys-discuss.net/angeo-2019-44/angeo-2019-44-RC2-supplement.pdf

Interactive comment on Ann. Geophys. Discuss., https://doi.org/10.5194/angeo-2019-44, 2019.