**RESPONSE LETTER TO REVIEWER #2**

***We thank the reviewer for taking the time to review our manuscript and provide important comments and suggestions. Please, see below our responses:***

1) In this article, the authors perform an analysis of foF2, hmF2, and foE between measurements made by a digisonde in Santa Maria, Brazil, and the output of various IRI-2016 model sub-routines. The article provides a unique analysis on the performance and validation of IRI-2016 in the South American Magnetic Anomaly region. The authors use the relative deviation, an approach used in similar studies, and the Spearman correlation coefficient to provide a quantitative analysis of model performance. Based on the results, the authors give recommendations on which IRI-2016 sub-routines to use.

The data used in their analysis includes ionograms made during geomagnetically quiet times. The authors use the Kp index to identify periods of quiet time by making sure that the sum of the eight 3-hour Kp indices for the day is less than 24. For clarification, are the authors using the eight 3-hour Kp indices made prior to a given measurement, or is it just based on the eight 3-hour Kp indices for a given universal time day? If it is the latter case, it would seem that determining whether a measurement occurred during geomagnetically quiet periods would depend on Kp values for times occurring after the measurement. This may falsely categorize some measurements.
***Our response:*** *We used the eight 3-hours Kp made prior to the measurements.*

In their analysis of foF2, the authors find that both the CCIR and URSI sub-routines provide correlation coefficients of r = .97, and suggest that the user may use either sub-routine to model foF2 over the Santa Maria region. However, the sentence starting on line 210 suggests that users should use URSI over the oceans and CCIR over the continents. This statement seems too general and beyond the scope of the paper. The statement also seems to be contradicted by the findings of Batista and Abdu (2004) who found URSI outperformed CCIR over São Luís in the Brazilian sector. Also, the authors should make sure that the Batista and Abdu (2004) reference is including in their bibliography. I could not locate it.
***Our response:*** *We absolutely agree with the reviewer that the statement 'the users should use URSI over the oceans and CCIR over the continents' is too general and beyond the scope of the paper. Indeed, reading the paragraph repeatedly we see now that the last 3 sentences of the paragraph do not add relevant information and might confuse the readers. Therefore, we deleted these sentences in the new version of the manuscript. Regarding the results of Batista and Abdu (2004), we explained in lines 219-223 that over the Brazilian territory the right choice between CCIR and URSI in modeling foF2 depends on the location of the users (equatorial, midlatitudes). The reference Batista and Abdu (2004) was not included in the bibliography because of lack of attention. We added it in this last version of the manuscript.*

In the discussion section, the authors provide some comparison with results from previous studies. For instance, the finding that URSI and CCIR provide comparable results was also found to be true in Ezquer et al. (2008). Additionally, Zhao et al. (2017) also found that SHU outperformed BSE and AMTB in hmF2 predictions in the China region.

The authors find the highest correlation between SMK29 and IRI-2016 for foE values and suggest that a potential reason for this is because the E-region ionization is controlled primarily by solar radiation and IRI can predict this radiation fairly accurately around the globe. The author make it clear that this is simply a proposed explanation, however, this claim could be substantiated by

providing comparisons with other studies. To the authors knowledge, do previous studies exist showing the similarly high performance of IRI-2016 foE predictions for other parts of the globe?
***Our response:*** *As far as we know there is no similar study comparing NmE (or foE) obtained from IRI with Digisonde data. However, there has been a significant amount of studies about the E-region modelling, most of them based on photochemical approximation. We did not include these references in this work because they are beyond the scope of our paper.*

Below are some corrections to grammatical errors and suggestions for potential changes to the text to help improve readability:
***Our response:*** *We acknowledge the corrections/ suggestions given by the reviewer. We corrected them accordingly.*

Line 38: “measurements as the worldwide” -> “measurements such as the worldwide” *Done*
Line 41: “were released by the IRI Working Group since the 1970s in order to constantly revising the model to remain it up to date and accurate as possible” -> “have been released by the IRI Working Group since the 1970s in order to update the model to keep it as accurate as possible” *Done*
Line 52: “E heights” -> “E-region heights” *Done*
Line 77: “dip anlge” -> “dip angle” *Done*
Line 83: “dip = -49.8” -> “dip angle = -49.8” *Done*
Line111: “the comparison is made considering the currently three options for determining IRI-hmF2” ->” the comparison is made using the three currently available options for determining IRI-hmF2” *Done*
Line 114: “deduced of ionograms” -> “deduced from ionograms” *Done*
Line 213: “Santa Maria has located” -> “Santa Maria is located” *This sentence was deleted*
Line 224: “Despite the inclusion of two new model options for the hmF2 (AMTB and SHU) be an important update” – I don’t have a suggestion but this sentence should be reworded. *We have reworded*
Line 265: “it is recommended the users to use” -> “it is recommended that the users use” *Done*

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*Finally, we would like to thank again the Reviewer #2 for he/she assistance in evaluating the paper, and the comments for improvements.*