

Interactive comment on “Performance of the IRI-2016 at the Brazilian low-latitude ionosphere over the South America Magnetic Anomaly during solar minimum” by Juliano Moro et al.

Anonymous Referee #1

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Review comments on paper “Performance of the IRI-2016 at the Brazilian low-latitude ionosphere over the South America Magnetic Anomaly during solar minimum” by Moro et al., 2019

The short paper is devoted to validating the IRI 2016 model with ionosonde data collected over a single location, Santa Maria, in Brazil. Results are presented for three ionospheric parameters, foE, foF2 and hmF2. Studies of this nature are important in showing the performance of climatological models such as the IRI; and subsequently assist in their improvement. Data from Santa Maria will be very vital for the updating and improving the IRI model and I suggest that the authors/owners of this ionosonde

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make the data available to the scientific community. In this regard, I would like to suggest that the authors indicate where the data can be accessed or whether it is being sent to the GIRO database that collects data for all digisondes all over the world. Having said this, I think, the title should have been specific that the study is done over one location, aside from this, the reader may think that this is a regional analysis focusing on the entire Brazilian low-latitude.

The authors presented the performance of the IRI model using different hmF2 options and came up with a conclusion that the Shubin option estimates hmF2 better than the other options over SMK29. This is consistent with some studies carried out over other regions such as China.

Statistical analyses was based on correlation coefficients and relative deviation (RD). In their discussion, the authors did not indicate quantitatively how their values compare with other studies at similar latitudes. I understand that their location is near SAMA. However, they should have compared their statistical values to other studies in other low latitude regions. Also other previous studies may be using different statistical parameters such as root mean square error which is simple to calculate for the sake of benchmarking the authors results with existing studies.

In the entire paper, there are a number of language usage errors that should be corrected.

Below are some comments that may be helpful:

1. The authors used the quiet time data in the analysis and the criterion used was “summation of Kp values less or equal to 24” . I wondered whether this had some references. Otherwise, wouldn't it be straightforward to for-example simply use $K_p \leq 2$?
2. In line 75, there is a spelling error in “angle=-37 degrees”
3. Line 140, the highest values during day-time hours between 13:00 UT -22:00 UT. According to the authors statement in the paper, this corresponds to 10:00 LT – 19:00 LT. It appears that the high values at 19:00 LT may be related to pre-reversal enhancement, and not necessarily

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to day-time hours? Please cross-check and correct where necessary. 4. In line 170 and elsewhere in the paper: The authors state “There is also pronounced hmF2 values between 300 km and 320 km ...”. I think this should be reworded. “pronounced” in what context? 5. Subsections 3.1 and 3.2 can be combined and discussed at once as they deal with F2-region parameters. This will eliminate some repetitions in the narrative. 6. There seems to be a mistake in “Years” in Table 1. The Years indicated are 2016 and 2017; and yet the authors state that their study was conducted during 2017-2018?

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