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Interactive comment on "Improvements to Predictions of the Ionospheric Annual Anomaly by the International Reference Ionosphere Model" *by* Steven Brown et al.

Anonymous Referee #1

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This study uses a newly conceived method of calculating hemispheric adjustments to the IRI URSI foF2 maps to investigate the model's capacity to represent the Annual Anomaly. The authors illustrate that the IRI performs better at reproducing this anomaly when using hemispheric indices versus using a single ionospheric adjustment index (IG). I have a few concerns regarding the concept of this study. The method used here to improve the IRI is essentially a two-pixel assimilation scheme (one for each hemisphere). It is hardly a question that such a scheme would improve the model's capacity to represent an anomaly that depends on the differential behaviour of the two hemispheres, especially when the stations you assimilated (i.e. used to develop IGNS) are the same ones being used to evaluate this phenomenon. The authors argue

that the improved performance through the use of separate hemispheric indices over a sole global index has physical connotations and can be related to differential solar activity scaling between the hemispheres. This conclusion, however, relies on the assumption that the errors in the URSI foF2 maps are related to physical behaviour and not the result of simply issues in the original fitting of the URSI foF2 maps, which were plagued by hemispherically asymmetric datasets, resulting in more artificial data being used in the Southern Hemisphere. The issue of data coverage and breadth has always been a barrier to the use of the IRI for teasing out physical conclusions and I believe that remains the case with this study. Regardless, in any assimilation-like scheme, increased flexibility in the basis set (i.e. allowing for two indices rather than one) would, by construction, improve performance with respect to the fitting dataset and thus improve the model's ability to represent the Annual Anomaly. Until the cause of the IRI's original AI underestimation is properly identified, drawing physical interpretations is largely conjecture.

In addition to the above I also have concerns regarding the ultimate utility of these hemispherical adjustments to the model:

1) Is this considered a potential replacement to the current methodology in the IRI? Using a hemispheric adjustment would likely create significant artificial gradients at the boundary between the hemispheres, making this approach unusable for many applications that rely on the IRI, such as trans-equatorial HF propagation modeling. 2) What is the advantage of this method over already available products, such as IRTAM, the real-time IRI? The IRTAM is presumably a far more robust assimilation scheme and is readily available at the moment. Where does your method fit? 3) One of the features of the current IG12 index is its stability to be forecasted. How would you implement this methodology in the IRI to retain the forecast capability?

I realize that some of these questions are more pertinent to the author's previous publication, but they are nonetheless relevant to this application of the method.

Based on the above concern regarding the legitimacy of the authors' physical interpretation of empirical model behaviour, I recommend that this study be rejected. There are valuable components of this study that may be worth publication; however, the main thesis of this document is far from convincing without significant additional investigation; in fact, the main point of the document may be better illustrated without the use of the IRI and instead solely focusing on observations.

Other Major Comments:

1) Figure 1 – Do you have copyright approval to reproduce this figure? If so, please state so and list the publisher. 2) Figure 2 – There are significant holes in the station distribution. Perhaps comment on how this may affect your results. 3) Table 4 – Do you have statistical error information for the values in this table? Are these statistically significant? 4) Figure 4 – Can you comment on the differences between your top left figure here and that from Rishbeth and Muller-Wodarg (2006)? There appear to be differences in the ionosonde-derived AI values for the same pairs. Were you unable to acquire the same ionosonde data or are there processing differences between your study and theirs? This would also somewhat highlight the need to have some sort of error measure associated with AI values. 5) Regarding the incompatibility between the IG from CCIR and using it for the URSI maps – Why would you not just recalculate a monthly IG index for the URSI maps, as you have done for the IGNS index? This way you would be able to definitively define where the errors are coming from.

Minor Comments:

Page 2, Line 15 – Please cite your previous paper regarding the method of determining the IGNS here.

Page 5, Line 1 – "utilized" -> "using" or "utilizing"

Page 6, Last Line – AP -> Are you referring to Ap, the daily arithmetic mean of the three hourly ap index, or are you referring to the three hour ap index itself. A bit semantic

being a capitalization difference, but these two indices are often confused.

Table 8 – Low SA -> move to right.

Page 18, Line 10-12 -> Perhaps mention the differential behaviour of the north and south PC indices here as well?

Page 19, Line 16 – GRO -> GIRO

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