

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

Anonymous Referee #2

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The manuscript attempts to improve the annual anomaly representation in the most acknowledged empirical model of ionosphere, IRI. Although the goal is very important, the study is flawed due to various mathematical and notational errors. The paper is also full of grammatical and syntax errors. Here is a list of major problems with the paper: 1) The ‘prediction’ is a mathematical operation where future values of a discrete-time signal are estimated as a function of previous samples. Here, the study is retrospective and deterministic in the sense that a preset algorithm with defined coefficient set is run with different index values. Thus, this operation cannot be considered as ‘prediction’. Therefore, the title and the wording in the text should be modified. 2) The definition of AI and the explanation given under the equation 1 are highly problematic. The mean

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M is defined to be the sum of two numbers which is wrong. The equation should be corrected to reflect the proper implementation. 3) The implementation of ‘AI’ is not clear. The explanation mentions that ‘The AI is computed by using an average of the NmF2 from both Northern and Southern hemisphere which have similar geomagnetic latitudes ...’ Yet, equation 1 does not indicate any averaging over the stations in both hemispheres. Further in the text, AI is applied to station pairs during daylight hours and for the months of January and July for a set of years. This operation is not reflected in equation 1. The definition of AI should be given properly to reflect the full intent and computation. In its present state, it cannot be accepted. 4) The explanation for the interpretation of the value of AI and the example given are also wrong. 5) The information on the background literature is not given properly. Figure 1 which is taken from another paper, may have copyright issues. It is not clear how the AI index is applied to station pairs and what those legends on the subplots mean. The authors clearly mention that the application in the Rishbeth and Muller-Wodarg (2006) paper did not provide any satisfactory explanation to their results and it has ‘reliability’ issues, yet they adopted their line of computation of AI index between the station pairs! This is a contradiction in itself. 6) In the official site of IRI, irimodel.org, IG index is mentioned to be an ionospheric index not a ‘solar cycle input’. The authors should clearly define what they mean by solar cycle input. 7) Apparently, the IRI model utilizes a set of coefficients and index values in the computation of NmF2 and foF2 for a user defined date, hour and location. The model uses IG12 from IGRZ.dat file. Since the model aims to produce hourly monthly medians, 12 month running median of IG is automatically input from the data files. If the user wishes to update the value, he or she can input it separately at the time of run. How did the authors prepare the index set for IG and IGNS? Since they are not available in a format that can be input automatically in the online version of IRI-2016, did the authors run the model offline in the Fortran version? 8) According to the information given in the introduction section, IGNS is developed using 50 ionosonde station so it is an ionospheric index more than a solar cycle input. In Figure 2, there is a map of the world with black dots indicating the ionosonde stations

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used in the study (and ionosonde is misspelt!). Then, there is Table 2 which lists a set of stations that are used in the study. Most of the stations indicated on the map are not listed in the Table and some stations such as Eglin, Florida and Huancayo, Peru are not on the map! The pairing of the stations are also flawed. The stations are paired according to not only north-south hemispheres but also east-west hemispheres! The geomagnetic coordinates are not taken into account and station in Virginia, USA is paired with another station in Tasmania. If the pairing is necessary, at least magnetic conjugates and local daylight hours should be considered. For example, the stations in Japan can be matched with those in Australia and New Zealand. The stations in Europe can be matched with those in South Africa. If the authors have some other mechanism in mind, they have to explain this in a better way. Otherwise, this kind of station pairing does not make sense at all. Taking the 'mean' of latitude of two stations in two different hemispheres do not make any sense either mathematically or physically. 9) The application of AI to a station pair, for daylight hours, for the months of January and July and for a set of years and 'averaging' should be clearly given in a mathematical equation with proper notation. 10) The authors should know by now that the units are never written in a mathematical equation. The unit of NmF2 is not 1/metercube but el/metercube. The unit of frequency is indicated by Hz not hz. The units are never written in italics. There should always be one blank space between the number and the unit. The asterisk is not a proper mathematical notation for multiplication. 11) Equation 3 does not represent the parameters of the application. For one ionosonde station that computes foF2 every 15 minutes, how can there be one value for whole month of January or July? 12) Figure 3 is also very unclear. The horizontal line for 80 is missing. The main problem is that the plot is prepared for the 12 month running mean of IGNS not IGNS itself. Which input data file did the authors use in this study? The years chosen for various levels of solar activity are given in Table 3. According to these information, the study covers the years from 1970 to 2014, yet in the rest of the paper, the results are provided for 1970 to 1990 (such as Figure 4) or the years are not mentioned at all. For a list of 8 years for low activity years,

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there is only one value in the tables. What happened to the data? The criterion for less than 10 percent is not clear. 13) In Tables 4, 5 and 6, there are two stations and one 'lono' value. How is this possible? How did the authors compute the values? The caption of the titles mention that the values are AI, yet the column titles indicate that they are IRI(IG) or lono. The 'average' and 'average*' operations are not clear. Why did the authors include station pairs with no data into the tables? 14) In Table 6, the conclusions drawn are wrong. For 7 station pairs that have data, IG input matched 4 of these, whereas IGNS matched only 2. So IG is a better input for high solar activity years. 15) Figure 4 does not include years from 1990 to 2014. It is not clear why all the stations are not given? What is the meaning of diamonds? Some lines have them and some does not. If they are the years of computation, this information does not match Table 3. The subplots are too crowded. 16) The information on the computation of AI on page 12 is not clear at all. All mathematical computations should be clearly indicated with proper notation and equation numbers. 17) I have reservations for the 'missing data replacement' as done in the manuscript. 18) Table 7 does not make sense at all. The correlation coefficients are computed for which data sets? What is NPTS mean? Is the correlation biased or unbiased? For what years and for which solar activity level? 19) Figure 5 is also another mystery. Even the labels are misspelt. What does the vertical bars or lines represent? And so on... 20) Table 8 is wrong. 21) Since the equations for computation of AI, averaging of AI and SfoF2 are totally unclear and may possibly contain significant physical and mathematical errors, none of the comments in the discussion or the drawn conclusions are reliable. 22) The paper is full of notational, grammatical and mathematical errors. The authors should start using at least a spell-checker and technical reviewer to edit the paper. There are syntax errors and singular-plural errors. There are many incidences of two verbs are used in the same sentence. 23) The authors should stop 'suspecting'. 'Suspect' is not part of scientific terminology. 24) There is no references for IGRF model and some of the data sources are not acknowledged.

The paper cannot be accepted in its present form.

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